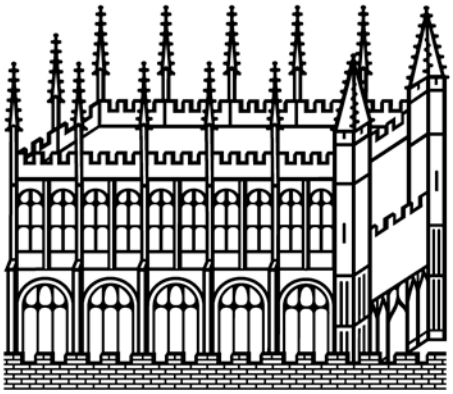

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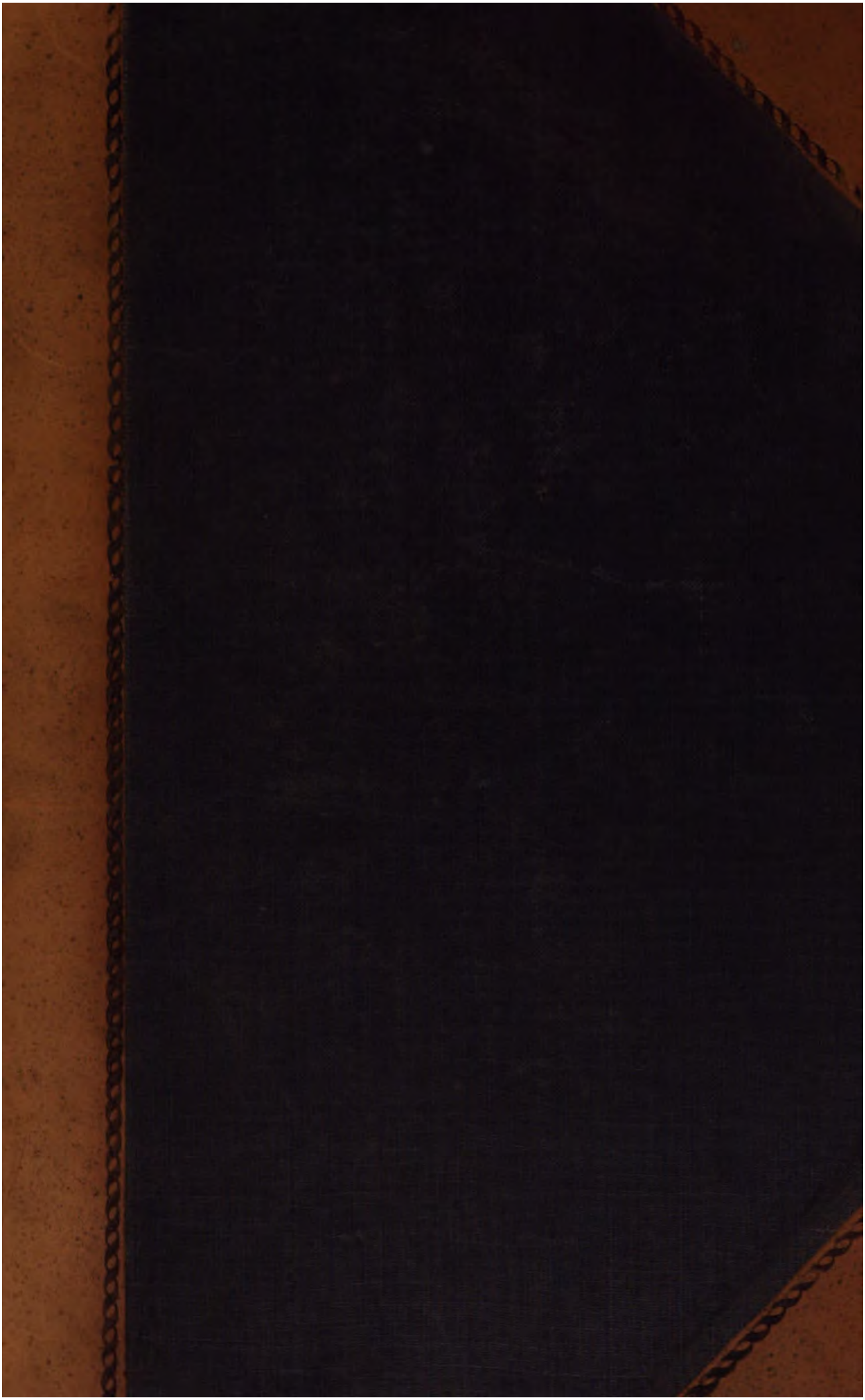

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OF

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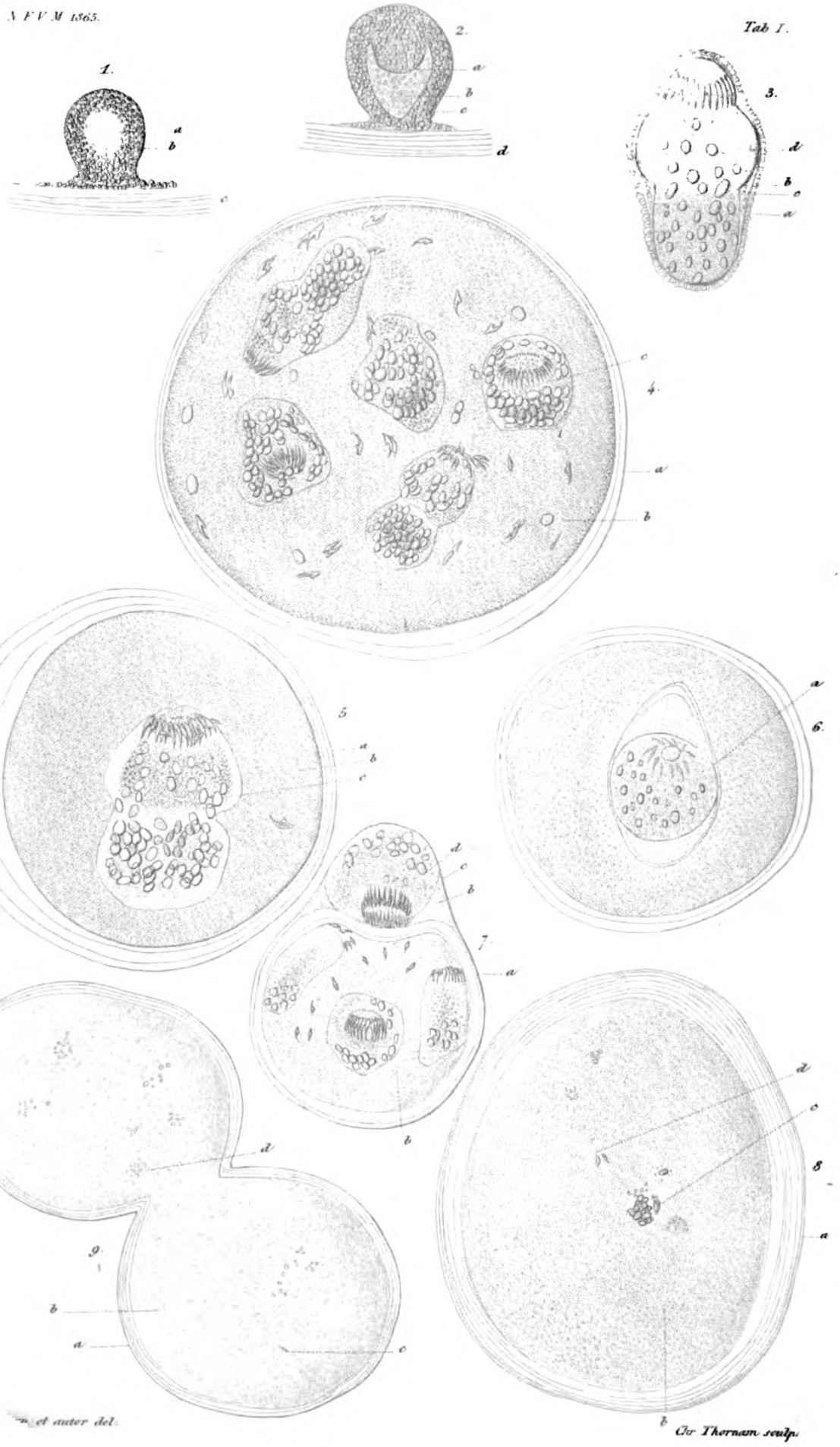

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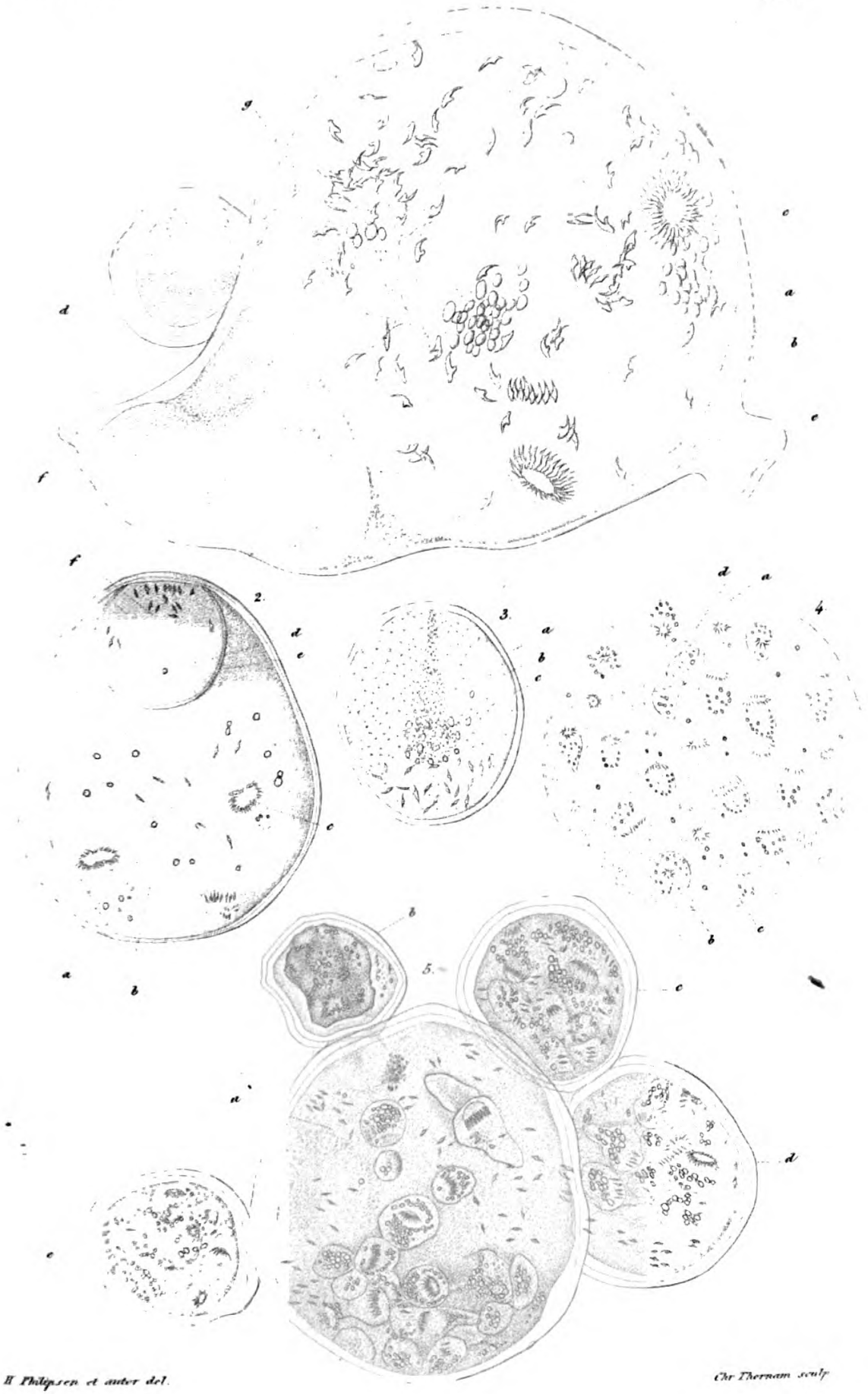

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1867.

PART FIRST.

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Review I.

1. *Untersuchungen über die Lymph-drüsen des Menschen und der Säuge-thiere.* Von H. Frey. *Leipzig,* 4to, mit 3 Taf., 1861.

Researches upon the Lymphatic Glands of Man and Mammals. By H. Frey.

2. *Das Saugader-system vom Anatomische Standpunkte bearbeitet.* Von L. Teichmann. *Leipzig,* 4to, mit 18 Taf., 1861.

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3. *Neue Beobachtungen über die Struktur pathologisch veränderter Lymphdrüsen.* Von Thos. Billroth. ('Archiv für Patholog. Anatomie und Physiologie,' Bd. xxi, p. 423.)

New Observations on the structure of pathologically altered Lymphatic Glands. By Thos. Billroth.

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9. *Ueber den feineren Ban der Lymphgefässe.* L. Auerbach, in 'Breslauer Zeitung,' 1865.
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10. *Zur Histologie der Lymphdrüsen.* Von Kowalewsky. ('Sitzungs berichte der Kaiserlichen Akad. der Wiss. zu Wien,' Band xlix, p. 455.)
On the Histology of the Lymph Glands. By N. Kowalewsky.

11. *Manual of Human Histology.* By Kölliker. 1863.

12. *Zur Chemie der Lymphe.* Von C. Dähnhardt. (Virchow's 'Archiv,' 1866, Band xxxvii, p. 55.)
On the Chemistry of the Lymph. By C. Dähnhardt.

13. *Ueber die Lymphgefässe des Herzens.* Von Prof. Eberth, in Zurich, und Dr. Alexander Belajeff, in Petersburgh. (In ibid., p. 124.)
On the Lymphatics of the Heart. By Prof. Eberth and Dr. Alexander Belajeff.

As the above list of works testifies, considerable attention has recently been bestowed on the lymphatic system; yet, notwithstanding the manifold inquiries and observations that have been made by so many experimenters, there is probably scarcely any subject in anatomy or physiology which presents more interesting facts for consideration, or will even now better repay a minute investigation by a painstaking and well-qualified observer. An additional interest attaches to the study of the lymphatic system at the present time, from the gradually increasing employment of hypodermic injections in the treatment of neuralgic and other affections. As in such cases there is good reason for supposing that the active agents are taken up by the lymphatics to a great if not to a greater extent than by the

blood-vessels, the conditions both structural and physiological which lead to such remarkably energetic and rapid effects, and which are so striking when compared with the ordinary mode of administration of medicines, are well worthy of careful inquiry.

From the results of such a comparison, and from the universally admitted uncertainty of the action of medicines, the conclusion appears unavoidable, either that a considerable proportion of the active principles contained in the usual drugs employed in medicine pass through the alimentary canal without undergoing absorption at all—as is well known to be the case with woorara, which fails to poison unless taken into the stomach in large quantities—or are decomposed by the various fluids with which they come into contact in their passage; whereas when such substances are subcutaneously injected, the extraordinarily augmented energy which they exhibit is to be attributed to the circumstance of their entering the lymphatic and vascular systems in an unaltered state, and, being rapidly conveyed by the current of fluid contained in both sets of vessels to the most remote parts of the system, are free to exert their peculiar powers on those for which they have the greatest affinity, whether nerve or muscle or gland.

The simple yet instructive experiments of Professor Graham, which appear to be within the reach of every one to repeat and verify for himself, but which where scientific accuracy is required really demand great pains and care for the elimination of disturbing influences, have opened out new views of the phenomena of osmosis, and have resulted in the establishment of certain laws which have already begun to exercise a powerful influence upon our theories of absorption, and have enabled physiologists to offer a probable solution of some of the most curious problems connected with digestion. Into these it is not at present our purpose to enter. We are desirous of placing before our readers a succinct description of the advances that have been made in the knowledge of the minute anatomy of the lymphatic system and of some of its more remarkable appendages, believing that here, as elsewhere, accurate knowledge of the structure of an organ should precede or at least accompany any attempt to discover its function.

From the most cursory examination, it is obvious that the two sets of vessels termed respectively the lacteals and the lymphatics are but parts of one and the same system: their structure is almost identical; the fluids they contain are, except for a certain period after the ingestion of food, essentially similar in physical and chemical qualities, and are mingled in one common receptacle before being discharged into the vena innominata; and,

indeed, the only important difference between them appears to be that, whilst the former is destined to absorb certain constituents of the food from the alimentary canal, and especially those of an oleaginous nature, the latter constitutes the means whereby the surplus nutritive material which has passed through the walls of the vessels, and has ministered to the growth and maintenance of the tissues, is again taken up and conveyed to the blood. In both instances the fluid is compelled to pass through a series of glands, where such changes are effected in it as in all probability render it more fit to enter the blood, and aid in its assimilation to that fluid, both morphologically by the production of corpuscles, and chemically by the development of fibrin or of those substances which form fibrin.

To enable us advantageously to discuss the several points of interest which have been recently worked out, it will be advisable to consider them in a definite order, and to examine, first, the mode of origin of the lymphatics and lacteals; secondly, the structure of the lymphatic glands, and of those structures which are so nearly related to them in a morphological point of view, namely, Peyer's glands; and, finally, to follow the course pursued by the lymph in its passage through these organs.

In the first place, then, let us ask, How do the lymphatics commence? Are they continuous, as was at one time supposed, with the blood-vessels through a system of exquisitely delicate capillary vessels, the so-called vasa serosa, the calibre of which is so fine as effectually to prevent the entrance of blood-corpuscles? or do they begin by a plexus of freely anastomosing capillaries totally unconnected with the vascular system? or, as has been recently sustained, do they take origin in the irregular and only partially enclosed cells and lacunæ of connective or areolar tissue? or, lastly, may they be traced into the hollow corpuscles and caudate processes found in this same connective tissue with which the lymphatic vessels are without doubt so constantly and so curiously connected?

In examining the statements made by different observers in support of some of these various theories, it may be remarked that, with regard to that which supposes the direct continuity of the blood-vessels with the lymphatics by means of extremely fine channels, there appears to be no evidence in its favour; and although those who are conversant with diseases of the eye will readily acknowledge, from the phenomena of conjunctivitis and keratitis, the possible presence of vessels so small that whilst in their ordinary state they may only allow of the passage of the liquor sanguinis, yet there can be no doubt that such minute vessels, though when relaxed by inflammation they may admit the entrance of blood-discs, do not convey them into the

lymphatic system, but invariably, after a shorter or longer course, discharge them again into the veins, with which they are continuous. The mode of origin of the lymphatics which is maintained by Teichmann, one of the most industrious and skilful of modern injectors, is that they commence in stellate cells, the caudate prolongations of which, intercommunicating with one another, form a kind of irregular network, and by their coalescence gradually unite to larger and larger tubes, which soon acquire valves in their interior. He expressly states, that in all instances the vessels are bounded by proper walls, and constitute a completely closed system of tubes, entering into communication with the blood-vessels at their central termination only, that is to say, at the point of entry of the thoracic duct into the superior vena cava. The most recent observations that have been made, those of Professor Eberth and Dr. Belajeff, on the origin of the lymphatics of the heart, support Teichmann's views, as they particularly note that the lymphatics commence in a network of tolerably wide tubes with small slit-like interspaces.

The views of Kölliker do not appear to differ essentially from these statements, except in one point. In the last edition of his 'Manual of Human Histology,' he agrees with Teichmann in holding that the lymphatics in the tail of the tadpole are minute vessels possessing proper walls, which unite to form first small lacunæ, evidently corresponding to Teichmann's stellate cells, and then the upper and lower caudal lymphatic canals; but he differs from the last-named author in considering that they arise by delicate rootlets *with closed free* extremities, and not by a plexus: and he believes that the same mode of origin may be observed in the mucous membrane of the human trachea, though he admits that the observations of Virchow have thrown some doubt upon the latter point.[1] The most interesting observations on the mode of origin of the lymphatics are, however, undoubtedly those comparatively recently published by Ludwig, Tomsa, and Noll, which, if supported by other testimony, will enable us to clear up many points that are now involved in great obscurity.

We may premise, that one of the chief difficulties of the ordinary view of the relations of the finest lymphatics, in which they are regarded as a closed system of capillary tubes lying side by side with the capillary blood-vessels, is that if we admit the lymph to be the surplus material which has exuded from the blood-

[1] Auerbach, grounding his opinion upon the appearances presented by injections of nitrate of silver, which stain the edges of the cells of a dark colour, appears to think that the walls of the finest lymphatics are formed exclusively of a delicate layer of epithelial cells, not supported upon any special membrane nor upon any modification of connective tissue; a statement which is corroborated by Eberth and Belajeff.

vessels and has been taken up by the lymphatics, it is necessary to grant, in addition, that the nutritive fluids, on escaping from the walls of the capillary blood-vessels, must occupy, together with the proper tissue of the part, such as muscle, or nerve, or gland substance, an intermediate space between the blood-vessels and the lymphatics into which they are about to enter. The passage of the lymph through the walls of the blood-vessels from within outwards may readily be attributed to the pressure exerted by the heart on the blood itself; but the question arises, what occasions the transudation of the lymph through the walls of the lymphatics from without inwards? why should not the exuded fluid rather travel along the interspaces between the blood-vessels, the proper tissue of the part, and the lymphatics? And to this objection, so far as we know, no explanation has been offered beyond the suggestion that it may be the office of the cells lining the lymphatics to take up such fluid in their normal process of growth, and discharge it when mature into the interior of the lymphatic tubes.

With the view of determining the question, Noll and Tomsa, under the direction of Ludwig, have carefully examined the origin of the lymphatics in the tunica albuginea of the testis and the sclerotic coat of the eye; and the result of their inquiries is, that, as Brücke, and, long before Brücke, Fohmann and Arnold, had previously stated, the extravascular spaces are themselves the origin of the lymphatic vessels: that these, in fact, arise from the splits and lacunar spaces and fissures of true connective tissue which invariably accompanies the finest lymphatic vessels; that, at first irregular in the extreme in their form and size, and possessing of course no proper walls, they are gradually confined and enclosed by condensation of the areolar tissue, until at length they begin to assume the form of vessels and acquire their proper coats. It is quite obvious, on this theory, that the original propulsive force of the heart may occasion the transudation of the nutritive fluids, which, percolating through the irregular fissures of the connective tissue, may at length, by coursing through larger and larger cavities, and from thence into vessels with better defined walls and supplied with valves, form a current of considerable rapidity, the moving power being, however, primarily derived from the heart, though all the while there may be no direct communication at all with the blood-vessels.

MM. Noll and Tomsa drew particular attention to the fact that the lymphatic spaces or lacunæ in the testis must intervene between the seminiferous tubes and the walls of the capillaries. But both these walls may be shown to be single and not double in construction; and hence it follows that the spaces

through which the lymphatic fluid passes possess no proper walls of their own, but are merely the interspaces between the other textures. Sometimes the passage from lymph-vessels with proper and well-defined continuous walls into the laminar splits of the connective tissue is quite sudden, the vessels losing their valves, muscular fibres, and epithelium; whilst in other instances there exists a series of imperfectly formed vessels, which they term lymphatic channels, in which the walls are formed only of loose connective tissue, but in which the cylindrical form is still preserved.

Speaking generally, the observations of Recklinghausen tend to confirm those of Tomsa and Noll, since he considers that the finest lymph-vessels stand in determinate relationship with the connective-tissue corpuscles, yet not in the mode formerly suggested by Leydig—namely, by the continuity of their cavities—but by the lymph-vessels ending in a network of intercommunicating spaces which he believes to surround and enclose the connective-tissue corpuscles. The existence of these investing spaces, however, is more than doubtful, as no other observer has seen them. The views of Noll and Tomsa are further supported by the high authority of M. Milne-Edwards, who, in his 'Leçons sur la Physiologie,' remarks that in the vertebrated division of the animal kingdom, the tendency to the division of labour, which is well marked in proportion as we ascend from the lower groups of animal life to the higher, results in the formation of three perfectly distinct systems—namely, a digestive system, a circulatory system, and a general lacunary system, the last subsequently differentiating itself, on the one hand, into the visceral cavities lined by serous membranes, and, on the other, into the inter-organic areolæ or cavities of the connective tissue. This last (he continues), by an organogenic process analogous to that which effects the formation of a circulatory system in the mollusks and annelids, tends to divide into two parts, becoming more and more distinct from one another—to wit, the lymphatic apparatus and the areolar system of the connective tissue. In conformity with these views, M. Edwards is of opinion that in the reptiles (frog) the lymphatics in the peripheral portion of their extent have no proper walls, and that the stellate vacuolæ described by Kölliker in the tail of the tadpole, with the passage of these into lymphatic canals, constitute, in reality, cavities analogous to the irregularly confluent vacuolæ of much larger size which are produced in the connective tissue, and which when pursuing another course of development form the ordinary serous cavities of the body. The lymphatics, then, essentially belong to the connective tissue, forming confluent lacunæ which are continuous

with vessels, the walls of which become more and more consolidated, and become lined with an epithelial investment.

Very few advances have recently been made in determining the mode in which the materials of the chyme or products of gastric digestion are absorbed in their passage along the small intestine, or of the mode of origin of the lymphatics, since the appearance of Teichmann's large work. There can be no question respecting the active part played by the epithelial cells covering the villi, but the exact nature and mode of that activity is still undetermined. We shall here only recapitulate the chief observations that have been made since 1853. In that year, the attention of microscopists was directed to the peculiar structure of these cells by Henle and Kölliker; and we find in the English translation of Kölliker's work on Histology, by Busk and Huxley, the cells described in these terms: "They constitute a simple layer of cylindrical cells slightly narrowed below, whose contents are nothing but fine granules, and an oval, clear, vesicular nucleus provided with one or two nucleoli. * * The addition of water to these cells produces a separation of the cell-contents from the broad end, giving rise in separate cells to the appearance of a membrane thickened upon one side, and in series of cells or entire villi to that of a peculiar structureless coat like the cuticle of plants. By its longer action, however, or by that of the intestinal fluids, the bursting of the cells produces apertures in them, or they become distended into large pyriform clear vesicles." The cause of these appearances, and the nature of this clear border, seam, or band, in the cells, soon became a fruitful source of discussion.

Brücke, in 1855, advanced the proposition that these epithelial cells were open at both their free and attached extremities, so as to constitute open funnels admitting the entrance of fat, the passage of which into the lacteals was only prevented by the soft tissue of the villi. This view was supported by the observations of Moleschott, who observed the entrance of various solid bodies into the interior of the cells, tending to show that there was no limiting membrane—but was vigorously contested by Donders and Funke, and also by Kölliker. The two latter observers in this year simultaneously published papers calling especial attention to the basal seam, and describing it as presenting a striated appearance occasioned by porous canals perforating perpendicularly the hyaline membrane closing the mouth of the cell.

Welcker, in 1856, stated decisively that the striæ of the band occluding the end of these cells were canals, and believed he was able to see in different aspects their apertures and lumina. Reichert, at the same time, drew attention to the presence of a similar seam in the cells of ciliated epithelium which had lost

their cilia, and V. Wittich suggested that the seam was an artificially produced appearance due only to the action of water. Funke examined the cells with great minuteness, and described the seam as being a hyaline thickening of the free border or edge of the epithelium, the opening of the cell being closed by a delicate membrane, and the striæ noticed by Kölliker being exclusively limited to the seam, and not being visible in their occluding membrane. He was of opinion that the striæ could not be regarded as pores or canals, but were due to the breaking up of the hyaline border into a number of bristle-like or ciliaform processes; and the transparent bulla appearing after immersion in water he regarded as the thin occluding membrane, bulging from the central part of the base in consequence of the imbibition of water.

Brettauer and Steinach, in 1857, in a paper contained in Moleschott's 'Untersuchungen,' also arrived at the conclusion that the striæ were not the optical expressions of pores, but of rods, and believed that these were in more intimate connection with the contents of the cell than with the cell-wall. In guinea-pigs and rabbits they noticed that after food had been recently taken, the seam was small and pale, and often only indicated by a line, and that no striæ could then be distinguished. If such cells were treated with water, the seam itself altogether disappeared, and its place was taken by a clear projecting mass or bulla that seemed to issue from the mouth of the cell. They regarded the cells as being open at the free extremity, of the form of a port-wine glass, with clean-cut edges, and with the mouth closed only by a plug of mucus, which, in fact, was almost identical with the view originally held by Brücke.

The next important observations on this subject were those of Heidenhain, who, corroborating the statements of Brettauer and Steinach, that the thickened border of the cells was composed of little rods intimately connected with the contents of the cell, endeavoured to show that from the attached and pointed extremities of the several cells processes were given off which, dipping into the very substance of the villus, entered into direct communication with the connective-tissue corpuscles which formed its matrix, and which again were continuous with the lacteals, and thus a path was opened by which the passage of the oleaginous constituents of our food could be reasonably supposed to enter the lacteal system. Many observers, however, as Henle and Kölliker, were unable to distinguish these prolongations, and regarded them as the result of reagents and of manipulation. In 1859, Lambl, after devoting much time with a good microscope to its examination, regarded the basal seam of the cells in question as only a smooth homogeneous border, and

considered the appearance of the cilia or canals as illusory: the extremity of the cells he described as being closed by a cup-like membrane, the hollow of the cup being directed towards the attached extremity of the cell, so that the whole cell resembled an elastic, broad-mouthed, and smooth-edged sucker. In 1860, Wiegandt and Balogh supported the general statements of Heidenhain that the epithelial cells gave off processes which dipped into the matrix of the villi; and Wiegandt traced the passage of fat-molecules into these processes, which presented dilatations in their course, the identity of which, however, with connective-tissue corpuscles he held to be doubtful. Balogh noticed that the basal seam disappeared in fasting animals, but was again brought into view after the ingestion of fat. He, therefore, drew the conclusion that the striæ were not preformed structures, but were merely the passages by which, during the absorption of fat, the molecules had made their way through the outer portion of the cell-contents. The absorption of water did not occasion the formation of striæ, but, on the other hand, did not prevent their formation if oil were also present. Wiegandt saw the seam remain as a bridge extending between two cells when one had fallen out of its place. He regarded it as neither a thickening of the cell-wall, nor as a ring around the edge as Lambl stated, but the lateral view of a uniform substance closing the cell, and superimposed on the proper membrane of its wall; and this appears to be now held by Kölliker and Funke. The distance of the striæ from one another varied, and he was unable to distinguish separate and distinct rods or bristle-like processes, and the appearances presented seemed to him rather to resemble folds of the membrane than cilia.

The latest observations that have been made are those of Eberth and Dönitz in 1864. The former found conical epithelial cells to be exceptional. They were more frequently cylindrical, beneath which, though not occurring with sufficient regularity to form a layer, were certain cytoid or globular bodies, which were probably epithelial cells in course of development. Dönitz describes the molecules of oil found in the cells as so small that they appear only as a mist under high magnifying powers, and does not consider it requisite to admit the existence of cells open at their free extremity, or of porous canals in their basal membrane.

The drops of oil described by previous observers he regards as the results of a post-mortem change; and here the matter rests, awaiting further and still more searching inquiries.

As regards the origin of the lacteals in the villi, it appears to be generally admitted that they commence by

simple closed extremities, or by a plexus of two or three meshes; though it is at least conceivable that the finest injections may fail to enter the delicate processes of the connective-tissue corpuscles described by Heidenhain. From thence the tubes pass into a close plexus, beautifully depicted by Teichmann, seated at the base of the villi, and from thence into a large external plexus, the branches of which are supplied with valves.

In whatever way the lacteals and lymphatics arise, it is certain that they all pass towards and finally enter certain structures termed lymphatic glands, in which a process of assimilation occurs resulting in the formation of fibrin and of nuclei and cells, from the further development of which there seems to be now no doubt that the blood-corpuscles actually proceed.

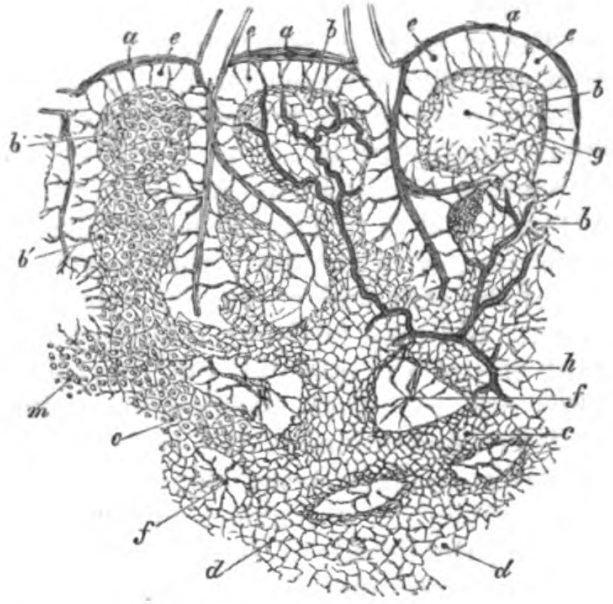
Respecting the structure of the lymphatic glands, two opinions have alternately held sway. By the earliest inquirers (Hewson, Mascagni) it was maintained that they were composed of a series of lymphatic vessels, arranged in the form of a convoluted plexus, and invested and made compact in form by a tunic of connective tissue. Hewson (Gulliver's edition, 1846) expresses himself in favour of this view in the most explicit terms. Thus he observes (p. 248), "If we inject a lymphatic gland with mercury, or inflate it with air, an irregular appearance is produced very much resembling cells; and if a gland prepared in this manner is dried and cut through, at first sight it looks like a honeycomb; but if we examine it more attentively, we shall find this cellular appearance evidently *made of convoluted vessels;* and in by far the greater part of lymphatic glands that we prepare, the subdivision of the lymphatic vessels into smaller and smaller branches, and not into cells, is apparent to the naked eye." And he proceeds to explain how the cellular appearance in the glands may be occasioned by the convolutions and serpentine windings of the vessels. This view has in recent times received strong support from Professor Goodsir and from Teichmann. Many other inquirers,—as, for instance, Malpighi, Morgagni, Boerhaave,—however, held that the afferent vessels of a gland either terminated at its periphery, or after plunging for a short distance into its interior, and that their contents were then discharged into a series of cells, or more correctly, perhaps, into cavernous spaces, freely intercommunicating with one another, and from which on the opposite side of the gland the efferent vessels took their origin. Neither of these views, however, is more than a rough approximation to the truth; and we are indebted for our present almost complete knowledge of these remarkable

structures essentially to the labours of three or four microscopists—to Billroth and His, who have worked to a considerable extent together, and to Frey and Teichmann. The descriptions furnished by these several writers, it is easy to see, are founded upon observation of similar appearances, with such differences only as it is natural to expect would occur in the detail of complicated textures when examined by different observers. Before presenting our readers with a summary of the principal facts ascertained by recent inquiries, it may be remarked that a satisfactory explanation of, as well as additional reasons for the slight discrepancies which we shall meet with in their several accounts is attributable to the circumstances—1, that a considerable variation exists in the development of the constituent parts of different glands in the same body; 2, that important changes occur in the glands with advancing age; and, 3, that in different animals some of the textures composing the gland attain a great degree of development, whilst in others the same parts are almost or altogether absent. In the first place, then, let us inquire what is the simplest form that may be assumed by a lymphatic gland. The reply is furnished by Teichmann in the following terms: The simplest or most rudimentary form of the lymphatic glands is to be found in certain convolutions and plexuses of vessels that may conveniently be termed retia mirabilia. In these, which are of inconsiderable size, one or more afferent or inferent vessels split up into several branches, which again divide and subdivide till they can only be followed by the microscope. The finer branches occasionally but not frequently anastomose, and are sometimes though rarely convoluted, and they are destitute of valves. By their reunion the vasa efferentia are formed. The plexus is usually flattened or slightly concavo-convex in form, varies in length from one sixth to one twelfth of an inch, and is invested by a sheath of connective tissue, containing a few fine elastic fibres, which dips into and occupies the narrow interspaces between the vessels supporting and accompanying the capillary blood-vessels, which also interlace but do not communicate with the lymphatics. These structures are comparatively rare, and have only been met with in the vicinity of the knee- and of the elbow-joints. In some instances, instead of being flattened, they assume the form of a rounded knot, or glomerulus: and here slight dilatations may be observed in the vessels at the points of anastomosis, the first indication of that saccular disposition which is subsequently carried to so great an extent. In other instances, and more extensively distributed through the body, collections of such simple retia mirabilia may be met with, the groups sometimes forming a flat and almost con-

tinuous layer, at others a more solid and rounded mass. In both forms the afferent vessels for the most part enter the convex surfaces of the retia, rarely penetrating their edges, whilst the efferent trunks take origin nearly perpendicularly from the inner or deep concave surface, and are very numerous, gradually uniting, however, to form one or two large trunks, which are always superior in size to the afferent vessels. With the increase in the number of capillaries, these compound retia acquire a greater firmness, thickness, and intricacy of arrangement. In all, however, the vessels preserve definiteness of form and outline, though they everywhere present the irregular contractions and dilatations characteristic of the lymphatic vessels generally. From such structures, observes Teichmann, the transition is easy to the simpler forms of true glands; for the construction of these bodies is essentially the same as that of the retia, with the important addition, however, that cells, the so-called lymph- and chyle-corpuscles, are developed, and collect in masses in the interior of the lymphatic vessels by which they are constituted; not, as we shall hereafter see, that such corpuscles are altogether absent in the peripherical portions of the lymphatic system—that is to say, in the stellate cells and anastomosing capillaries, or in the retia themselves—but that here the production of the cells and corpuscles attains an extraordinary development, and may fairly be considered as one of the most important if not the essential function of the organs in question. Thus we see that Teichmann fully adopts the tubular view of the formation of the lymphatic glands. His investigations seem to have been chiefly made by means of injections which, though a valuable and indeed indispensable mode of displaying the details of structure, require to be supplemented by other methods; and these appear to have been most carefully pursued by His and Frey, whose chief results we shall now detail.

The observations of His were principally made on the large inguinal and axillary glands of oxen, which were removed immediately after the death of the animal, and were hardened by immersion in alcohol or bichromate of potash. Thin sections were then made, and after being place on a slide, were carefully pencilled with a camel-hair brush, with the object of removing the numerous lymph-corpuscles occupying the interstices of the meshes of the gland, and which, if not detached, seriously interfere with the transparency of the section. According to the description of these writers, speaking generally, lymphatic glands form oval or bean-shaped bodies, receiving on their convex surface one or more afferent vessels, and presenting on the other a slight depression or hollow—the hilus—from which the efferent

vessels and veins emerge, and at which the larger arterial branches enter. The vessels lie imbedded in much connective tissue, termed the *hilus stroma*. On making a horizontal section of the gland, it is found that besides the general investing sheath of connective tissue, an external or cortical portion of the proper glandular substance can be distinguished from an internal medullary portion, which again is more or less hollowed out towards the hilus, the cavity being filled with a quantity of connective tissue, which is a continuation of the hilus stroma. The cortical substance is usually of a lighter colour, in consequence of being less vascular than the medullary; is soft but not spongy, and even to the unassisted eye appears to be composed of minute grains (*b*, *b*, *b*, Fig.),



that are connected with one another by bridges of similar material, or, as it may be otherwise expressed, are only partially separated from one another by processes of connective tissue dipping inwards from the general investing sheath. Many different appellations have been given to these grains. They are the "cells" of Malpighi, the "little clumps" of Noll, the "glandular elements" of Brücke, the "lobuli" of Donders, the "alveoli" of Kölliker, the "follicles" of Leydig. When the gland is either naturally or artificially filled with fluid, these follicles projecting on the surface give to it a knobby or granular appearance. On section they present a yellowish or reddish-yellow colour, and are found to be largest and best defined near the surface, becoming smaller, much less completely separated from one another, and more angular in form,

as they approximate the medullary substance (*d*, *d*), with which, in fact, they gradually blend. The medullary substance, as described in general terms by His and Brücke, is soft and spongy in texture, reddish, reddish-brown, or even black from deposition of pigment, and contains a few large blood-vessels distinguishable to the eye. These are provided with a strong *T. adventitia*, the branches of which are partly distributed to the medullary substance itself, and partly radiate to the alveoli of the cortex. Throughout this tissue are numerous fine passages, which serve as conduits for the lymph and chyle.

From this general description it appears that the points in the structure of the gland to which it is especially requisite to direct attention are—1, the general investing sheath, the hilus stroma, and the trabecular structure which pervades the gland, and confers upon it its due degree of firmness; and, 2, the proper glandular substance, composed of an external or cortical and a more central or medullary portion. We shall then be in a position to follow the path pursued by the lymph and chyle in their passage through a gland.

The stroma of the hilus was formerly mistaken for the true medullary substance, leading to great discrepancies in the accounts of the latter, which it has been the great merit of His satisfactorily to clear up and explain. It consists of connective tissue, containing fat-cells in its meshes, with large blood-vessels, and an abundant network of fully developed lymphatic vessels, in which respect it differs most essentially from the medullary substance, the canals or passages of which are not directly continuous with the efferent lymphatic vessels. The stroma of the hilus is whitish and tough, and appears porous on section from the presence of so many large lymph- and blood-vessels. It is highly developed in the inguinal and axillary glands of man, dipping deeply into the interior of the gland, and sending off processes which nearly or completely extend to the periphery. The true medullary substance in these glands is, on the contrary, small in quantity, and in young subjects it has been found by His to be represented only by a small concentric reddish stripe intervening between the central mass, composed of the stroma of the hilus and the brighter coloured cortical substance. In the same glands of the ox, the true medullary substance is, on the other hand, developed to an extraordinary degree, whilst the hilus stroma plays only a subordinate part. A similar preponderance of the true medullary substance over the hilus stroma is seen in the mesenteric glands of the ox, sheep, dog, and rabbit, and probably also in man. In the rabbit the mesenteric glands present a very remarkable arrangement, being fused together into a single mass or organ known

as the pancreas Asellii, somewhat resembling the pancreas of the human subject. Here there is scarcely any cortical portion. It has not been satisfactorily determined whether the tissue of the stroma of the hilus can increase with advancing age at the expense of the true medullary substance, or whether the state of development of these two parts in relation to one another is persistent and unchangeable.

1. *The general investing sheath and trabecular structure.*—The sheath of an ordinary gland is composed of connective tissue *a a*, Fig.), with numerous spindle-shaped corpuscles interspersed amongst the fibrils. Externally it becomes very loose, and continuous with the surrounding cellular tissue, which contains more or less fat, small lobules of which are seen here and there in the sheath itself. Internally it sends off processes or septa dipping into the substance of the gland and extending to the hilus stroma. The question of the presence of muscular fibres in the sheath and its processes has long been debated. Malpighi, in his 'Epistola de Glandulis conglobatis,' was the first who positively determined their presence, maintaining that the outer layer of the sheath contained vessels, whilst the inner one was composed of muscular fibres—a view which was regarded as erroneous by Haller and others. Heyfelder more recently again called the attention of microsopical observers to the presence of unstriped muscular fibres in the sheath and septa of glands, having found them to be particularly abundant in the mouse, but very sparingly distributed in man. Heyfelder's statements have been fully supported by Brücke and Donders; and His remarks that they are so abundant in the ox as to make up the greater proportion of the trabeculæ. The fibres do not unite, but cross in various directions, and after the action of nitric acid they appear as long flat bands, which on being teased out present the appearance of elongated fusiform cells. He believes them to be present, though rarely, in the inguinal and axillary glands of man. The thickness of the sheath varies from 1-50th to 1-500th of an inch, attaining the former size in the glands of the extremities which are more or less exposed to pressure, and in the bronchial glands. It is usually sparingly but sometimes abundantly supplied with blood-vessels, some of which are derived from external sources, whilst others reach it from the branches distributed to the interior of the gland. The trabecular structure given off from the inner surface of the sheath differs in arrangement in the cortical and medullary substances, and is the chief cause of their different appearance. In the cortical part the trabeculæ enclose more or less perfectly circular spaces, often of considerable magnitude, whilst more internally they circumscribe angular or prismatic spaces; in

both instances these constitute the alveoli (*b*). The trabeculæ then gain the medullary substance, where they form a trellis-work with close meshes, the fibres constituting which are at the same time much more delicate. If we conceive the contents of the interstitial spaces of the trabecular tissue to be wanting, it is obvious from the above description that a series of inter-communicating cavities would remain, the shape of which would be dependent on the arrangement of the trabecular tissue. In the outer part of the cortical substance they would be ampulla-form and large, becoming smaller internally (*b'*), whilst in the medullary substance they would appear rather as a branched canal system (*c*), the spaces circumscribed by the connective tissue being short and more or less cylindrical, and freely communicating in all directions, though still generally converging towards the hilus. This entire system of cavities may be injected from the vasa afferentia when the pressure employed is considerable and, as His observes, according as the inquirer lays more weight on the presence of ampulla-formed dilatations, or upon the bridges or isthmuses which connect them together, will he consider the glands as consisting of cells which open into lymph-vessels, or as a network of lymph-vessels with manifold windings and with lateral pullulations.

2. *The medullary substance* (*d*).—This substance is essentially composed of an extremely delicate stroma or network of connective tissue, in which numerous blood-vessels ramify, and which contains in its interstices a large number of lymph-corpuscles. In the cortical part of the gland it occupies the cavities of the alveoli, or follicles formed by the trabeculæ of the sheath, but it does not completely fill them, a space being left between the surface of the trabecula and the surface of the medullary substance (*e*), which is traversed by a few coarse fibres connecting the two together, and, as it were, fixing and maintaining the medullary substance in position. The arrangement may perhaps be better understood by referring to the comparisons made by Frey and His, the former of whom compares the medullary substance to the canvas of a piece of worsted work, which is fitted into its wooden frame (represented by the trabeculæ) and maintained in position by a few radially arranged threads ; whilst His likens it to a series of cavities that have been filled with some plastic substance, which on setting has contracted in all its dimensions, leaving a space surrounding it on every side, interrupted only here and there by fibrous adhesions. This space is the lymph-sinus, and is from 1-2500th to 1-800th of an inch in diameter. Towards the centre of the gland, in those instances where it is strongly developed, the medullary substance forms a well-defined mass, and the follicular arrange-

ment is less perceptible; for here the connective tissue forms a much more delicate web of branches and anastomosing filaments or trabeculæ (*f*), each of which, however, is still surrounded by a lymph-sinus, which separates it as in the cortical alveoli, from the medullary substance itself. The greater number of these sinuses, and their closer collocation, cause the medullary substance to resemble a series of short tubes, though they are in reality of solid though spongy consistence. These glandular canals, as His terms them (Drüsenschlauchen) are the structures which have so frequently and erroneously been considered to be intraglandular lymphatic vessels; the truth being that the lymph-sinuses (*b*, *b*) are the passages which are really continuous with the afferent and efferent vessels.

It is in the description of the delicate connective tissue (*b*, *d*) forming the basis of the medullary substance that the chief discrepancies in the accounts given by different authors occur. His and Billroth describe it as simply consisting of delicate filaments, which at their points of junction or division present in some instances nodular swellings, or in others nuclei, and in others again cells, and which are connected on the one hand with the sheaths of the blood-vessels traversing the glandular structure, and on the other form a tolerably compact and dense membrane, though clearly not a uniform and impervious investment, covering the surface of the alveoli, or, in other words, those surfaces of the medullary substance which abut upon the cavities of the lymph-sinuses. His maintains that the tissue gradually becomes finer towards the central part of each alveolus, until at length it is altogether deficient, leaving a clear space (*g*), which he has termed the vacuola, and is of opiniou that this hiatus is of constant or almost constant occurrence in all glandular structures. He suggests, with much diffidence, that it may serve as a reservoir for the chyle or lymph; though a strong argument, he admits, against this view is the fact that these spaces do not become filled with injection unless this has been forced in with powerful pressure. The size of the vacuolæ is from 1-100th to 1-240th of an inch: they do not appear to have attracted the attention of other observers.

Before proceeding to detail the views held by Frey on the structure of the medullary substance, and especially on the relations of the connective tissue, we shall here briefly state the mode in which the vessels are distributed as described by His. In the inguinal, cervical and axillary glands of the ox several large vessels may usually be seen to enter the hilus, whilst a few smaller twigs may also penetrate into the interior from the convex surface of the gland. Having attained the hilus stroma, some of the branches of the vessels enter the trabeculæ and

accompany them in their further course; others (*h*) dip into the medullary canal-like masses, radiating always towards the periphery, giving off numerous branches which form a network of polygonal-shaped meshes of capillaries surrounding the medullary canals; and from these veins arise, which again, like the arteries, occupy the axis of the medullary canals. From the central medullary substance the vessels penetrate into the follicles, and form a particularly close network near their outer surface, or that part which is surrounded by the lymph-sinus; but no branches to the follicles were ever observed by His to be derived from the trunks accompanying and imbedded in the trabeculæ. These always passed to the surface of the gland, and there formed a plexus of capillaries. The lymph-sinus is always free from vessels; consequently the vascular system of the trabecula and of the glandular substance is, except in so far that they are derived from the same primary trunks, almost completely independent. Fine capillary branches cross the vacuolæ already mentioned, and these spaces are for the most part surrounded by circularly arranged branches. The structural arrangements of the medullary substance in man are, according to the same author, essentially similar, with such modifications only as might be anticipated from the excessive development of the hilus stroma and the comparatively thin layer of the medullary substance which are commonly present.

Now, if we turn to Frey, we shall find the following description of the connective tissue. In the follicles, he remarks, the matrix (*b*) consists, in all instances, of a continuous delicate cellular network with roundish, polyhedral or indeterminate meshes, but presenting considerable variations both as regards the cells and the caudate processes given off from them, according to the age of the person from whom the gland has been taken, the turgescence of the gland, and often according to its pathological condition. If we examine the mesenteric glands of a fœtus of about the sixth or seventh month, we shall find the tissue to consist of a tolerably close network, at the larger knot-points of which well-defined oval nuclei, containing one or more nucleoli, and surrounded by a delicate cell-wall, may be seen. The caudate processes—which are sometimes as many as five, six, or seven in number—radiate in all directions, and either meet with those of others, or themselves branch and intercommunicate, forming smaller knot-points in which no nucleus is visible. In the interspaces of the network in youth, as well as at the adult period of life, are numerous lymph-corpuscles (*m*) with a somewhat sparing distribution of capillary blood-vessels. In the glands of the adult, the knot-points are commonly imperfectly defined, and contain only the shrunken

nucleus. Near the periphery of the follicle (*b*) the fibrous network becomes condensed and the fibres more closely interwoven, by which means an imperfect capsule is formed, though it must clearly be understood that the follicles never possess a separate and distinct tunica propria capable of being detached by manipulation. As regards its arrangement in the medullary portion, Frey describes it as it appears in the pancreas Asellii, where, he says, it seems to be formed by the junction of the processes of the stellate connective-tissue corpuscles. After food —and in this statement Frey essentially differs from the opinions of His and Billroth—after food, and particularly when this has been of an oleaginous nature, the cells and their prolongations contain fat globules, giving them an opaque and milky appearance, and proving what may also be directly shown by the microscope, that the caudate processes of this intra-cavernous cellular network open directly into the lymph-canals of the medullary portion of the gland. It is apparent, therefore, that Frey regards the fibres, bands, and laminæ of the connective tissue as being themselves of a tubular nature, capable of conveying fluids—as being, in fact, offsets or lateral prolongations of the lymphatic system, and as communicating more or less frequently and directly with the interareolar passages and lacunæ. Frey considers that the structure of the pancreas Asellii of rabbits furnishes the means of determining, because it presents in a very simple grade of development the structure of the medullary substance of the more complicated mesenteric glands of man, and we shall therefore give a brief abstract of his observations.

On examining a fine section of the pancreas Asellii injected from the cæliac artery and portal vein, and macerated for twenty-four hours in spirit, on gently brushing it with a camel-hair pencil it will present to the naked eye a net-like or cellular arrangement, and with low microscopic powers it may be observed that the whole medullary mass consists of the convolutions of numerous branched and anastomosing canals, each of which contains in its axis a blood-vessel, artery, vein, or capillary. These canals may be termed lymph-tubes (evidently equivalent to *ff*), which we have already shown must be regarded as in reality portions of medullary substance, and not as tubes. They vary greatly in size, both as compared with one another and also in different parts of their course, in consequence of their being closely compressed together. A number of fine intercommunicating fissures, lacunæ, or spaces are left, giving to the whole medullary substance a spongy aspect and texture. The lymph-tubes possess a very simple structure, consisting merely of a transparent, colourless, and delicate mem-

brane, with faintly marked longitudinal striæ on their outer surface. They are in relation with the connective-tissue matrix above described. Their contents are lymph-corpuscles, in greater or less number, which often fill up the cavity of the canal, and in uninjected specimens entirely conceal the central or axial blood-vessel. After oleaginous food, fatty particles are also easily discernible surrounding and accompanying the lymph-corpuscles. The peculiar relations of the smaller arteries, veins, and capillaries, in being always contained within a lymphatic tube, is worthy, Frey goes on to say, of particular notice. It is rare to find a portion of lymphatic tube destitute of a central blood-vessel, and it is further remarkable that the contained blood-vessel has always lost its external tunic, or tunica adventitia; and, in point of fact, we may conceive that this membrane is replaced by the lymph-tube itself which loosely surrounds it, and an arrangement is thus effected by which the interchange of the constituents of the two fluids, lymph and blood, by diffusion or osmosis, is singularly facilitated, since the only septum that intervenes between them is the delicate membrane formed by the union of the internal and middle coats of the smaller vessels.

Now, we may remark on the above description that, notwithstanding the close resemblance which the tissue just described presents to tubes, it is undoubtedly incorrect to term them so. The tubes of Frey are really portions of medullary tissue, so separated and bounded by splits and fissures—which, as we shall immediately see, are the true lymph paths or sinuses—that they resemble tubes. That they are really formed of medullary substance, is shown by their containing a matrix of delicate connective tissue with lymph corpuscles in the interstices, and by the circumstance that they do not in the first instance become filled with injection when forced in through the afferent vessels; though, no doubt, owing to the spongy texture of their periphery, which forms one of the boundaries of the fissures or splits, the fluid, when forced in with great violence, will penetrate through the condensed matrix and gain the interior. A further proof, if further proof be required, of their glandular nature is, that these processes of medullary tissue contain blood-vessels which, as has been already stated on the authority of His, is never the case with the true lymph-paths.

Course pursued by the Lymph in its passage through a Gland.

On injecting one of the large afferent vessels running towards a gland, it is usually found to divide into several branches, which dip into the sheath and are at once lost. In the majority of

instances it is impossible to follow the vessel, with its proper walls, beyond the sheath itself, with which, as Goodsir? originally stated, the tunics appear to blend. The fluid passes easily from the vessels into the spaces surrounding the alveoli of the cortical substance, which are consequently mapped out, and surrounded by a red or blue ring, according to whether carmine or indigo has been employed. Where the force employed has been slight, and in those glands where the cortical and medullary substances are well defined, the fluid may never penetrate into the medullary substance at all, but, running round the alveoli, rapidly gain the efferent vessels, and so flow on to another gland. This course is termed by Frey the superficial lymph path. The channels through which the injection flows in such cases are formed on the one hand by the periphery of the alveolar or follicular masses of medullary substance, and on the other by the internal surface of the trabeculæ, and they are traversed by the coarse radial fibres above alluded to. Each trabecula, since it separates two alveoli, is bordered by two lines of colour. It is evident from such injections that there is a direct and easy communication between the circum-alveolar spaces and the afferent and efferent vessels; and we may suppose under ordinary circumstances, where the glands are not actively performing their function, that this is the course pursued by the lymph. But if the injection be driven with greater force, it is soon apparent from the more complete coloration of the follicles, and of the medullary substance of the gland, of which the follicles are only rounded prolongations, that it has entered other channels and is pursuing a deeper course. This course is of a double nature. In the first instance, it traverses the splits, fissures, and lacunar passages (*g*) which are found in the medullary substance nearer the centre of the gland, and are directly continuous with the circum-alveolar spaces just mentioned, than which they are at once more irregular in form and more frequently connected with one another by anastomosis. In the second instance, the fluid may and does penetrate from the follicular investing spaces into the very substance of the alveoli and of the medullary tissue (from *c* into *b*, or from *g* into *f*). The periphery of these, it will be remembered, which abuts upon the wider lymph paths or sinuses, is not invested by a dense, firm, and continuous membrane, but is simply a condensation of the connective tissue forming the matrix of the glandular substance; it consequently presents numerous fine slits and fissures between the fibres, which, though sufficiently small and compressed to prevent the passage of fluid under ordinary circumstances, yet will, when great pressure is exerted, open to a sufficient extent to admit readily the passage of fluid, and, which is of greater importance, the exit of the lymph-

corpuscles from the interstices of the matrix into the wider channels of the lymph-sinuses. Frey appears to think that the fibres and stellate cells of the matrix of the medullary portion of the gland—or, as he terms them, the fibres of the intra-follicular cell network—are hollow, and states that he has succeeded in injecting them with carmine in the mesenteric and inguinal glands of the dog; but this must be considered as yet doubtful.

The exact mode in which the efferent vessels of the lymphatic glands take their origin has not been made out; but there can be little question that they commence from the lymph-sinuses of the medullary substance, the glandular prolongations of which cease at the point of exit, the vascular walls immediately developing themselves from the tissue of the trabeculæ, which in oxen is so rich in muscular tissue.

From all this we gather, that in the interior of the glands there is a system of channels for the circulation of the lymph of great complexity, and which, though continuous with the afferent vessels, is certainly not formed by them. If we reflect on the necessarily resulting retardation of the current occasioned by the frequent division and reunion of these passages—the numerous blood-vessels contained in the medullary substance and its cortical prolongations through which the lymph slowly percolates, it will be immediately evident that it is in this part that the greatest facilities are offered for the interchange of material between the lymph and blood. During the period of quiescence, the formation of lymph-corpuscles is slowly taking place in the interstices of the matrix of the glandular substance, and the lymph, which is simply the surplus material poured out from the blood-vessels for the purposes of nutrition, circulates chiefly in the larger channels on the surface of the gland; but after food, when absorption is being energetically performed, the flow of lymph, and in the mesenteric glands of chyle, is, through the gland, greatly increased, the lymph channels or sinuses both in the cortical and medullary substances are dilated to the utmost, their walls are consequently distended, and the apertures, fissures, and slits in their walls are sufficiently opened to admit the passage of lymph-corpuscles from the glandular tissue when they are in process of development into the lymph-sinuses, from whence they are borne along into the afferent vessels.

If Frey's statements respecting the tubular character of the connective-tissue fibres be true, there would yet be a third course taken by the lymph, or by fine injection carefully thrown into the afferent vessels, namely, into the interior of these very delicate connective-tissue fibres; but the instances in which the entry of injection into them has been observed are so few and doubtful, and are opposed by the negative statements of so many

writers, that it seems scarcely worth while to take it into serious consideration.

On the place of origin of the Lymph-corpuscles.

According to the ordinarily received view, the lymph-corpuscles are developed in the interior of the glands, from whence they are washed out by the current of lymph; and as their generation takes place continuously, so also do they continuously enter the stream of lymph circulating through the glands, their number, however, being very materially increased when the pressure of the fluid is augmented, as after meals. Lister observed in the lacteals of mice in the act of digesting food, lymph-corpuscles which, on account of their size, he regarded as completely developed. This opinion seems to be substantiated by a remarkable case of Baum's, quoted by Teichmann, in which a fluctuating tumour formed in the arm after venesection, from which, when opened, a clear fluid flowed. On stroking the arm upward, the discharge was increased, and there seemed little doubt that it was really an enlargement of a lymphatic vessel. When this fluid was examined by the microscope, not a single corpuscle could be discovered in it. Kolliker, however ('Zeitschrift f. Wiss. Zoologie,' Bd. vii, p. 182), found numerous corpuscles in the vessels of a well-fed dog, between the intestines and the mesenteric glands; and Brücke ('Denkschrift d. k.k. Akad., vi, p. 131 et seq.) made a similar observation. In these cases, therefore, we must either admit that the corpuscles were formed in the radicles or capillaries of the lymphatic system, or we must consider that they were developed by the solitary or agminate follicles, which indeed, as we shall hereafter show, present many characters indicating a close analogy to the mesenteric glands. In order to solve this question, Teichmann made a series of special investigations on the contents of the lymphatics of the extremities immediately (1—3 hours) after death in three decapitated criminals. The points at which the vessels were opened in the first subject were—1. In the arm between the skin and brachial fascia in the lower third of the biceps; 2. In the middle part of the leg between the cutis and subjacent fascia. Two vessels were opened in the first experiment, and one in the second. The greater proportion of the lymph obtained from the lower extremity appeared colourless, transparent, and slightly opaline. With the microscope, four out of five specimens obtained from the arm showed clearly the presence of lymph-corpuscles, though in small numbers: in the fifth, however, they could not be discovered, showing that error may arise when

very small quantities are examined. All the specimens obtained from the leg contained corpuscles. The lymph began to coagulate forty minutes after collection, and the coagulum exhibited many corpuscles imbedded in it. Similar observations with similar results were made in the other two convicts. To avoid all chance of error from the possible presence of glandular organs in the skin itself which might produce lymph-corpuscles, Teichmann subjected the finest ramifications of the lymphatic vessels to a careful scrutiny without being able to discover any structure that could be regarded in this light; and as in the above experiments there was a complete absence of blood-corpuscles, which speaks strongly in favour of the purity of the lymph, he believes he may fairly draw the conclusion, that the ordinary hypothesis is incorrect, and that the seat of the formation of the lymph-corpuscles is not exclusively the lymphatic glands, or those simpler structures which he terms the retia, but is also certainly external to them, taking place in those minute vessels and stellate cells from which he believes the lymphatic system to take origin, though doubtless the exact details of their development is still obscure. M. Tomsa also (see his "Essay on the Formation of Lymph" in the 'Wiener Sitzungsberichte,' Bd. xlvi, p. 185) seems to have convinced himself that isolated lymph-corpuscles may frequently be seen in the lymphatics of the testis of the dog.

Chemical Composition of the Lymph.

The latest observations that have been made on the composition of the lymph are contained in the last volume of Virchow's 'Archives' (xxxvii, 1866, p. 55), and are by Dähnhardt under the supervision of Professor Hensen. The lymph was obtained from the thigh of a patient suffering apparently from Barbadoes leg, or some analogous affection, and a considerable quantity was daily obtained (500—700 Ccm). It had a whitish turbid appearance, passed quickly into putrefaction, possessed an alkaline reaction and a specific gravity of 1·007. The following table shows the results of three analyses:

1000 parts of lymph contained—

	No. I. analysis.		No. II.		No. III.
Water	987·7		986·126		985·201
Solids	12·3		13·874		14·799
Fat		0·030			
Organic extractives soluble in alcohol		1·284			
Organic extractives soluble in water = extractive and albumen		0·908		3·811	6·875
Organic substances insoluble in water and alcohol = fibrine and insoluble albumen		1·699			
Inorganic substances soluble in water		8·076			
= Chloride of sodium	6·148				
Soda	0·573				
Potash	0·496		9·816	.....	7·777
Carbonic acid	0·638				
Sulphuric and phosphoric acids, and loss	0·221				
Inorganic substances insoluble in water		0·303		10·063	7·924
= Chalk	0·132				
Magnesia	0·011				
Oxide of iron	0·006				
Phosphoric acid	0·118		0·247	.....	0·147
Carbonic acid	0·015				
Carbonate of magnesia, sulphuric acid, and loss	0·021				

Professor Hensen himself endeavoured to determine the nature and quantity of the albuminous substances more exactly, and obtained the following results. He found that 1000 grains of lymph contained

1·070 of fibrine not perfectly purified,
1·408 of serum-albumen, and
0·894 of albuminous compounds precipitated by acetic acid.

It is worthy of particular notice that the proportion of the organic to the inorganic constituents was very low, not being more than as 296 : 704 whereas in Schmidt's analysis it was as 815 to 185, and in Nasse's (horse) as 877 : 123. Now, Professor Hensen points out that this is a circumstance in favour of Ludwig's and Tomsa's views of the origin of lymph, since the pressure under which the blood flowed in this man was, in consequence of the insufficiency of his auriculo-ventricular valves, exceedingly low, whence it was to be expected that those constituents of the blood which could easily osmose into the intertextural spaces would do so in larger proportion than those which, like albumen, require a considerable force to cause them to transude through the vascular walls.

(*To be continued.*)

REVIEW II.

Die Krankhaften Geschwülste. Dreissig Vorlesungen gehalten während des Wintersemesters 1862—1863 *an der Universität zu Berlin.* Von RUDOLPH VIRCHOW. II Band.

On Tumours. Thirty Lectures delivered during the Winter Session of 1862—1863, *in the University of Berlin.* By RUDOLPH VIRCHOW. Vol. II, pp. 756. *Berlin,* 1864—65.

THE second volume of this remarkable book, which, owing to stress of work and the multifarious occupations of the author, had long been delayed, is at length completed. Virchow's original design of finishing the book in this volume has been of necessity laid aside, owing to the unavoidable bulk of some of the lectures; and there is yet abundant material left in the history and description of the cancers to furnish a large third volume, which we hope to see ere long completed.

The expectations raised by the style and excellence of the first are more than fulfilled in this volume, some of the chapters in which must be regarded as masterpieces of compilation and original description combined.

Lect. 17 treats of the *Osteoma,* or bony tumour. This is to be distinguished from other tumours which are prone to ossify by the fact that the whole plan of its development is bony and that it tends to bone-formation from first to last. Still, he confesses to the difficulty of determining in many instances whether a tumour is to be called osseous or ossified enchondroma. Before going into detail, he alludes to the mistake so often made by surgeons who dry bony tumours in order to preserve them. By this method of preparation, the soft parts of the tumour are lost, and, after maceration, it will be often impossible to distinguish a bony tumour with a large medullary cavity in its centre from a cartilaginous tumour with a bony capsule. Like the chondromata, the osteomata are either *hyperplastic* (bone growing in bone), or *heteroplastic* (bone growing in soft parts); but, whereas in the case of the chondromata the heteroplastic forms are common, in that of the osteomata they are rare, unimportant, and not malignant. The hyperplastic tumours are divided into exostoses, periostoses and hyperostoses, and enostoses. One form of *exostosis* is the subject of especial notice; that, viz., which is developed out of cartilage, is covered over in greater part with cartilage, and grows most often in long bones—humerus, femur, and tibia—shooting or sprouting out from the line of junction between the epiphysis and diaphysis, where there is a layer of cartilage present up till puberty. Two ex-

planations of this are offered. First, in the not uncommon cases where these tumours date from a very early period of life, it may reasonably be supposed that the primordial cartilage tends at a certain part to deviate in its development. Secondly, in cases where the tumour is of later growth, it is to be explained by the action of an irritant on the bone at the above-mentioned part, causing an unwonted lateral development of the cartilage. With the exception of these and certain of the loose cartilages in joints, the majority of exostoses are developed out of connective tissue. The consistence of the exostoses, as of all osseous tumours, is very variable, depending on the quantity of blood-vessels and medulla that they contain. Sometimes they are ivory-like; sometimes spongy, containing abundant soft cancellous material; sometimes medullary, containing large medullary cavities. The ivory exostoses which grow from the outer and inner table of the skull are described at length, and a microscopical section of one is drawn to show how the bone lies in dense concentric layers superimposed one on the other. This concentric growth takes place in two different ways: first, as in the compact substance of long bones, by internal concentric growth within the medullary canals; secondly, by external concentric growth, the bony layers growing from the periosteum or connective tissue around the bone, and being deposited one outside the other. The osseous tumours of the orbital and frontal region require a separate mention. They occupy especially the upper and inner part of the orbit, and grow, some from the periosteum, others from the medulla of the diploe. Of these, the former are true exostoses; the latter are at first rather enostoses, but appear afterwards as exostoses by growing and forcing their way through the bony shell which covers them. They are most often met with in the young, their first development dating back from an early period of life, and depending probably on some disturbance in the original formation of the bone. The bony growths which form at the points of insertion of tendons or fasciæ are curious. They are the result of ossification which proceeds from the surface of the bone and extends to the connective tissue contained in the adjacent tendon or fascia. Interesting specimens of the kind are seen in the "Reitknochen" of horsemen, the little bony growths which spring from that part of the pubes and ischium from which the adductor muscles take their origin, and which is pressed upon in riding.

Periostoses and hyperostoses differ from the exostoses in that their base of attachment is not defined or limited. The former term signifies that part of a bone's surface is involved, the latter that the bone is uniformly enlarged. He traces the resemblance between hyperostosis and elephantiasis Arabum. Hebra has

shown how the latter disease is in most cases preceded by repeated attacks of erysipelas; and Virchow quotes the famous Prague case related by Gruber, where, after repeated attacks of erysipelas, the whole head became the seat of hyperostosis. He suggests that careful observation will swell the list of such instances.

A true *enostosis*, or bony tumour growing out of the medulla in the interior of a bone, is very rare. It has been already stated that certain of the tumours in the orbital region which take their origin in the diploe belong to the enostoses.

Due weight must be attached to local causes in considering the origin of the osteomata; for it is a well-known fact that the bones which lie most exposed to injury are more liable than others to be the seat of bony tumours.

Constitutional causes, such as syphilis, rheumatism, rickets, are undeniable.

Lastly, hereditary tendency exercises a decided influence; and it is in the early growth of the bone that this predisposition manifests itself.

Heteroplastic osteomata are well studied in the membranes of the brain and spinal cord. They are not infrequently met with in the dura mater, to whose inner surface they adhere as a consequence of chronic pachymeningitis, or inflammation of that membrane. Again, they sometimes form in the outer layer of the choroid membrane of the eye, and are mistaken for ossifications of the retina. The vitreous, too, is occasionally occupied by bony growths which lie close behind the lens.

Lect. 18 is devoted to the description of certain tumours of the nervous system which the author thinks fit to separate from the great group of sarcomata, and to which he gives entirely new names. The first of these is named *psammoma*, or sand-tumour (ψάμμος). The brain-sand is well known as the substance which is so often met with in the pineal gland, choroid plexuses, and Pacchionian glands. It consists of small, round, or oval granular bodies with a central chalky nucleus. When crushed, they break into angular fragments; and on the addition of an acid, the lime which they contain is dissolved out, leaving behind an organic basis-substance. They resemble closely in appearance the "corpora amylacea," and were formerly confounded by Virchow with those bodies, but differ from them essentially in consistence and chemical characters. Their mode of development varies. First, they seem to be formed in some cases by calcareous deposit in the connective tissue. Secondly, as Ludwig Meyer has shown, they may be developed out of cells, as, for instance, the epithelial cells of the arachnoid. Thirdly, as Virchow believes, the greater part of them are the merest

concretions, strictly inorganic bodies, increasing by repeated deposit of lime. A psammoma is, then, a connective-tissue tumour which contains an agglomeration of these sand-bodies, or, as Busk ('Journal of Microsc. Science,' 1854, No. vi) has named them, "corpora chalcedonica." It is smooth on the surface; seldom larger than a cherry; has a reddish-white colour and a tolerably firm consistence; is separable from the surrounding parts without much force. When put in water, its fibrous framework unfolds itself, while the sand-bodies are loosened and either escape or hang out free in the surrounding fluid. The favorite seat of this tumour is where brain-sand is commonly met with, viz., in the choroid plexuses of the lateral and fourth ventricles. It is also found, in old people, attached to different parts of the dura mater, particularly about the base of the cranium, in front of the sella Turcica. It is a consequence of chronic irritation, being often found in connection with chronic pachymeningitis, and is therefore especially common in epileptics and insane people.

For a species of tumours which take origin in the neuroglia, or delicate interstitial matrix of the brain, Virchow proposes the name of *glioma*. This tumour is composed of an intercellular substance, varying in quantity and toughness according to the properties of the neuroglia out of which it is formed; of cells and nuclei. The cells vary in shape and size: sometimes are small, with fine, delicate, anastomosing offshoots; at other times are large, round, spindle-shaped or stellate, with large and long offshoots. Gliomata are of two kinds, soft and hard. The soft variety bears a very close resemblance to the myxoma, or, when full of cells, to the medullary sarcoma. The harder form is more allied to the fibrous tumour; its basis-substance is harder and tougher, and its cells are small with fine shining nuclei: it has been described by Robin and Hardy under the name of "scleroma," but that name should be reserved for cases where the nervous elements are present and there is no unusual development of cells. *In the glioma, nervous elements are not present;* but the neuroglia alone has undergone a localised hyperplasia, and the neuroglia cells are excessively increased in number. The glioma is never sharply separated from the surrounding brain-substance, but looks almost as though it were a part of the brain. It is, however, recognisable on section by its greater vascularity, its different consistence, and its more translucent, bluish-white appearance. It will often reach a considerable size, that of a fist or even a child's head, and, being of slow growth, may exist without causing any symptoms, till at length a blood-vessel bursts in it, or it kills by causing effusion into the ventricles. It is, as a rule, solitary, and is not malig-

nant. Its favorite seat is one of the posterior cerebral lobes, and, next to this, the upper and lateral surface of one of the hemispheres. The cerebral nerves—as, for instance, the auditory—suffer occasionally from the same tumour. In them the perineurium is the starting point from which it grows. The retina may be in like manner affected, the connective tissue in the outer layer of that membrane next the choroid serving as the centre of development. Detachment of the retina follows, and the growth, gradually pushing its way across the eyeball, around the contracted vitreous, forms a thick funnel-shaped mass, which stretches from the optic entrance to the ora serrata, or else fills up the whole globe, and at length breaks through the cornea, so as to protrude in form of a fungous excrescence. Such a tumour is a glioma so long as the cells and nuclei do not exceed in size those of the connective-tissue layer of the retina; but where the cells and nuclei are larger, the growth is apt to be malignant, and must be referred to the sarcomatous group.

Lect. 19.—The name *sarcoma,* which has been used in the most different senses—which was once employed by Abernethy as a sort of generic term for all tumours with a "fleshy" look, and which has of late fallen almost entirely into desuetude is revived by Virchow, and forms the subject of the most elaborate and comprehensive chapter in the volume. The reader feels, as he reads, the truth of the author's statement in his preface, "that in one line—nay, sometimes in a few words, is contained the result of many a search after references, and many a patient anatomical observation." Each of the connective tissue group of tumours which has been already considered—the fibroma, the myxoma, the chondroma, the osteoma, the glioma—may occasionally undergo a further development, and a kind of transformation by which its cells increase materially both in size and number, and the tumour becomes strictly a *cellular tumour* (*the cells, however, still retaining the general characters of those in the mother-tissue from which they are developed*) or a sarcoma. In this way may be formed a sarcoma fibrosum, mucosum, cartilaginosum, osteoides, gliosum, according to the original nature of the tumour from which the sarcoma is derived. But a sarcoma may also start from the very first as a sarcoma, and grow directly from the cells of the connective tissue by partition and multiplication of the same. It thus passes at once into what Virchow denominates the "granulation," *i. e.* cellular, stage, and is from the beginning a sarcoma, without ever having put on the dress of any one of the connective-tissue group. We have in this way a *sarcoma by transformation,* and a *genuine sarcoma by birth.* In both, the essential characters of the tumour are the same; but the latter is naturally more malignant than the former, because of the excessive number of cells

that it contains. Under the head of sarcoma are thus included the "fibroplastic tumours" of Lebert—the "malignant fibrous," "recurrent fibroid," "fibro-nucleated," and myeloid of Paget. Besides these, very many cases hitherto described as medullary cancer would be included. The adjectives "malignant" and "recurrent," expressive of physiological peculiarities, are of course rejected by Virchow, who bases his classification strictly on the anatomy of tumours. Instead of the term "myeloid tumour," Nélaton's "tumeur à myeloplaxes," he proposes the name "sarcoma giganto-cellulare." As may be supposed, the cells of the sarcomata vary much in size and shape, though all are but modifications of the connective-tissue cell. They may be round, spindle-shaped, fusiform, or stellate. The nuclei and nucleoli are, as a rule, very large, the former sometimes filling up almost the whole interior of the cell. In nearly all cases the cells are singularly fragile, so that, when the tumour is fresh, with any but the very gentlest manipulation the nuclei are set free, and give rise to the mistaken notion that many of these tumours are composed of free nuclei, the first stages of new cells. So much is this the case, that whenever Virchow examines a fresh tumour, and finds it composed of large, naked nuclei, with shining nucleoli, he at once suspects it to be a sarcoma. The greatest care is therefore necessary in the examination of such specimens, and it is best to harden them first in chromic acid. The intercellular substance always holds a decided relation to the cells, which are not so close packed together as in soft cancer. Chemically, it is seldom purely gelatinous, as in true connective tissue, but contains generally an abundance of albumen, caseine, or mucine. Microscopically, it is sometimes homogeneous, at others granular, at others distinctly fibrillated. All small-celled sarcomata are more dangerous than the large-celled varieties. Thus the giant-celled sarcoma and those which are made up of colossal spindle-cells are, as a rule, benign; whereas the smaller and round-celled glio- and myxo-sarcomata are very near the cancers in malignity. The difficulty, indeed, of distinguishing some of these latter from medullary cancer is confessedly very great; but the criterium is to be found in the relation that the cells hold to the intercellular substance. In the sarcomata the cells are everywhere separated and supported by intercellular substance, which is uniformly distributed, and not in alveolar form. In the cancer they are close crowded or packed together, and lie thus in alveolar spaces. Again, the elements of the sarcomatous tumour blend with those of the surrounding tissues; not so those of the cancerous, which are more or less isolated and distinct from them. But yet, he allows, "there are mixed forms of sarcoma and cancer." Nor are these to be regarded as cases in which sarcomatous elements have degene-

rated into cancerous, but they are strictly mixed tumours, the two different species starting from the same point, and growing side by side from the same mother-tissue, like two branches on one trunk. It may be said of almost any kind of sarcoma that *it has a tendency to return in loco;* but it is the soft, cellular, juicy forms which are prone to metastasis. Propagation takes place—1st, by direct continuity, *a*, in homologous tissues; *b*, in heterologous tissues (as when a tumour spreads from the eye to the orbit): 2nd, by discontinuous extension, *a*, of secondary or daughter tumours in the neighbourhood; *b*, of daughter tumours, exactly like the parent, in distant parts. The latter, which is called metastasis, or dissemination, is effected by the blood, either the cells or the juice being transported, and acting as excitants in the organs to which they are carried. A strong argument in favour of this explanation of the phenomenon, and against the supposition that the tumours are developed simultaneously in different organs of the body in consequence of a dyscrasia, is, he thinks, to be found in the fact that the secondary tumours are always identical in structure with the primary. He also instances the sarcoma as a tumour that in its mode of origin strongly supports the cellular pathology, and the doctrine of tumour-formation in consequence of local irritation, as opposed to the dyscrasiæ of the humero-pathologists. For, first, sarcoma frequently shows itself in parts which are naturally in an excited or irritable state; *e.g.* sexual organs at the time of puberty: secondly, it forms as a consequence of inflammation; *e.g.* sarcoma of the eyeball following chronic inflammation of the globe; of the serous membranes following repeated attacks of inflammation in those membranes: thirdly, it often follows injuries, especially of the breast and testicle. Space will not allow us to reproduce the careful sketches that the author gives of the different varieties of this tumour as it shows itself in the different tissues, the bones, the breast, the testicle, &c. One variety, the pigment- or melano-sarcoma, a form of melanosis, has already been noticed in this Review (for July, 1865, p. 39), and thither we refer the reader for a short account of it. The surgical moral of *early removal in all cases, if possible,* is drawn from the history of the sarcoma.

In Lect. 20, Virchow classifies a series of morbid products which are not usually included among tumours, which form another link between inflammatory products and tumours proper, but which have in many cases so decided a form and shape that he proposes to rank them among the tumours, under the title of *granulation tumours.* The granulation tissue, composed of cells in masses, and seen in its typical form in young marrow, has been already shown to constitute a kind of transition

stage through which many of the sarcomata pass in their development. But the morbid products now under consideration never get beyond this stage, never ripen into connective-tissue, but consist of transitory elements which tend to soften and degenerate. Their starting-point is in the connective-tissue cells. These latter form, by partition and multiplication, groups of small round cells, resembling lymph-cells, which lie in the midst of a certain quantity of intercellular substance, the whole being characterised by a marked tendency to fatty degeneration. The syphilitic gummy tumours, lupus, elephantiasis Græcorum, and the products of glanders and farcy, make up the group under consideration. The *gummy tumour* is wrongly regarded as being a product of tertiary syphilis alone: for, in the first place, the primary induration of syphilis is really a small gummy tumour, exactly identical, in miniature, with the larger form; and the appearance of an ulcer (chancre) is due to nothing else than the central softening and breaking down of the small tubercle which first forms. Again, the mucous tubercle is likewise a gummy growth of a less developed form. Hence the chancre, the mucous tubercle, and the gummy tumour *par excellence* are all, strictly speaking, syphilitic tumours. In describing the effects of syphilis on the internal organs, he draws particular attention to the different ways in which it affects those organs. Thus it may act as an irritant, either on the interstitial or on the investing connective tissue, and give rise simply to an increase of that tissue, by exciting on the one hand an interstitial, on the other a capsular inflammation. Further, you have but to imagine the irritant a little stronger, and the gummy tumour results. Another point worth noticing is that the gummy tumour grows by preference in those parts of the organ where the soil has been, as it were, prepared for its reception by a previous syphilitic inflammation, and is very often found set in a surrounding framework of cicatrix-tissue, a product of the action of the syphilitic virus.

The description of these different appearances in the various organs is masterly. A parallel is then drawn between syphilis and malignant disease. Both begin in the form of tumours, the syphilitic induration and the mother-nodule (Mutterknoten) of the cancer; both affect the nearest lymphatic glands; both spread by metastasis and involve distant parts, the gummy tumour exactly repeating the primary induration, just as the secondary cancerous deposit repeats the primary. *Lupus* is a young, soft, vascular granulation-tissue, which has its origin in the connective tissue of the cutis, and thence penetrates to the deeper parts. It soon degenerates and softens, breaking

down centrally, and forming the well-known ulcer covered with a scab, beneath which the disease slowly progresses.

Virchow, like Hebra, is opposed to the belief that lupus is a form of syphilis, from the fact that, first, in post-mortem examinations of people with lupus he has never found a trace of syphilis in internal organs; secondly, that lupus is very chronic, painless, and unattended with cachexia; thirdly, that with it there is an absence of any glandular affection. In like manner the assumption that lupus is a scrofulous disease (Hardy and Bazin) is quite arbitrary and unsupported by facts; for the most marked symptom of scrofula, glandular disease, is absent in lupus; and, on the other hand, in the most decided cases of scrofula there is no lupus. Both Virchow and Hebra are convinced that there is but one remedy—destruction of the affected part by caustic.

The tumours of the *Elephantiasis Græcorum*, or Spedalskhed, are very closely akin to those of lupus, but they are more enduring than the other granulation tumours; and, though they break down and ulcerate in parts, they do not manifest the same constant tendency to degeneration. They first show themselves, after a long *stadium prodromorum*, in the cutis, in the form of dark-red or bluish spots, over which the epidermis is but little, if at all, changed. In the next stage the spots become tubercles, hard and red, generally broad-based, sometimes pedunculated, for the most part prominent, but here and there deep-seated, and scarcely protruding beyond the level of the surface. At the same time, the hair falls off, and the sweat and sebaceous glands perish. The surface of the tumours now assumes a pale, dirty-yellow tint, and they soften centrally. The microscope shows invariably a granulation-tissue rich in cells, no enlargement of the papillæ, but an unusually smooth and level cutis-surface. Besides the skin and mucous membranes, the lymphatic glands and testicle, as well as the lungs and liver, are liable to be similarly attacked. The "lepra anæsthetica" is due to the same disease in the nerves; but in the form of a chronic perineuritis, or swelling of a considerable portion of the nerve, rather than a distinct tumour. In the last stages, parts of the body become deeply gangrenous, so that sometimes the limbs drop off,[1] and the disease is called "lepra mutilans." Virchow went to Norway in 1859, commissioned by the Government, and investigated the matter thoroughly. But, though he was able to make the most extensive and satisfactory observations on the anatomical characters and

[1] Von Hasselaar relates the case of a mulatto affected with advanced leprosy, who, on walking into his house, knocked his head against the door and fell down with his head separated from his body!

symptoms of Spedalskhed, he could form no trustworthy opinion as to its cause. He therefore wrote letters to all parts of the world where the disease is endemic, begging for information and help. This appeal has been so far responded to, that he has received a number of valuable communications, but not as yet sufficient for him to make any decided statement. The hereditary predisposition is not enough, for it only holds good in about one fourth of all cases. It is certainly not contagious. He looks rather to certain conditions of *place* and *diet*, hitherto undetected, to clear up eventually the vexed question. There is no doubt that the disease prefers the sea-coast or the banks of large rivers, and that the diet of those inhabitants of Norway who are afflicted with it is very poor, consisting in great part of bad fish; but there is more yet to be made out. It was not till the year 1821 that Schilling, in Berlin, made known the first authentic observation of *glanders* communicated to man from the horse. Since then the number of similar observations has increased a hundred-fold, and a year seldom passes in Berlin without one or more cases coming into the pathological room for dissection. Glanders and *farcy* are but different manifestations of the same disease. The former takes on a more decidedly fever type, and is accompanied by more pustular outbreaks, carbuncle and muscular abscesses, in man than in the lower animals; but, even in man, the characteristic little yellow tubercles, which tend to ulcerate, will be found if sought for in the mucous membrane of the nasal passages. The lymphatic glands are less severely and less constantly affected in him than in the horse, but metastases in the lungs are exceedingly common. The specific tumour, first shown by Virchow to be a neoplastic formation out of pre-existing tissue, and not an exudation, appears as a tubercle much like that of lupus, and exhibits the same disposition to break down and soften, constituting the ulcer of glanders when superficial, the abscess when deep. In farcy the tumours (buds) are larger, more chronic, and more persistent, presenting, therefore, more analogy with the tumour of lepra than with that of lupus.

In Lect. 21, the auther grapples with a very wide and a very difficult subject. Leukæmia with its essential structural changes, scrofula, tubercle, and simple hypertrophy of lymphatic glands, are all fully dealt with under the common title of "lymphatic tumours," or, preserving the same termination hitherto adopted, "lymphoma." The two great primary divisions into which he separates this group, viz., "hyperplastic lymphomata," mere hypertrophied lymphatic glands, and "heteroplastic lymphomata" growths of lymphoid elements in parts where they do not naturally exist, make it clear that all the tumours comprised

under this denomination exactly imitate in their composition the simplest lymphatic gland, and are made up of cells whose type is the lymph-corpuscle or lymph-cell. In connection with *leukaemia*, one of Virchow's great pathological discoveries (for, though not the first to observe and relate a case of the disease, he was the first to give the true explanation of its real nature, and to apply to it the appropriate name which it now bears), both forms of lymphoma are present.—A. The hyperplastic form, in the hypertrophy of the spleen, or the lymphatic glands, or both, on which the excess of white corpuscles in the blood essentially depends. He states that there is a slight difference in the appearance of the blood according as the disease affects the spleen or the lymphatic glands, so that a *lienal* may be distinguished from a *lymphatic* leukæmia. In the former, the cells present in the blood exactly resemble the white blood-cells in appearance, in shape, and in the change that they undergo on the addition of acetic acid; but they are rather larger than those cells. In the latter, they are smaller than the white blood-cells, and possess, each of them, one single, rather large nucleus, which contains abundant granules. In the massive glandular enlargements so well known in these cases, the condition of things is very similar to that in the scrofulous enlargement; but herein is an important difference—that in the leukæmic tumour the gland remains a perfect gigantic lymphatic gland, discharging its functions and maintaining its proper relations with the blood and lymph which circulate through it: in the scrofulous enlargement, so soon as it has reached a certain stage, so surely does a retrograde change (nekrobiotischer process) begin—the elements perish, and the function of the gland is annihilated. —B. The heteroplastic form is best studied in the liver and kidneys. The liver is generally enlarged, and strewed with little collections of lymph-corpuscles agglomerated either in branching lines which run through the whole organ, or in small greyish-white deposits which encircle the several acini. These all start from the connective tissue of Glisson's capsule, and very closely resemble miliary tubercles, but differ from the latter in their soft, almost fluid consistence, and in that they show no tendency to fatty degeneration. In the kidney they are exactly the same, and are clearly seen with the microscope to be formed in the matrix, especially round and about the Malpighian bodies, and to push their way between the tubules. They are, indeed, small lymph-follicles, formed in the midst of organs which naturally contain nothing of the kind.

The riddle of *scrofula* which has perplexed so many generations, and been read in so many different ways, is thus solved by Virchow. In the first place, the lymphoma, which is the most prominent feature of the disease, is not, as is wrongly

supposed, a primary change depending on a scrofulous dyscrasia or a scrofulous poison circulating in the blood, but is always secondary, depending on some irritant conveyed from the neighbourhood, most often the skin or mucous membranes, by the lymphatics to the glands. Thus, eruptions of the scalp and catarrhs of the mucous tract of the mouth, nose, and throat, precede the cervical lymphoma; attacks of bronchitis, the bronchial lymphoma so common in children, and, in the majority of cases, wrongly termed tubercular disease; while catarrh of the gastro-intestinal tract is a forerunner of a similar mesenteric lymphoma. The catarrh, it is true, may be often slight and transient, but the disease has been set a-going in the glands with which the mucous membrane communicates, and holds to them with strange tenacity even though the exciting cause is past and gone. But something more than this is required. Either through hereditary tendency or acquired weakness (a result of bad food and air), certain parts of the body, the lymphatic glands in particular, are brought into such a condition that a slight irritant, which in another person would produce no effect, causes them to swell up and become the seat of a hyperplastic change. After persisting in this state with extreme obstinacy, they at length tend to decay, undergo the fatty degeneration, and are converted into a yellow, cheesy, useless mass. For the occurrence of scrofula, then, are wanted, first, *certain vulnerable tissues in which a morbid process of hyperplasia can be excited by a slight and seemingly insufficient cause;* secondly, *persistence of that morbid process after the withdrawal of the exciting cause.* In like manner, certain diseases, especially those of the skin and mucous membrane, are well called "scrofulous"—*e.g.*, ophthalmia, ozœna, eczema—because they have an intimate connection with glandular affections, and run a peculiarly chronic course. It will be at once obvious to his readers how Virchow, in this way, avoids the theory of a scrofulous poison which is supposed to wander about in the blood and settle ever and anon in the lymphatic glands, and solves the whole difficulty with the aid of his cellular pathology.

Are tuberculosis and scrofula different diseases, or are they one and the same morbid process? This is a much-vexed question, and one which cannot be answered till it is first clearly stated what is understood by the word *tubercle*. Virchow's views on this subject are so well known in his 'Cellular Pathology,' that it is here unnecessary to repeat them. We would merely remind our readers that the only tubercle recognised by him is the grey miliary tubercle, either single and separate, or aggregated in clusters; that what is talked of commonly as "yellow tubercle," distinct from the grey, is considered to be

really nothing else than either the grey after it has undergone fatty degeneration, or a mass of inflamed tissue which is the seat of the same cheesy change, and which ought more properly to be called scrofulous. Herein, now, lies the essential difference between these two diseases—that tubercle is heteroplastic, scrofula hyperplastic. A scrofulous enlargement is a simple multiplication of pre-existing lymphatic elements; a tubercle is a growth of lymphatic elements in a part or tissue where they do not naturally exist—the connective tissue. It may be that tubercle, seeing how often tubercular disease attacks at a later period of life persons who in early youth have had scrofulous glands, is no more than a heteroplastic or metastatic scrofula, offering an exact analogy with the heteroplastic lymphoma in leukæmia; but, on the other hand, so many cases occur where scrofula has never preceded tubercle, that, while admitting the near relationship which exists between them and the frequent coincidence of the two, it seems best, on the whole, to separate tubercle from scrofula, and give it a distinct individuality. The counterpart of the leukæmic and scrofulous lymph-cell is seen in the tubercle-cell, which is generally rather smaller than a white blood-cell, with colourless, faintly granular contents, and a single small shining nucleus. The tubercle-cell is also singularly fragile, so that the nucleus is often set free and alone appears. It is to be distinguished from Lebert's *specific tubercle corpuscle*, which is nothing but a shrunken, atrophied body, met with in all decaying tissue, cheesy pus, dead scrofulous glands, and degenerate cancer. Cheesy metamorphosis is the regular end of every tubercle; for there is nothing in the body so perishable as tubercle. Virchow is now convinced that there is a tubercular disease of lymphatic glands distinct from the scrofulous. The morbid process begins in the connective tissue of the gland, especially in the septa, and does not involve the glandular tissue directly. The gland, on section, is seen beset with small greyish deposits which give it a finely turbid or clouded look; and, at a later stage, when the cheesy change has set in, a diffuse cheesy infiltration of the whole gland, such as occurs in scrofula, is never met with, but the masses are always smaller and more isolated. The principal features worthy of notice in the history of tubercle seem, then, to be as follows: 1st, its purely heteroplastic nature; 2nd, its frequent eruption in multiple form; 3rd, the small size of the original deposit; 4th, its tendency to perish; 5th, the frequency with which its formation in a tissue is preceded by inflammation of that tissue. Virchow recognises two forms of tubercle—A. In which the original outbreak is multiple, and the tubercle not infectious (the term infection being employed to signify secondary deposit

by metastasis); B. In which the original outbreak is single or localised, and spreads to distant parts by metastasis, the blood serving as the vehicle of transmission (as seen in the secondary tubercular disease of the lung when the primary disease is in the urinary tract, or secondary disease of the liver and kidneys when the primary disease is in the lung). A closer look at certain forms of tubercle must thus suggest to any observer the resemblance which it bears to cancer, and show that, in its tendency to spread locally, and disseminate its germs in distant parts or organs of the body, tubercle has the closest relations with the malignant tumours.

The term *lympho-sarcoma* is applied to *that form of glandular enlargement which is not attended with leukæmia.* The lymphatic glands sometimes become the seat of enormous hyperplasia, and *do not show, as in scrofula, a proneness to degenerate,* but simply persist in growing, and are terribly refractory to every kind of treatment. This distinction of a lymphatic tumour separate from the leukæmic and scrofulous, is, therefore, quite in keeping with facts. There is, further, this peculiarity in the lympho-sarcoma—that it has sometimes quite a malignant character, the spleen being more particularly the seat of secondary heteroplastic deposits, in form of translucent, greyish-white, firm, irregular masses, not limited by a capsule, but blended with the spleen-tissue. Some of the cases described by Hodgkin ('Med.-Chir. Trans.,' 1832) and Wilks ('Guy's Hosp. Rep.,' 1856) as cases of lymphatic glandular enlargement in connection with peculiar deposits in the spleen, belong, evidently, to this subdivision of lymphatic tumours.

Review III.

Lehrbuch der Psychischen Krankenheiten. Von Dr. Max Leidesdorf, Docent der Psychiatrie an der Wiener Hochschule, &c. &c. *Erlangen,* 1865.

Textbook of Mental Diseases. By Dr. Max Leidesdorf, Lecturer on Psychology at Vienna, &c. &c. Second Edition, revised and enlarged. 1865.

This book deserves to be better known than it is likely to be while it remains untranslated. It gives evidence of conscientious and hard work, and, coming from a well-known teacher and physician at Vienna, carries with it some of the *prestige* belonging to the best medical school in Europe.

The author shows an acquaintance with the writings of other

countries not common among Continental writers, and an accuracy still more rare.

In the historical sketch of the development of psychology, the progress of this study is traced through many ages in the records of various European nations, and with great fairness as regards England, where the change has been the most marked. It is difficult to be novel on such a theme, and we are, therefore, not surprised to meet the name of Saul as the first authentic lunatic, nor to find David referred to as an instance of feigned insanity; but we are, perhaps, not so familiar with Nebuchadnezzar as the victim of delusions!

The anatomy and physiology of the brain are first discussed, and the succeeding chapters are occupied by detailed descriptions of the causation and various phases of insanity, their pathology, treatment, and medico-legal bearings.

The author divides mental affections into three categories, viz. those characterised by depression, by exaltation, and by weakness, considering separately paralytic dementia, and that due to epilepsy. Such a classification is at once a simple and useful one, and far preferable to the elaborate systems which multiply varieties to the mere confusion of the reader. There is no great difference as regards the main features of mental disorders between English and German writers, the point on which there is least agreement being probably the details of treatment. Foreigners still find "non-restraint" a stumbling-block in the way of their belief, and while indulging in ill-concealed doubts as to the existence of the system, somehow forget to try the experiment for themselves. It sounds oddly in English ears to hear of directions how to apply the strait-jacket, and of the care to be taken lest it should press on the nerves or interfere with the circulation, but we must give Dr. Leidesdorf credit for the honesty with which he states his views. He says—

"The jacket has, according to my experience, when it is applied for sufficient reasons, a therapeutical use, and is a remedy which often calms the patient, is sometimes desired by him, and can be in no way injurious or dangerous. I disapprove of restraint as a means of discipline, but I value it as a therapeutical means of quieting excitement. I am convinced that two thirds of those restrained in many asylums would be better without it, but that one third could have no means of cure so harmless and really calmative."

In the treatment of acute cases of excitement, the author is in the habit of giving acetate of zinc in preference to tartar emetic or ipecacuanha. He dissolves from twenty to forty grains in six ounces of fluid, and gives two table-spoonfuls every two hours. He also recommends digitalis as a "narcotic

acting directly on the nervous centres," and prefers it to other depressing remedies, as it can be given for a longer time, and its effects can be watched more closely by means of the pulse. He, of course, disapproves of the device of "compressing the carotids in order to quiet maniacal excitement"! The chief difference between the treatment adopted in this country and on the Continent consists in the much greater use made by foreigners of baths, and we are inclined to think that this means has been too much overlooked hitherto by English physicians. Dr. Leidesdorf remarks that the duration of a bath must depend on the strength of the patient and the degree of excitement, but that in acute cases tepid baths may be used for six or seven days running for from one to three hours. Brierre de Boismont advocates keeping such cases in for ten or twelve hours at a time, but discontinues it if no improvement ensues after eight or ten baths. Professor Hebra has an idea that the withdrawal of atmospheric air on the one hand, and the continuous application of moisture on the other, as well as the maceration of the epidermis, contribute largely to the cure of mental diseases; and, in 1862, he produced a patient before the Medical Society of Vienna who had been *in a continuous bath for* 109 *days and nights!*

The chapter on Pathology is the most valuable in the book, and contains very able expositions of the more recent views of the German inquirers as to the microscopical appearances and morbid changes observed in the brain. In speaking of the alterations produced by atrophy of the brain in the elementary structures of the nervous tissues, the author says—

"We find the ganglionic cells of the cortical substance either filled with a brownish granular pigment in senile atrophy, or, which is more rare, undergoing a fatty change, though this latter change rather affects the cell-elements of the neuroglia, as Virchow has stated (in the 'Gesammte Abhandlungen'). I have not been able to determine whether the ganglionic cells undergo any diminution in size, in spite of the measurements I have made, for their normal size is subject to great variations, but more frequently the contrary is the case; for I have found in the cortical substance large bleb-like cells, with delicate walls, containing neither nuclei nor granular contents, and distinguished by their peculiar power of refracting light, reminding one of growths which had undergone amyloid degeneration, but not giving any violet colour on the addition of iodine and sulphuric acid. Such forms occur more particularly in the brains of melancholics and old people, in children, and in those who have died from typhus. The nerve-tubes are either destroyed or shrunken, or have lost their medullary contents.

"The intercellular substance, which, as in hypertrophy, is the chief seat of the change, is found in the slightest forms of the disease

to have undergone scarcely any alteration, being merely condensed, or it is present in relatively greater proportion, which is either caused by atrophy of the included nerve-elements, or by proliferation, *i. e.* an increased production of nerve-cells and of intercellular substance. . . . . The changes in the vessels, which are of the greatest moment as regards the conditions of atrophy, are of especial interest.

"Whilst the fatty and pigmentary metamorphoses of the walls of the vessels occur especially in the brains of the aged, we meet with, in advanced atrophy, and particularly in dementia of whatever degree, changes which, according to Wedl, consist in atrophy of the capillary veins, and of the capillaries opening into them. This atrophy, which always exists in the most marked degree in the cortical layer, consists, on the one hand, in a shrinking of the nuclei of the walls of the vessels, and, on the other hand, in a change within the structureless internuclear substance of the walls, which becomes marked by many lines, with total extinction of the cavity of the vascular tube.

"It is plain that these changes cannot be without influence on the nutrition as well as the circulation. The former of these we see injuriously affected within the part supplied by the diseased vessel, so that, in the slighter forms of the affection, substances which ought to be excreted, such as fat, pigment, calcareous matter, &c., are deposited in the region in question. When the nutrition is still more defective, the part of the brain is atrophied. Moreover, since at many points a complete obliteration of the capillaries emptying into the veins follows, whereby the flow of blood is considerably hindered, the nutrition of the vascular walls also much impaired, and in consequence the vessels often become dilated, it follows that the equality of the circulation must be notably disturbed, which in its turn reacts on the vitality of the brain-functions."

Arguing from the well-known connection between the grey matter and the intellectual power, he says (p. 267)—

"If the circulation in the cortical layer be destroyed, the normal nutrition must be interfered with, since the cells of that part necessarily receive either too much or too little material, and consequently changes, which in the end cause their destruction, take place within. We then see the cortical layer undergo atrophy, the memory lost, and dementia follow.

"The atrophic states of the brain and its cortical layer are especially peculiar to paralytic dementia, without, however, as Griesinger holds, the extent of the dementia always corresponding with the degree of the atrophy. The peculiarity of the individual attacked, the position of the atrophy, and the state of nutrition of the convolutions affected, are not without their influence on the degree of the dementia. The changes most frequently met with in the majority of cases of paralytic dementia are thickening and œdema of the membranes, adhesion of the pia mater to the cortical layer, which has a dirty-brown discoloration, of a darker or lighter shade, according as hyperæmia is or is not present. The peripheral layers appear in the first stages spongy, soft, and moist, and adhere in patches of

considerable extent to the pia mater when the latter is stripped off; in the later stages they are more resisting, thicker, extremely hard, pale, and in extreme cases the outer cortical layer is quite destroyed. The medullary substance is thus often atrophied in its whole extent or in patches, is unnaturally compact, and the ventricles are dilated. These changes often deviate from the above description, according as the adhesion of the cortical layer to the pia mater, or the general atrophy of the cerebral mass, or the sclerosis of the cortical layer preponderates. The essence of these changes is, according to Rokitansky, generally an extreme and long-continued hyperæmia, but sometimes an inflammation of the cerebral surface and the membrane covering it, causing destruction of the nerve-elements, while at the same time the neuroglia is stimulated to proliferous action, and gradually passes from the state of a viscid fluid into a tough, fibrous mass, and disorganises the cortical substance by destroying the nerve-elements.

"There are frequently found with the proliferation of the connective tissue and destruction of the nerve-elements, colloid and amyloid bodies in great quantity; and the ganglionic cells are torn from their connections, distended, and filled with fat-granules.

"Now, while Rokitansky is of opinion that the morbid changes observed in paralytic dementia consist essentially in an hyperæmia with proliferation of the connective tissue of the cortical substance, others, and especially the French writers, have given it the name of chronic diffuse periencephalitis, regarding it as a chronic inflammation of the surface of the brain, in which the pia mater and arachnoid invariably take part, any separation of the affections of these parts being artificial, and the cell-changes as arising from an active perversion of nutrition with multiplication of the nuclei.

"Tigges examined the cortical layer of the brains of those dying from paralytic dementia, and on macerating portions in a mixture of alcohol and water, found cells of a ganglionic character, containing a variable quantity of nuclei, and, from investigations of various morbid processes of short duration, came to the conclusion that the ganglionic cells are liable to undergo active nutritive changes with growth of nuclei, though it might be difficult to demonstrate this in cases of paralytic dementia, owing to the long duration of the disease."

Dr. Leidesdorf notices the frequent occurrence of hypostatic pneumonia as a cause of death in the bedridden insane, and says he has found this the case in 25 per cent. of the deaths from general paralysis. He also refers to the frequency of gangrene of the lungs in cases of refusal of food, having found this in 26 out of 287 post-mortems.

He sums up his conclusions on the influence of pathological changes on mental phenomena as follows:

"We see, then, that mental disturbances may be traced back to extensive irritation of the brain and its membranes, giving rise to nutritive, formative, or destructive changes, attended by more or less

serious consequences, sometimes curable, sometimes not so. Among the causes of such irritation must be mentioned hyperæmia, which I believe to be of the greatest importance, as, under its influence, there are frequently developed mental disturbances which, indeed, often disappear on the cessation of the hyperæmia. More frequently, however, hyperæmia leads to alterations in the brain allied to the nutritive processes, and consisting in an increase of the molecular contents of the cells, such as we have occasion to observe in cases of acute mental disturbance in the epithelium of the membranes, and still more in the cells of the cortical substance. Should the hyperæmia continue a longer time, or end in a more serious perversion of nutrition, then we see formative changes which are associated with graver psychical disturbances.

"As changes of real importance, we must mention the proliferation of the epithelium of the dura mater and arachnoid which occurs in all cases of brain irritation; the cell proliferation in the pia mater, in the outer coat of the vessels and connective tissue, which according to my experience occurs even in acute cases, as well as the division of the nuclei in the ganglionic cells. These formative changes generally begin in the cerebral membranes, spread from these to the vessels of the cortical layer, and end in the interstitial connective tissue; but there may be exceptions to this order. Generally, the formative processes do not exist by themselves, but further changes are met with which show themselves either as retrograde or destructive metamorphoses. Among such are included the fatty degeneration of the epithelium of the meninges, the cell-growth in the outer coat of the vessels and in the ganglionic cells of the brain, the amyloid degeneration of the ganglionic cells and vessels, the calcification and also total obliteration of the vessels.

"As these changes extend, we see the primary conditions fade, while the secondary forms become more apparent, and end in the total destruction of mental power.

"The diseased state of the vessels must, further, be insisted upon as a constant and important change. As a consequence of it, we see considerable disturbances of the circulation and nutrition: on the one hand, dilatation of the vessels takes place, and, on the other, a change in the cell-elements, an increase and alteration of the interstitial connective tissue occurs, to which latter we should attach great importance, since the interstitial connective tissue is implicated in most brain affections, and especially in a chronic form, it causes the most severe mental disturbances, as a type of which we may take paralytic dementia, in which almost all the above-mentioned changes are found. But, while the changes in the brain in the acute and even in the chronic primary forms are essentially different to those in the secondary, there is, from a pathological point of view, no perfectly characteristic distinction between the primary forms of melancholia and mania. Acute cases of melancholy are very rarely fatal; but in those of longer duration that die, we often find anæmia of the brain, and generally œdema. Still, it must not be overlooked, that the state of depression, the dread and anxiety, diminished

nutrition, abstinence, &c., may have produced, in a secondary way, a general and also a localised cerebral anæmia. From a theoretical point of view, it may be assumed that frequently one and the same pathological change, according to the individual and the spot in the brain affected, can cause essentially different symptoms. This is most plainly seen when we observe different individuals in various degrees of excitement. While one man, through excess of wine, *e. g.* gets into an excited and extravagant state, another becomes silent, or says but little, is reserved, and sheds tears of emotion or despair. This is, however, by no means a proof of the unity of the brain-processes when symptoms of excitement or depression are present, but at most affords indications which may in many cases be useful." (P. 301, &c.)

Review IV.

Om Brokincarceration. Afhandling for Doctorgraden i Medicinen, af P. Plum, forhv. Reservechirurg ved Frederiks Hospital, Kjöbenhavn. G. E. C. Gad, 1866.

On Strangulated Hernia. A Thesis for the Degree of Doctor in Medicine. By P. Plum, formerly Surgeon Extraordinary to Frederick's Hospital, Copenhagen. G. E. C. Gad, 1866, 8vo, pp. 200.

The author remarks, that in the copious literature which exists upon the subject of strangulated hernia, one branch of it has been treated in "stepmother style," and that is the statistical. In not one of the principal works upon the subject has any, he says, extensive statistical basis been laid for the opinions therein expressed as to the etiology, prognosis, or treatment of the disease. And yet it is in this mode that we can first expect any true progress in our knowledge of the essential nature and treatment of the latter. In his investigation of the subject the author has taken as his foundation the material supplied by Frederiks Hospital during the last thirty-five years, for five of which he had the opportunity of personal observation as Candidate and Surgeon Extraordinary to the Institution.

In starting, it is necessary to decide as accurately as possible what is to be understood by strangulated hernia. Most writers are agreed that by this term such an entanglement of the hernial viscus is to be understood, that the intestine, in whole or part, cannot, as formerly, be returned into the abdomen. But the question then arises, does strangulation exist only when the hernia cannot be reduced without operative interference? or is the name applicable also to those cases where it cannot be re-

placed without considerable difficulty? Another question is, whether other morbid conditions may not make the hernia for the time immovable; and accordingly various authors have adopted hernial inflammation, hernial obstruction, and congestion or overloading of the hernia (*engouement*), sometimes as stages in or as different forms of strangulation; sometimes as independent of the latter, as peculiar forms of disease whose origin and treatment are different from those of strangulation proper.

Previously to deciding these points, the author lays before his readers a brief retrospect of the history of his subject: but, as our object is chiefly to place his statistical deductions within reach of the English reader, we shall pass over this portion of his volume, interesting and valuable as, being ably done, it undoubtedly is.

The author speaks first of overloading, obstruction (*engouement*) of a hernia, and in doing so distinguishes between overloading with solid and with fluid and aëriform matter. Among 229 cases of hernia operated on, to be found recorded in the journals of Frederiks Hospital, the bowel was in three overloaded with hardened fæces, and in one with undigested food. He next devotes a section to the consideration of hernial inflammation, and comes to the conclusion "that there is no reason to look upon obstruction or inflammation of hernia as distinct forms of disease, differing from incarceration, but that they are both referrible to the latter as elements therein." At the same time he remarks that "the incarceration, as I consider it, is always the cause of the acute hernial symptoms; and these may, therefore, correctly be termed symptoms of incarceration." He adds that it is not easy to give an adequate definition of strangulated hernia, and, after some further remarks, concludes that it may be defined as "the hernia which in its integrity, or in greater part than hitherto, becomes difficult or impossible to replace, and, by reason of greater or less pressure of the hernial gate,[1] gives rise to morbid symptoms" (p. 33).

But our object being, as we have already stated, to pick out, for the advantage of our readers, the statistics of the disease as supplied by the journals of Frederiks Hospital, we shall pass over the more systematic portions of the work, in which the author treats elaborately, and in a manner indicative of great industry and of active research, as well as of intimate acquaintance with his subject, of the opinions of writers as to the nature, &c. of the disease, of its symptomatology and course, its pathological

[1] "By hernial neck, I understand the narrower opening in the hernial sac; by hernial ring, the opening in the abdominal wall; and by hernial gate (Brokporten), the hernial neck and hernial ring together."—Note, p. 36.

anatomy, etiology, diagnosis, prognosis, and treatment, and shall proceed to bring forward such figures from the source alluded to as may appear to us capable of affording the greatest amount of information upon the subject in hand.

The first figures we meet with refer to the seat of the incarceration—a point which it is very difficult and often impossible to decide—a circumstance which probably most frequently depends upon the fact that the neck and ring are, as Chassaignac remarks, in "juxtaposition," that the incarceration occurs in both places. Unfortunately, only 34 cases have been noted in this respect; and in these the incarceration was met with 21 times in the hernial ring and 12 times in the neck of the sac, while in 1 instance it was effected by a newly formed cord in the sac. Of the 21 cases of incarceration in the ring, 9 were found in inguinal hernia and 12 in crural, of which 4 were in the fascia cribrosa and 8 in the annulus cruralis; and of the 12 incarcerations in the neck, 10 were found in inguinal hernia and 2 in crural.

We have next some statistics in reference to Pitha's observation, that the symptoms are more violent and the danger greater the higher up in the intestine the hernia is situated—the nearer the seat of constriction is to the centre of the nutritive nervous system, the solar plexus. The inference drawn by the author from these statistics, referring to 12 patients dead in the first three days of strangulation, to 19 dead on the fourth, fifth or sixth day, and to 28 dead after the sixth day, is, however, that "hernias of the upper part of the intestinal canal have not led to a more rapid death than those of the lower part, and that in the violence of the symptoms, also, there has not in this respect been any remarkable difference" (p. 66).

As to the question how a strangulated hernia proves fatal, the author remarks that in the great majority of instances peritonitis is certainly the cause of death. Thus in 126 cases dead in Frederiks Hospital, either after operation or without it, and where a post-mortem examination was made, peritonitis was met with 119 times, and it had most frequently been so violent as to be sufficient to have caused death. Only in seven cases was no peritonitis present, so that in these death must be supposed to have occurred in consequence of nervous collapse. Malgaigne[1] mentions such a case of death on the eighth day, where no trace of peritonitis was met with; and Pitha[2] records a similar instance of death after twenty-four hours. As Gosselin states[3] that he has met with this form of death only in cases which

[1] 'L'Union Médicale, 1854, p. 248.
[2] 'Prager Vierteljahrschrift,' 1846, i, p. 93.
[3] 'Leçons sur les hernies abdominales, par Léon Labbé, Paris,' 1865, p. 123.

have been operated on, and therefore proposes the question whether the result may not possibly be attributable to the wound, the author remarks that, among the seven cases just mentioned, there were two in which no operation was undertaken, but where death occurred after replacement. He also communicates at length a similar case, which occurred recently in Frederiks Hospital, where death ensued on the third day of strangulation, while the hernia was still unreduced.

It was formerly stated that gangrene was very common in cases of strangulation. Among 229 cases of herniotomy investigated by the author, in 3 only was the whole intestinal loop gangrenous; in 40, smaller gangrenous spots and perforations were met with.

The author gives the results of his investigations as to the *predisposing causes* of strangulation:

"Most authors have been agreed that crural hernia exhibits a decided predisposition to become incarcerated. Pott is the only writer whom I have found to express an opinion opposed to this; he states[1] that crural herniæ are rarely incarcerated.

"Of 512 incarcerated herniæ which have occurred within the last thirty-five years in Frederiks Hospital, 317 were crural, = 55·4 per cent.; 240 were inguinal = 42·1 per cent.; 13 were umbilical, = 2·2; 2 were ventral, = 0·3 per cent.

"We have thus no slight preponderance in favour of crural hernia; but in order duly to estimate this preponderance, it is necessary to take into account the frequency with which these herniæ on the whole occur. According to the statistics of the City of London Truss Society[2] and of Maisonneuve, about eight times as many inguinal herniæ are to be met with as crural; and if the latter were incarcerated only with the same frequency as inguinal herniæ, we should among our cases have met with only 30 strangulated crural herniæ, while we have in fact found 317. Hence it follows that crural hernia is ten times as liable to incarceration as inguinal hernia." (Pp. 87, 88.)

As crural hernia occurs much more frequently in women than in men, we find that strangulation is met with more frequently in the former than in the latter. Of the 572 cases 344 or 60·2 per cent. occurred in women, 228 or 39·8 per cent. in men.

Bearing in mind that hernia on the whole occurs more frequently in men than in women, in the proportion of 4 to 1, it will follow that women are, according to the foregoing, six times more predisposed to strangulation than men are.

Investigating what the predisposition to incarceration is in

[1] Percival Pott, 'A Treatise on Ruptures,' London, 1763, p. 39.

[2] William Lawrence, 'Abhandlung von den Brücken,' Busch, Bremen, 1818, p. 17.

the two principal forms of hernia, inguinal and crural, in the two sexes, we obtain, among 572 cases of strangulated hernia, 291 of crural hernia in women, = 50·9 per cent.; 39 of inguinal in women, = 6·8 per cent.; 26 of crural in men, = 4·4 per cent.; 201 of inguinal in men, = 35 per cent.

The author draws the general conclusion—

"That crural hernia in women is that which, with great preponderance, is most disposed to incarceration; next follows crural hernia in men, then inguinal hernia in women; and lastly, the least disposed to incarceration is inguinal hernia in man." (P. 91.)

A table is next given, showing that the great majority of cases of strangulated hernia in the female occur in women who have borne children, and that it is especially to incarceration of crural hernia that previous delivery predisposes. The decennial period at which the maximum number of cases is met with is between the fortieth and fiftieth years.

It is well known that far more ruptures occur on the *right* than on the *left* side—a rule which applies to both inguinal and crural hernia. Of the two forms taken together, the author found, in the male, 145 right against 81 left; in the female, 186 right against 145 left.

As to the *time of life* at which strangulated hernia is most likely to occur, the author found that it is scarcely to be met with in infancy, that it is very rare in youth, and that, contrary to the opinion of some, "the predisposition to incarceration, far from diminishing, increases considerably with years."

With respect to the *season of the year*, it does not appear that one has more predisposing influence than another.

Of hernias *consisting*, first, of intestine alone; secondly, of intestine with omentum; thirdly, of omentum alone; the latter are by far the least liable to incarceration. Of cases operated on, the author refers 166 to the first class, 54 to the second, and 9 to the third.

As to the *size* of the hernia, the incarcerated medium-sized herniæ are those which occur most frequently both in the inguinal and crural form. Of these the author met with 50·6 per cent. The small incarcerated hernia is the next most frequent, and represents 28·5 per cent. Large herniæ amount to only 20·9 per cent. By large herniæ, Hr. Plum understands those of the size of a large clenched fist or upwards; by medium, those of the size of a hen or duck egg; and by small herniæ, those of the size of a pigeon's egg or walnut.

For the sake of comparison, the author gives a table of 259 reducible herniæ met with in Frederiks Hospital, which ex-

hibits the following per-centages :—Large, 31·3 ; medium, 54·4 ; small, 14·3.

The author remarks that if these latter numbers be used for comparison (the correctness of doing which is, for various stated reasons, doubtful), it would appear that writers are justified in saying that the tendency to incarceration increases with the diminution in size of the hernia ; for if herniæ of all sizes had the same tendency to strangulation, we should expect that the per-centage of the large, medium, and small incarcerated hernia should be respectively 31, 54, and 14 ; whereas it is actually 20, 50, and 28.

"In general it is the *small intestine*, most frequently the inferior two ells (four feet), which is met with in the incarcerated hernia. The large intestine is found only exceptionally. In our 229 cases of hernia operated on, the cæcum was found only in 2, the colon in 2, the sigmoid flexure in 1, and the vermiform appendix in 1 ; and all these were inguinal herniæ. It is therefore not easy to understand how Pott can say,[1] that in the cases where he has been obliged to operate for strangulation, he has most frequently found the cæcum and colon in the hernia ; nor does Sharp's statement,[2] that the cæcum is met with more frequently in crural than in inguinal hernia, seem to derive any confirmation from our statistics.

"Formerly it was thought that operation for strangulated hernia acted in general as a *radical* cure : this opinion has, however, long since been given up, but we may in the great majority of cases say that the operation is a preservative against relapse of strangulation. Nevertheless this is not always true. Among the cases recorded in Frederiks Hospital are two of incarcerated crural hernia where the operation was a second time necessary, in one instance after the lapse of three years, in the other after seventeen years ; and three cases of crural hernia have presented themselves, with symptoms of strangulation, respectively three, three, and five years after having undergone operation for strangulation, but in these the efforts at reduction were successful. Gosselin[3] reports a case of incarcerated inguinal hernia on which he was obliged to operate five years later, and it was subjected to the same operation." (P. 105.)

The chapter on Prognosis affords a wide field for statistics, and the author accordingly introduces a large number of most valuable and elaborate tables, which we regret we cannot reproduce in their integrity. We shall, however, be able to lay before our readers the most striking general results ; and in doing so we shall confine ourselves almost exclusively to the information derived from the records of Frederiks Hospital, as

[1] Op. cit., p. 18.
[2] Samuel Sharp, 'Recherches critiques sur l'état présent de la chirurgie,' par Jault, Paris, 1751.
[3] Op. cit., p. 247.

being that least likely to be found elsewhere in English surgical literature :

"Of the 572 cases of strangulated hernia which within the last thirty-five years were met with in Frederiks Hospital, 154 died—that is, 26·9 per cent.; but of these only 560 were subjected to any treatment in the hospital, and of the latter 143 died—that is, 25·5 per cent.

"Reduction was accomplished in 331 cases—that is, in 59·1 per cent.
Operation was performed in 229 „ „ 40·9 „

"Of the 331 reduced cases 21 died—that is, 6·3 per cent.
„ 229 operated on 122 „ „ 53·3 „

"Besides, there were 12 cases which were neither reduced nor subjected to operation; and of these 11, or 91·7 per cent., died." (P. 111.)

From an examination of the above, and of cases recorded by other writers, the author would fix the mortality in strangulated hernia as between one fourth and one fifth of all cases. He further concludes that reduction is accomplished in about three fifths of all cases, with a mortality of 6 per cent. It appears tolerably evident, he remarks, that the mortality in cases of reduction increases according to the number in which this was effected.

In Table VII the author collects from a large number of sources a total of 2130 cases of herniotomy, of which 904, or 42·4 per cent., were fatal.

Table VIII shows that, according to the records of Frederiks Hospital, the mortality in incarcerated crural hernia is about 50 per cent. greater than in strangulated inguinal hernia; and about in the same proportion, or rather less, is the mortality in strangulated hernia greater in women than in men.

The same records show that most reductions were effected in inguinal hernia in men (71·1 per cent.); next follows inguinal hernia in women (64 per cent.); then crural hernia in men (53·8 per cent.); and lastly, crural hernia in women, where reduction was most difficult of attainment (47·7 per cent.).

Tables XI and XII exhibit the frequency and mortality of herniotomy, according to *sex and species of hernia*. From these it would appear that the operation was undertaken in

"Crural hernia in 50 per cent. of the cases with a mortality of 50·9 per cent.

Inguinal	„	29	„	„	„	58·6	„
Women	„	47	„	„	„	49·4	„
Men	„	29	„	„	„	62·6	„

As to the influence of *previous pregnancy* upon prognosis, it

appears that of 288 females of whom it was ascertained whether they had given birth to children or not—

"218 were women who had had children, of whom died 72 = 33 per cent.; while
70 were virgins „ „ 13 = 18 „

From supplemental tables, it appears "that the slighter mortality in virgins is due partly to the fact that reduction is in them more easily obtained and with a better result, and partly to herniotomy proving in them more successful." (P. 121.)

As relates to prognosis in connection with *the right and left side*, the author has found that ruptures of the left have a greater mortality than those of the right, and that this difference, which is too great to be merely accidental, is due to inguinal hernia.

"Of 331 incarcerated dextral herniæ 79 = 23·8 per cent were fatal.
„ 226 „ sinistral „ 70 = 31 „ „
Of 151 right inguinal strangulated hernia 23 = 15·2 „ „
„ 89 left „ „ „ 26 = 29 „ „

while, on the contrary, the mortality in crural hernia was the same (nearly) for the two sides, since

"Of 180 strangulated right crural herniæ 56 = 31·1 per cent. died.
„ 137 „ left „ „ 44 = 32·1 „ „

With respect to the influence of *age* upon prognosis, Table XIII shows the mortality of strangulated hernia under 60 years to be only 23·8 per cent., while above that age it is 39·2 per cent. Under 60 years, reduction was effected in 58·2 per cent.; above 60, in 55·8 per cent. As to herniotomy, the result was fatal, under 60, in 50·8 per cent.; above 60, in 62·5 per cent.

The author next gives tables to illustrate the increase of mortality according to the *duration* of the strangulation.

The records of Frederiks Hospital show that the *employment of a truss* has not so great a causal influence upon incarceration as some writers have supposed. In like manner it appears from the following that the previous use of a truss has no injurious influence upon the prognosis of strangulation.

In 248 cases in which a truss had been worn, the mortality amounted to 23·4 per cent. In 83 instances, or 33·5 per cent. of all these cases, operation was necessary, and gave a mortality of 51·8 per cent. In 279 cases, on the other hand, where no truss was employed, the mortality reached 39·5 per cent., and the operation had to be performed in 68 per cent. of the cases, with a mortality of 51·5 per cent. The patients who have worn trusses have therefore been more favorably circumstanced than those who have worn none.

With respect to the mortality according to the *contents* of the

hernia, in 229 cases of operation it amounted to 61·6 per cent. where the contents were both intestine and omentum; to 53 per cent. where they were intestine only; and to 11·1 per cent. where omentum alone was found in the sac.

As to the mortality according to the *size* of the hernia, the author found that it was greatest for large herniæ, though in them it did not much exceed the mortality in small herniæ. The mortality was least in cases of middle-sized herniæ. The mortality *after operation* in large herniæ was 76 per cent., almost double that met with in cases of medium herniæ. In small herniæ the mortality after operation was about 10 per cent. greater than in medium herniæ, but was 20 per cent. less than in large.

We have dwelt so long upon the interesting details brought forward by Hr. Plum in reference to the prognosis in strangulated hernia, that we can touch upon only one point connected with the *treatment*, viz., the employment of *chloroform*.

"Ether was first employed in Frederiks Hospital in the treatment of incarcerated hernia in 1847, when it was used in three cases, of which one was reduced. In 1849 a single trial was made with the taxis under chloroform narcosis, but it failed; and in 1850 a similar attempt was made, and likewise failed. Subsequently a couple of trials were made, of which only one was successful, and it was not until 1855 that chloroform was constantly employed as an adjuvant to the taxis. There is no doubt that part of the success with which strangulated hernia has since that time been treated in Frederiks Hospital (see Table XXV) must be attributed to the use of chloroform . . . In 109 cases of strangulation where chloroform was employed during the taxis, reduction was effected in 74 or 67·97 per cent. Of those cases where reduction was effected, 5·1 per cent. died, and of those operated on 51·4 per cent., while the total mortality amounted to 20·2 per cent. If this per-centage be compared with that yielded by the same circumstances for all cases treated in the same and earlier periods, it will be seen that much better results have been obtained than by treatment in the two earlier periods; but a somewhat less result than by treatment of all cases in the last period, including those treated without chloroform; and it is hence evident that the favorable result of treatment in the last twelve years does not proceed exclusively from the fact that chloroform was employed in some of the cases, but also from this—that the taxis was more efficiently used." (Pp. 158, 159.)

It will be borne in mind that in the foregoing notice of Hr. Plum's very elaborate and valuable work, we have, according to the plan we laid down in commencing our examination of the book, confined ourselves almost entirely to attempting to place

within reach of the English reader some of the important statistics afforded by the records of Frederiks Hospital. We have consequently passed over in silence much of his volume which would also be worthy of careful review.

REVIEW V.

Traité de Médecine opératoire, Bandages et Appareils. Par le Docteur CH. SEDILLOT, Médecin-Inspecteur des Armées, Professeur de Clinique Chirurgicale à la Faculté de Médecine de Strasbourg, &c. Troisième Edition. *Paris:* Baillière et Fils, 1865, 2 vols. 8vo.

Treatise on Operative Medicine. By Dr. C. SEDILLOT, Army Medical Inspector, &c.

DR. SEDILLOT is a man well calculated to write a treatise upon operative surgery. He has seen a great deal of it, both in civil and military practice; and he has had abundant opportunities of estimating the value of the remedial measures which he describes. As inspector of armies, he has been able to collate cases from a large area, and to study the various questions of hygiene upon a great scale; while, as professor of clinical surgery at Strasbourg, he has had to deal with individual cases, to elaborate the details of treatment, and to explain to his pupils the principles upon which his proceedings have been grounded. We can hardly imagine a position better calculated than this to enable a man to write with weight and authority, both upon the speculative questions of science and the practical application of surgery.

But Dr. Sédillot has not only addressed himself to the task of bringing out a complete work on operative surgery; he has also from time to time published monographs on a variety of subjects connected with pathology, and the results of these his earlier labours he has incorporated in the volumes before us, so that we have a guarantee, if a guarantee were needed, that his opinions upon such difficult and obscure subjects as cancer, pyæmia, &c., have not been hastily formed, but are the result of careful and special study.

The present work is a treatise on operative medicine, or, as we should say in this country, on practical surgery. With medicine, properly so called, it has nothing to do; and the space which it devotes to the science of surgery is very small. Like similar books in our own language, it is almost entirely taken up with describing the symptoms of surgical diseases, and

explaining the treatment applicable to each. Indeed, the domain of general surgery—to say nothing of the special branches—is now so extensive that it has to be subdivided into various parts in order that we may study it conveniently. No single work can adequately treat both the science and the art of surgery. One writer occupies himself with the etiology and pathology of surgical diseases, while another confines himself to an exposition of their treatment. It is this latter branch of the subject which Dr. Sédillot has undertaken to explain in his present work; and he deserves no small credit for the complete and systematic way in which he has accomplished his task. He has begun by setting forth the simplest proceedings of minor surgery, and has advanced to the most difficult and dangerous of the capital operations. Everything, great and small, which falls within the practice of an operating surgeon has found a place in the pages of this work. It seems to us to be as nearly exhaustive as any book on such a vast and ever-increasing subject as surgery can be. Not that every topic is elaborated in all its details,—that were almost impossible,—but the reader is supplied with all the information which, in the light of modern science and after an ample experience, appears to the author to be of practical importance.

The arrangement which Dr. Sédillot has adopted is this:—After setting forth briefly the symptoms of the disease under consideration, he enumerates the various methods of treatment which have any claim upon our attention; and then he gives an *appréciation*, or estimate, of the value of each. In some instances we have observed that he mentions as many as fifteen or twenty different modes of treatment; but they are all described shortly and clearly, so that they do not weary or confuse the reader, and the *appréciation* helps him to determine which is the most applicable to any particular case. In many instances the author proposes methods or modifications of his own, and shows a good deal of originality in adapting the means to the end.

In his preface Dr. Sédillot laughs at the *pruderie anglaise* which objects to the practice of vivisection, and says that, while we allow our soldiers and sailors to be flogged with the cat-o'-nine-tails, we cannot permit the sacrifice of a few animals for the purposes of scientific inquiry. But the analogy is a bad one, and will not bear a close examination. The cat-o'-nine-tails is used only under necessity, and when the case requires a severe and degrading punishment. And in like manner when any good purpose is to be served by vivisection, no one would say a word against it. We are not aware that any reasonable Englishman objects to experiments being made upon animals for the establishment of scientific truth. On the contrary, such experiments

have frequently been made in this country, and are made at the present day. But what we object to is the repetition of these experiments, after the facts have been ascertained and the truth established, merely for the satisfaction of a class of pupils, who ought to be contented to take the statement upon the authority of their teacher.

It would be impossible for us to follow Dr. Sédillot through the multitude of subjects which he has handled in this treatise. The field of surgery is too vast for us to traverse all its paths and bye-paths with him. We must be contented to speak of his labours in general terms; and we are the more disposed to do this because the opinion which we have formed of them is far from unfavorable. In a few cases we should find ourselves at variance with him, but in the great majority of subjects we believe we could entirely endorse the pathological views which he holds and the methods of treatment which he recommends. Indeed, it is most satisfactory to find that the practice which prevails in England is so much in harmony with that which prevails in France. In the present day, every opinion, every statement, every theory is subjected to the most severe and searching criticism; and when we find the practice of various countries, and those the most highly civilised, in substantial agreement, we may venture to hope that we have arrived at something like certainty.

Dr. Sédillot makes some very sound and useful introductory remarks upon operative surgery in general, upon the preparation of the patient, and upon the after-treatment of the case. His object is to enforce attention to the minutest details—to urge upon surgeons that they ought not to consider themselves above studying the most trifling points; and in this recommendation we entirely agree with him. It seems highly probable that it is this which makes a great part of the discrepancy which we find in the statistics of capital operations in the hands of different surgeons. One elaborates the details both of the operation and of the treatment; another looks only at the leading features of the case, and leaves the rest to nature. And thus it happens that the careful attention and minute study which one or another recommends is often overlooked in the performance of a brilliant operation; and when this occurs, it is much to the disadvantage of the patient, and to the discredit of surgery.

In speaking of chloroform, Dr. Sédillot lays down a proposition with which we are hardly prepared to agree. After giving various cautions with regard to the use of anæsthetics, he says, "*Le chloroforme pur et bien employé ne tue jamais.*" Now, it seems to us very unwise in the present state of our knowledge to lay down such a dictum as this. The subject of death from

chloroform is very obscure and intricate. It is far from being thoroughly understoood; and we are constrained to admit that deaths have occurred for which we could assign no good cause. Even when the purest chloroform has been administered with every precaution, fatal accidents have sometimes happened; and, if such is the case, Dr. Sédillot's statement is calculated to do much harm. To say that pure chloroform properly administered never kills, is a doctrine which is likely to lead to a careless and negligent use of anæsthetics, and to increase the evil which we all deplore. Some day we may, perhaps, have an anæsthetic which may be used without fear; but at present it seems to us both the safest and the most scientific plan to point out the known sources of danger, and to lay down rules for the careful administration of chloroform.

Dr. Sédillot is tolerably familiar with English surgery and with our professional literature; and yet there are some omissions in his work which excite our surprise. For example, twenty-five proceedings are enumerated which have at different times been adopted for the radical cure of hernia, but no mention is made of Mr. John Wood's operation, which has attracted so much attention in this country during the last few years. And yet Mr. Wood's operation is the more deserving of notice, because it proceeds upon a different principle from all the others, and because—to say the least of it—it is fully as successful as any operation which has been undertaken for the same object. Again, many ways of dealing with old and obstinate strictures of the urethra are mentioned, but not a word is said about Mr. Holt's modification of M. Perrève's dilator, although this is an instrument which is held in high estimation in this country, and is preferred as a means of rapid dilatation to almost every other. Again, the amputation by a long rectangular flap taken from the extensor side of the limb (Teale's amputation) is not alluded to, though the author speaks of the inconveniences which this operation is intended to meet. These are some of the points which we have observed in which Dr. Sédillot's work fails to give an adequate view of those surgical measures which are most esteemed in this country, and other examples of a similar kind might be adduced, but the ones which we have quoted are sufficient for our purpose.

Dr. Sédillot enters at some length into both the diseases peculiar to women and the special surgery of the eye. Both of these subjects have made so much progress of late years, that they can hardly be adequately treated in a work like this. Nevertheless, it is important that they should not be overlooked altogether—it is important that their relation to general surgery should not be forgotten; and Dr. Sédillot deserves credit

for the amount of useful and practical information upon these subjects which he has given in a short space.

Dr. Sédillot's book is profusely illustrated, and some of the woodcuts in which the vessels coloured by a process of chromo-lithography are extremely good. They are the first of the kind that we have seen, but they are so excellent—so sharp and clear and distinct, that we are inclined to think this style of coloured engraving will come into use in many of our medical and surgical works. There are, however, some illustrations in the treatise before us to which we cannot accord the same measure of praise. They are those which represent the operator and his assistants as they stand round the patient in the act of performing an amputation or a lithotomy. They are so ridiculous, that they are calculated to do more harm than good; and, as they are not at all necessary to the elucidation of the subject, we would advise Dr. Sédillot to omit them altogether in future editions of his work.

Review VI.

Guy's Hospital Reports. Third Series, Vol. XI, edited by Samuel Wilks, M.D.; and Vol. XII, edited by C. Hilton Fagge, M.D., and Arthur E. Durham.

The first of these volumes contains twenty papers, of which we give the following analysis:

I. *On the Value of the Study of Morbid Anatomy.* By Samuel Wilks, M.D.—In this paper Dr. Wilks upholds the importance of morbid anatomy, in opposition to those who maintain that an excessive cultivation of this branch of medical science is inconsistent with enlarged ideas of general pathology, and that the continual examination and record of details tend to obscure the view of broad principles. Dr. Wilks argues that, so far from morbid anatomy merely consisting of a multitude of minute distinctions, it actually simplifies the history of disease, and establishes the existence of certain leading conditions, of which the nosological divisions adopted by pathologists are only varieties. Thus, for instance, Bright's disease presents on the post-mortem table a degeneration of the kidney, which may be accompanied by changes in the heart and arteries, or derangements in other organs, as the lungs; but in the wards of the hospital this same morbid condition may be classed as albuminuria, dropsy, heart-disease, apoplexy, chronic bronchitis,

epilepsy, laryngitis, &c.; and an apparently local disease, such as hydrocephalus, may really derive its origin from general tuberculosis, to be discovered only after death. Again, in a nosological arrangement, ascites, cancer of the liver, cancer of the stomach, and several other affections of the abdomen, may appear as distinct affections, but they may in reality be all due to a single diathetic disease, which accidentally fixes upon certain portions of the abdominal viscera. It is clear, therefore, according to Dr. Wilks, that the morbid anatomist is less prone to localise disease than the physician who regards it only in the living body, and that the result of his study would not be to complicate or extend nosological nomenclature, but to generalise and simplify it. Dr. Wilks believes thoroughly in the existence of certain diatheses, and, together with them, predispositions to particular diseases, and he altogether repudiates the opinion that local affections, such as inflammations, may arise in healthy bodies. The diathesis may be congenital, as in gout and scrofula, or it may be produced from a definite cause, as from the introduction of syphilis into the system, whereby the whole constitution is changed. Another opinion expressed by Dr. Wilks is, that particular tissues undergo only a very limited number of morbid changes, and that if other diseases are found in them they are brought from a distance. Thus, when abscess occurs in the lung, the affection is not idiopathic, but the blood is diseased, and the source of contamination will be found in some other part of the body; and when suppuration of the kidney takes place the source of the affection must be sought in the bladder or elsewhere, as idiopathic nephritis or Bright's disease never ends in abscess. The same view may probably be taken of abscess of the liver and of the brain. Cancer, again, does not originate spontaneously in the lung, but is brought from the abdomen or the exterior of the body, and in so-called cancer of the kidney the disease really exists in a very large number of cases outside the organ instead of in its texture; and Dr. Wilks applies the same reasoning to cancerous affections of the liver, which he thinks are derived from the stomach, or from Glisson's capsule about the vessels or hepatic duct.

II. *Additional Cases of Supra-Renal Disease, with Remarks.* By Samuel Wilks, M.D.—In a former paper, published in the 'Reports,' Dr. Wilks recorded all the cases of Addison's disease which had come under his notice, the series corresponding to the specimens in the museum of Guy's Hospital. His object then was to show the peculiarity of the morbid changes in the supra-renal capsules; and that it was of a much more definite character than Addison himself at first supposed. Since the

publication of these cases several additional specimens have been placed in the museum, and are identical in character, the whole constituting a series of thirty-three cases. Dr. Wilks strongly insists upon the fact of the peculiarity of the disease, because its want of recognition as a specific morbid condition has operated more than any other circumstance to retard the progress of correct ideas as to the value of Dr. Addison's discovery. The original work of Dr. Addison contained several cases, but only five of them were true instances of the disease which he described, and the author himself was aware that such was the fact before his death; but other writers have taken the whole of the cases as belonging to the category of Addison's disease, and have either necessarily fallen into error or have doubted the reality of the disease altogether.

But Dr. Wilks, after some years' attention to the subject, repeats with much confidence that the disease of the capsules in Addison's disease is uniform and peculiar, that none other than this has yet been found in connection with the symptoms described by Addison, and none other which has been satisfactorily proved to be the sole cause of death. In all the thirty-three cases in the museum of Guy's Hospital the disease is of the same nature, and the same may be stated of all the cases which have come under Dr. Wilks's notice at the Pathological Society and elsewhere. The peculiar disease, which Addison and others have called scrofulous, consists in the conversion of the capsule into an amorphous, yellow, cheesy substance, the material being sometimes softened into an almost fluid mass, and in others becoming cretaceous. This so-called scrofulous matter, however, is due to the degeneration of a previously effused material, consisting of a translucent, tough, fibrous tissue. Dr. Wilks thinks that in the well-marked cases of Addison's disease the capsules are irretrievably destroyed as soon as the symptoms are observed, and that then the malady has previously existed for several years. Such would not be the case if the disease were cancerous, as cancer could not exist for many months without destroying life. Dr. Addison himself, however, included cancerous affections among his cases, although he subsequently knew that he had committed an error in this respect. Professor Mattei regards the symptoms in Addison's disease as being due to the implication of the sympathetic system of nerves, and this explanation was given by Dr. Addison himself. Dr. Wilks has always believed that the vaso-motor nerves were implicated, for the symptoms correspond with those observed when those centres are evidently involved, and, moreover, it is clear that the symptoms are not due to the destruction of the capsules, inasmuch as these organs are totally destroyed for

months or years before the death of the patient. The experiments of Brown-Séquard and Mattei have shown that by crushing the supra-renal capsules in rabbits the same results ensue as when the sympathetic ganglia are experimented upon. These capsules receive so many nerves that they have been regarded by some authors as a kind of nervous ganglia, and the existence of nerve-cells in their interior appeared to prove the truth of this view, but the researches of Nagel, Bardeleben, Ecker and Frey, show that the capsules are really glands. Kölliker thinks that the two substances which compose these organs possess different functions, and that while the cortical substance resembles a vascular gland, the medullary substance, from its richness in nerves, may be regarded as an apparatus forming part of the nervous system.

Dr. Wilks concludes his paper by describing in detail his eight additional cases, which are all numbered in correspondence with the morbid preparations in the museum of Guy's. They are all well-marked instances of Addison's disease, the symptoms during life being chiefly the bronzed skin and great depression of the system, and the post-mortem appearances being those just alluded to and described by Dr. Wilks as characteristic of the affection.

III. *Cases of Enlarged Spleen, with remarks on the Operation for its Removal.* By Samuel Wilks, M.D.—Dr. Wilks first relates two cases of enlarged spleen, both being fatal. In the first case there was no assignable cause for the disease, and after death there was nothing discovered to account for the fatal result except the hypertrophied spleen itself; in the second case there was no previous history, but post-mortem examination revealed universal adhesions of the intestines from old peritonitis.

Dr. Wilks thinks it clear that death in such cases results from an impoverished condition of the blood, induced by the state of the spleen. Experience also shows that medicines are wholly inefficacious in reducing the size of the organ, quinine being quite inert except in diminishing a temporary enlargement under a miasmatic influence; and absorbent remedies, such as iodide or bromide of potassium, which tend to remove adventitious products of a low organizable character, being useless in their action on a healthy tissue, unless given in poisonous doses. As there is consequently no possibility of reducing the size of an hypertrophied spleen by ordinary remedies, it becomes a question whether the removal of the organ is a justifiable operation, and whether there are sufficiently good reasons to expect it to be successful. It is well known that the spleen has often been

removed with impunity in the lower animals, and Dr. Crisp quotes two cases, in one of which a dog lived six and a half years after the extirpation, no trace of the organ being found after death; and in the other it was found that it was reproduced, the dog being killed two years after the operation. It should be mentioned that the entire reproduction of the spleen, when the whole of it has been removed, is not credited, but it is supposed that a portion may have been left as a nucleus on which subsequent growth may have taken place. Besides the experimental removal of the spleen in animals, it is recorded by Morgagni in one case that the spleen was torn out by accident in the human subject, the patient surviving the removal for five years, during which period she became pregnant and bore a child. Cases of a similar kind are met with in medical works; and for these reasons and others to which he refers, Dr. Wilks considers that if the enlarged spleen could be removed with facility, the patient might recover the health which was lost through the excessive action of the function of the organ. He then quotes the history of some cases in which the operation of extirpating the spleen was performed on the human subject. One objection to the operation is, that in cases of enlarged spleen the blood is in a diseased state, and therefore the patient is in a bad condition, besides there being a tendency to hæmorrhage. On the whole, Dr. Wilks thinks the operation is a feasible one, the hypertrophy itself being a fatal disease; but the patient should himself wish for the operation, and be made acquainted with its dangers.

IV. *Report on Lardaceous Disease.* By Samuel Wilks, M.D.—The opinion advanced by Virchow, that the adventitious material in lardaceous disease is amyloïd in character, is adopted by few English observers, and it is more usually regarded as a peculiar substance of an albuminoïd nature, and more allied to cholesterine than any other hitherto recognised proximate principle. In a paper published in 1856 Dr. Wilks reported thirty-six cases of lardaceous disease, and he now offers a further list of sixty cases. The whole series proves indisputably that the disease is due to a long-standing and deep-seated cachexia; it is seen in its most intense form after protracted caries and necrosis of bone, having their origin in tuberculosis or syphilis. If no affection of the bones be present the lardaceous disease is associated with tubercle in other parts of the body. The term *amyloïd degeneration* is never used by Dr. Wilks, as he believes both words are incorrect in their application, the amyloïd nature of the disease having not yet been proved, and the word *degeneration* being unsuitable to a case

where new material is deposited in a healthy part, and superadded to the existing structure, and not substituted for it. Still, the lardaceous affection leads finally to the destruction of the healthy elements of the liver and kidney, or at least prevents the due performance of their functions. In the sixty cases now related by Dr. Wilks two thirds were connected with diseased liver or syphilis, and of the remaining third the majority were tubercular, there being only five in which there was no history of syphilis, tubercle, or disease of bone.

V. *Cases of Enlargement of the Lymphatic Glands and Spleen* (Dr. Hodgkin's Disease), *with remarks.* By Samuel Wilks, M.D.—In this paper Dr. Wilks refers to a disease where the lymphatic glands are increased in size and associated with a deposit of a morbid kind in the internal viscera, more especially in the spleen. This affection was described originally by Dr. Hodgkin, and afterwards by Dr. Wilks, who, however, mentioned it in connection with lardaceous disease, and thus, perhaps, caused the two diseases to be confounded. Hodgkin's disease is mainly characterised by an enormous enlargement of the lymphatic glands throughout the body, along with a peculiar white deposit in the spleen; but Dr. Wilks observes that of the six cases described by Hodgkin four only are examples of the disease, one being doubtful and the other being a syphilitic affection. The exact nature of the adventitious material causing the glandular enlargement in Hodgkin's disease forms an interesting subject of inquiry. The lymphatic glands are large, firm, and translucent, the microscope showing an abundance of cells scarcely distinguishable from the natural secreting bodies, and with more or less fibre-tissue. In the liver the material is much tougher and fibro-nucleated, and in the lungs, spleen, and kidneys, it is composed mostly of cells somewhat resembling those of tubercle. Dr. Wilks considers that Hodgkin's disease is clearly separable from lardaceous disease, and from cancer and tubercle, although all these affections may bear a relation to one another; and that it quite deserves a special name.

VI. *Clinical Report on the Diseases of the Testicle.* By Thomas Bryant.—This report consists of a brief description of the diseases of the testicle and its covering, together with a series of illustrative cases. The first affection described is hydrocele, which is divided into the *vaginal* and the *encysted.* The former is stated to be generally due to some inflammatory affection of the serous membrane covering the testicle, as is shown by the flocculi of lymph sometimes seen floating in the secretion, by the occasional thickening of the tunica vaginalis,

and by the presence of membranous bands and septa in the cavity of the serous sac. But in a small class of cases it would appear that the exudation of fluid is of a passive nature, and is merely an excess of the natural secretion of the serous membrane, and such appears to be the case in the congenital form of hydrocele. After describing the symptoms and pathology of the disease, Mr. Bryant proceeds to the consideration of the treatment, indicating the cases where the palliative treatment should alone be adopted, and those where the radical cure should be tried. For the latter purpose he prefers the injection of a solution of iodine, he himself employing equal parts of the tincture and of water. Encysted hydrocele differs from the ordinary form of the disease by the slowness of its growth, its globular shape, the anterior position of the testicle, and the nature of the fluid, which always contains some granules in suspension and frequently spermatozoa. Hæmatocele, like hydrocele, is divided by Mr. Bryant into *vaginal* and *encysted*. The causes of hæmatocele, as is well known, are generally blows or injuries, but sometimes the disease arises spontaneously, in which case it is rather difficult to explain the origin of the blood. In certain cases Mr. Bryant recommends that the fluid contents should be drawn off by a trocar and canula, and, if symptoms of suppuration or softening of the coagula should appear, then a free incision is the only sound practice. Under the head of "Inflammation of the Testicle" Mr. Bryant considers three forms of disease, namely, *testitis*, or inflammation of the testicle and epididymis, and *orchitis* and *epididymitis*, respectively signifying inflammation of the organs from which the names are derived. Epididymitis may be distinguished by its boat- or truncated half-moon shape, appearing to hold the body of the testicle in its concavity. Under the head of "Orchitis" Mr. Bryant considers the simple and the syphilitic forms, the latter being distinguished by its tendency to deposit a product which rapidly undergoes fibrous changes. In the treatment of chronic orchitis, whether simple or syphilitic, Mr. Bryant relies chiefly on pressure and mercurials, but in the gouty form the treatment must be modified according to the special peculiarity of the patient. In cachectic subjects, moreover, where mercurials may be contra-indicated, the iodide of potassium may be given with the syrup of the iodide of iron. Tubercular disease of the testicle must be treated on general principles, with total abstinence from sexual intercourse. Hernia testis, which was treated by castration in former times, may be cured by less severe measures, as by the restoration of the testicle to its natural place by pressure, and the application of the red oxide of mercury to the granulations when they are very exuberant. Of cystic disease

of the testicle, on the nature of which there exists much difference of opinion, Mr. Bryant gives a brief description, and recommends excision as the only treatment. Cancer of the testicle can be treated also only by excision, and this should be performed as early as the diagnosis can be made. Mr. Bryant concludes his paper by a brief recapitulation of the points to be observed in the diagnosis of a scrotal tumour, and by a table of the diagnostic characters of chronic disease of the testicles.

VII. *The Stereoscope and Stereoscopic Results.* By Joseph Towne.—This paper is one of a series on the same subject by the same writer, but it is impossible to present an abstract of it, as the facts and reasonings can only be explained by reference to the diagrams with which the paper is accompanied. It contains, however, an elaborate and very able explanation of a number of experiments made by Mr. Towne on binocular vision.

VIII. *On Cystic or Hydatiniform Disease of the Chorion.* By J. Braxton Hicks, M.D., F.R.S.—In this short paper, which is accompanied by two plates, Dr. Hicks describes and illustrates a case of hydatiniform disease of the chorion, where the microscopic appearances seemed to throw some light upon the real nature of this affection. The point still at issue in reference to its pathology is, whether the morbid growth is an instance of the class called the "proliferous cyst," or whether the production of the so-called cysts of the chorion is merely a change in parts already formed or forming at the time when the change commenced in the large cysts, to which the secondary cysts were attached. Dr. Hicks adopts the latter view, and he considers that the structures exhibited by the microscope in the case particularly described confirm his opinion. The specimen was expelled from a woman at the full period of utero-gestation, although there was no fœtus, and the patient had thought herself normally pregnant. The solid mass was composed principally of the ovular membranes and villi, with some deciduous portion, a small amnial sac, but no trace of the fœtus.

IX. *Several Cases of Abdominal Tumour.* By S. O. Habershon, M.D.—These cases are five in number, and illustrate various forms of abdominal tumour most of them, however, being cancerous. The diagnosis in such cases is often obscure, but Dr. Habershon points to several facts which ought to be ascertained as guides in the inquiry, such as the position of the tumour, the number and size of the growths, the nature of their

surface—whether smooth, rough, or nodular; their consistence —whether solid, fluid, or gaseous; their mobility—whether fixed, movable, or pulsatile; their sensibility as to pain, with or without pressure, and the presence or absence of functional derangement. All the cases related by Dr. Habershon were fatal, but the record of the symptoms during life compared with the post-mortem examinations throws considerable light upon the diagnosis, and even affords the opportunity for suggestions as to treatment. In the first case, which was that of a man of sixty-five, the disease began in the appendix cæci, and set up inflammation in the adjoining tissues, followed by suppuration and abscess, which latter opened under Poupart's ligament, but eventually the disease extended above the diaphragm, producing empyema, and causing death by acute pleuro-pneumonia. Dr. Habershon thinks that if the patient had been young and strong the inflammatory mischief might have been arrested and that only adhesion and thickening might have resulted; and the treatment best adapted to the case would have been the use of tonics, opiates, and as generous a diet as the patient could bear.

X. *Some Remarks on Diseases of the Skin.* By S. O. Habershon, M.D.—This paper contains some general remarks on the classification of skin diseases and their arrangement into groups by different authors, but it is chiefly devoted to a brief epitome and analysis of the cases of cutaneous affection which have been treated at Guy's Hospital during a year. More than a thousand such cases presented themselves, but the exanthems of scarlet fever, measles, smallpox, &c., were not found among the out-patients, as they are excluded by the rules of the hospital; and many of the milder forms of disease were also absent. The arrangement preferred by Dr. Habershon is the natural one, dividing the diseases as they affect particular structures of the skin, or as they depend upon the presence of animal or vegetable parasites, or as they are connected with the scrofulous or syphilitic cachexia, &c. Dr. Habershon concurs with Dr. M'Call Anderson in regarding eczema as comprising several diseases formerly considered distinct, as lichen, impetigo and pityriasis, and he therefore believes that they ought all to be treated on similar principles. He does not agree with Mr. B. Squire that pediculi exist in nearly every case of prurigo, but he has often found them in that disease, even when their presence had not been suspected. Of tinea favosa only one case presented itself. Besides constitutional predisposition as leading to skin diseases, Dr. Habershon attributes the existence of many of these cases among the poor to erroneous modes adopted in feeding children, and to the use of

yellow soap, which is often found to dissolve the skin and produce severe eczematous eruptions.

XI. *The Ventilation and Warming of the New Wards, Hunt's House, Guy's Hospital.* By JOHN CHARLES STEELE, M.D.—The object of the writer of this paper is to describe the mechanical arrangements adopted for ventilating and warming a part of Guy's Hospital erected some thirteen years ago from a sum of money bequeathed by Mr. Thomas Hunt, and also to communicate the results in reference to the comfort of the patients. The paper is accompanied by diagrams, without which it would be impossible to make the arrangements intelligible. We may state, however, as a matter of general interest, that the attempt made to warm and ventilate some of the wards by means of the apparatus and without the use of open fire-places appears not to have been successful, and several reasons are given for this result, the chief of them being the uncertainty of the English climate and its frequent changes, so that the requisite amount of warmth or of coolness, as occasion may require, cannot be obtained by any self-acting machinery. The effects of the apparatus being uniform, the wards are sometimes made too warm and sometimes too cold in relation to the temperature of the external air and the feelings of the patients, and the only means of obviating this inconvenience is by a judicious management of the ward fires and of the windows. Still, Dr. Steele writes in commendation of the general arrangements for warming and ventilation adopted in the buildings, although they do not supersede the use of open fire-places and the adoption of the ordinary modes of admitting air.

XII. *On Vesico- and Recto-vaginal Fistulæ and Ruptured Perinæum, &c.* By THOMAS BRYANT.—This paper is divided into two chapters, the first being on vesico-vaginal and recto-vaginal fistulæ, and the second on the operation for the relief of a lacerated perinæum and sphincter ani, with some of its complications. The different steps of the operations for the relief of these affections are described and are illustrated by engravings, and Mr. Bryant adduces several cases to prove the efficacy of judicious surgical treatment. The instruments recommended by Mr. Bryant for the operation for vesico-vaginal and recto-vaginal fistulæ are constructed by himself, but are modifications of an instrument devised by Mr. Hillyard, of Glasgow. They are pronged instruments for seizing the tissues between the rectum and the vagina, the scalpel being used to pare the edges and make a clean and raw surface. Mr. Bryant's cases appear to have been very successful.

XIII. *Cases and Observations on Medical Jurisprudence.* By Alfred S. Taylor, M.D., F.R.S.—These are miscellaneous cases which have been collected together by Dr. Taylor, as illustrating some interesting points in forensic medicine. The first case illustrates the effects of poisoning by vermin-powder, a compound of white arsenic, flour, and sugar, the patients being three children, who swallowed the poison in consequence of its sweet taste, but who all recovered under medical treatment, which consisted in the administration of castor-oil with milk and barley-water. Dr. Taylor strongly condemns the practice which, it appears, prevails in some country districts, of keeping this vermin-powder in grocers' shops, without labels and without any colouring matter being added to it to distinguish its nature. In the case recorded, the vermin-powder appears to have fallen accidentally into a basket of groceries sent home to the parents of the children.—The second case is one of death from perforation of the jejunum as a result of disease; but as the patient died very soon after he had been wrestling with a companion, it was at first suspected that violence might have caused the fatal result. A post-mortem examination, however, revealed the real nature of the case, and a verdict of "accidental death" was returned by a coroner's jury.—The next case is one of death from perforation of the colon, and in this instance also the result was at first attributed to violence, as the patient, being engaged in a drunken quarrel, had been thrown down by one of his companions. Here also, however, a post-mortem examination detected the disease, which, it afterwards appeared, had existed for some time.—Then follows a case of accidental strangulation, a boy who was working at a factory having had his neck-tie seized by the hand of an engine, and having his throat thus tightly compressed, so as to prevent the admission of air to his lungs for at least one minute. He recovered, but the case seems to show, from the statement of the patient, that such strangulation is not attended by pain, and it confirms the general medico-legal opinion that complete occlusion of the trachea is attended with immediate insensibility.—In the next case there was an accidental laceration of the kidney and fracture of the ribs, but without any marks of external violence, and the patient walked after the accident, though he died soon afterwards. The fracture of the ribs was detected during life, but not the laceration of the kidney.—In another case the recovery of a child from a dose of twelve drachms of laudanum is related. The poison was swallowed by mistake, and the treatment consisted, after the trial of the usual remedies, in the administration of belladonna. It appears that *sixteen grains* of the extract of belladonna were

given in divided doses, and with a successful result, although the condition of the patient at one time was very precarious. It is remarkable that no symptoms of poisoning by belladonna were produced.—The next case is one of recovery from a large dose of extract of belladonna injected into the rectum. Strangely enough, an injection containing one drachm of extract of belladonna and one drachm of wine of opium, in a dilute solution of sub-acetate of lead, had been injected into the rectum of a lady by her husband, under the advice of a medical man. Violent symptoms of poisoning were produced, those of belladonna poisoning being the most prominent; but the patient eventually recovered, the treatment consisting in washing out the rectum, and thus removing the remains of the poison. As a curious sequel to the case, it is related that after the lady's recovery the medical man who had ordered the injection recommended that it should be repeated, but this advice was not followed.—The next case shows the effects of a large dose of datura stramonium, one of Dr. Taylor's pupils having tried the effect of this poison on himself, by way of experiment. The serious effects lasted eight hours. The effects of atropia and daturia are then described, together with the difference in their operation; daturia, among other points of difference, being twice as strong as atropia.—In the last case in the paper Dr. Taylor discusses the question as to what is the fatal dose of strychnia, and it appears that the effects vary in different cases; medicinal doses, such as one fifteenth of a grain, sometimes producing very violent tetanic convulsions. It would appear, however, that while the medicinal dose of pure strychnia should not exceed the eighth of a grain, a poisonous dose commences at about one fourth of a grain.

XIV. *Clinical Lectures delivered at Guy's Hospital during the Winter Session* 1864-5. By John Hilton, F.R.S.—The six lectures forming this series were delivered at occasional intervals, and are illustrated by cases occurring in the hospital. Mr. Hilton's plan of delivering a clinical lecture is to take a single case, and to make it the starting-point of practical observations on some special disease or accident. Thus, the first lecture is on the treatment of fractured clavicle; the second on concussion of the brain, with depression of bone; the third on strangulated hernia; the fourth on irreducible inguinal hernia; the fifth on senile gangrene; and the sixth on fracture of the spine; and all these subjects were suggested by cases as they presented themselves in the wards.

XV. *Case of Ruptured Popliteal Artery.* By Alfred

POLAND.—The case here recorded, and of which a coloured engraving is presented, was that of a farmer's boy, who, without any previous symptoms, suddenly felt a numb sensation in his foot while walking, and soon afterwards perceived a swelling at the back of his knee. Mr. Poland detected the nature of the case, and endeavoured to effect a cure by position and the employment of constitutional remedies; but as gangrene at last threatened to attack the calf of the leg, amputation was performed, and with success, for the stump healed well and the boy recovered. The examination of the amputated limb revealed the nature of the injury, which was a rupture of the popliteal artery and extravasation of blood under the muscles of the leg as far as near the ankle, but only in thin detached patches, not enough to produce any pressure.

XVI. *Poisoning with Fungi; fatal effects of the Amanita Citrina on a Woman and Child.* By ALFRED S. TAYLOR, M.D., F.R.S.—A woman living near Ipswich gathered some plants which she thought were mushrooms, and fried them, and afterwards ate them and gave portions to her children. All three were soon taken violently ill, and one child, aged six, died the next day, and the woman herself died five days after eating the fungi; the other child recovered. Dr. Taylor examined some of the fungi gathered from the spot from which the poisonous specimens had been taken, and considered them to belong to the species described as the *Amanita citrina*. The pileus was of a dirty yellow colour, and the whole fungus had a very offensive smell, resembling that of putrescent animal matter. These noxious fungi are wholly unlike the ordinary edible mushroom (the *Agaricus campestris*), and the woman showed great ignorance in gathering them. The poison of this fungus, whatever be its nature, is proved by Orfila to be of a most virulent kind, and is fatal to the lower animals as well as to man. Its action was of a mixed character, being at first that of an irritant, producing vomiting and purging, but in the last stage it affected the brain and spinal cord, producing stupor and insensibility, and in the child, convulsions. The cooking of the fungi did not destroy the poison, although they were exposed to a high temperature in frying. Many experiments made by Orfila on dogs prove the extremely poisonous nature of this fungus. In the case of the child who forms the subject of Dr. Taylor's paper, the pupils were observed to be dilated, this symptom and several others being similar to those produced by belladonna. Dr. Taylor remarks that, as a rule, poisonous fungi may be recognised by the pileus or cap having a peculiar conical or flattened shape, and being coloured yellow, brown,

green, red, or orange-red, and by a very disagreeable odour when the fungus is bruised. These poisonous kinds generally grow under trees or in groves and woods, and are often seen attached to decayed vegetable matter. The *Agaricus campestris*, or wholesome mushroom, has a white silky pileus, at first convex, and afterwards plano-convex; the gills are at first of a pale brick colour, but afterwards become mottled and of a brownish-purple colour. It is remarked, however, that even the ordinary mushroom, either from some peculiarity in its place of growth, or in the mode of cooking it, or from its indigestibility, or from idiosyncrasy in those who eat it, occasionally produces pain in the stomach, with nausea, vomiting, and purging. There is uncertainty and even danger in using any mushrooms as articles of food unless they have been cultivated, and Richard, the eminent botanist, although he was well acquainted with the distinctions of fungi, would never eat any except such as had been raised in mushroom-beds in gardens.

XVII. *Cases of Rheumatic Fever, treated for the most part by Mint-water.* Collected from the clinical books of Dr. Gull, with some remarks on the Natural History of that Disease by Henry G. Sutton, M.D. Lond.

XVIII. *Four Cases of Rheumatic Fever, treated by Mint-water only, with Remarks.* By Dr. G. Owen Rees.—The object of both these papers is to show that rheumatic fever will often subside under very simple treatment. Dr. Gull has remarked that his cases cannot be considered to have been treated solely on the expectant plan, for an occasional dose of Dover's powder, or half a grain of opium, night and morning, with two or three ounces of brandy a day, are remedies which may have had some slight influence on the course of the disease. One of his chief objects in instituting the investigations was to endeavour to gain a more accurate knowledge of what may be termed the natural history of the disease, and it must not be forgotten that rest in bed, a well-regulated diet, and good nursing, are powerful elements in the treatment of any acute disease, experience having proved that many cases of continued fever and pneumonia have recovered under such treatment. Dr. Gull does not, however, consider that rheumatic fever ought to be allowed to run its own course, but he regards the profuse acid perspirations, the acid saliva, the highly acid urine, and the other acute symptoms, as favorable indications, and he thinks there is evidence to show, from the result of his cases, that the disease tends unaided to get well. A perusal of the cases, says Dr. Gull, goes to show that the best treatment for rheumatic fever has still to be determined, and he thinks it absolutely necessary

to understand the natural progress of the disease before any conclusion can be drawn concerning the operation of remedies. The cases show that too much importance has been attached to the use of medicines, especially in those acute cases where the tendency to a natural cure is the greatest.

Dr. Owen Rees treated four cases of rheumatic fever with mint-water only, and with success; but he remarks that he has been in the habit for many years of employing lemon-juice in this disease, and from long experience he comes to the conclusion that two of the patients whose cases are recorded in the paper might have been earlier relieved if that remedy had been administered. He thinks that lemon-juice diminishes the general excitement and lowers the pulse, but the present cases show that acute rheumatism is a disease which seems to go on quite unassisted to a cure, and that this happens even when it appears in the most acute form. His cases were treated absolutely with nothing but mint-water, and the results have firmly impressed him with the belief that the old plan of treatment by calomel, opium, and antimony, and the free use of colchicum, did much harm, not only by retarding recovery, but by leaving great subsequent debility. He also states that the duration of cases of acute rheumatism in Guy's Hospital is much shorter now than when he was a student in that institution, and he attributes the improvement to the less heroic treatment now adopted by the physicians.

XIX. *Select Clinical Reports.* By G. H. Barlow. M.A., M.D. *Cardial Obstruction, the result of Intra-uterine Disease.*—Many years ago Dr. Barlow called attention to certain disorders of the thoracic viscera commencing in early youth, and consisting, in the first place, of a defect in the relative proportions of the lungs and the chambers of the heart; in most cases the first morbid symptoms seemed to be referable to defective expansion or development of the lungs themselves. Sometimes the defective expansion was caused by antecedent disease, but in other cases it was questionable whether the defective expansion of the lungs was the primary cause or whether the defective development might not arise from a tendency to tubercle. In others, again, the primary lesion seemed to be a narrowness of the trachea, diminishing the volume of air inhaled at each inspiration, the narrowness being obviously the effect of pressure of distorted vertebræ or some similar cause. But it was also a question whether the narrowness of the trachea was the cause of the non-development of the lung, or the non-development of the lung the cause of the narrowness of the trachea. Certain propositions, however, indicating the relation of the

parts presenting these abnormal conditions appeared to be pretty well established, and were tabulated by Dr. Barlow; and the case he now adduces affords an instance of a form of disease similar to those he formerly described, and appears to have originated, if not before birth, certainly very soon afterwards. The patient was a youth of twenty-one, and his mother had suffered from some fever shortly before his birth. He was observed to be livid when he was washed as a child, and he suffered frequently from dyspnœa. A number of symptoms referable to the lungs presented themselves with increasing severity about the age of puberty; the heart, lungs, and liver became engorged, and there was dropsy, and he died at last rather suddenly from apnœa. The post-mortem examination showed that the heart was enlarged and altered in shape, and the auricles were double the normal size. The opening of the coronary vein and its valve were of great size, and there was a communication between the auricles through the foramen ovale. The right auricle was firmly adherent to the lung behind and the pericardium in front. Dr. Barlow attributes these conditions to a very early attack of pericarditis, the existence of which was proved by the presence of very old pericardial adhesions, which had even proceeded as far as ossification, and he thinks it probable that the pericarditis had been intra-uterine. In this case the mitral and tricuspid valves were healthy, but, nevertheless, from the constriction exercised by a circle of thickened membrane on the right side of the heart, the respiration was permanently embarrassed. In reference to treatment, Dr. Barlow observes that digitalis was used with good effect, and he thinks this drug serviceable in disease of the mitral valve and in those affections which resemble it, as was the case in the instance recorded.

XX. *A description of the Appearances of the Human Eye in Health and Disease, as seen by the Ophthalmoscope.* By C. Bader.—This paper, which is the first of a series, is illustrated by a coloured plate with three figures, showing, first, the optic disc and the adjoining fundus of a healthy eye; secondly, a view of a portion of the optic disc and the adjoining fundus in a hypermetropic eye; and thirdly, a portion of the optic disc, with several of the retinal blood-vessels and of the adjoining fundus, in a myopic eye. The author describes the method of making the examination of the eye by means of the ophthalmoscope, and the letter-press is an explanation of the plates.

The twelfth volume contains twenty-five papers, of which the following is an abstract:

I. *On Hydrophobia.* By J. Cooper Foster.—The writer of this paper observes that an impression prevails as to the greater prevalence of hydrophobia in the present time as compared with former periods, and that this opinion is in some degree borne out by the facts; but still the deaths from this disease are very few, and are distributed irregularly over the different years. In the first half of the year 1866 there were six deaths from hydrophobia, and nine in the whole of 1865; but in some of the preceding years there were no deaths at all from this cause. In 1838, however, there were twelve deaths, and in the latter half of 1837 there were seven deaths. In Guy's Hospital six cases of hydrophobia were reported by Dr. Bright as having been admitted between 1820 and 1830; another case occurred in 1837, but from the last-named year until 1856 no case is recorded. Mr. Foster gives the details of seven cases which have occurred since the year 1829. They were all fatal, all presented similar symptoms, the post-mortem examinations revealed similar appearances, and the treatment was exceedingly varied. In one case the medulla oblongata and spinal cord were prepared, after death, according to Mr. Lockhart Clarke's method, and carefully examined, when it was found that their sections showed extreme congestion of the grey matter both of the anterior and posterior cornua of the cord, especially in the cervical and dorsal regions. In his remarks upon the cases Mr. Foster first discusses the question as to the period which may elapse between the bite and the appearance of the symptoms of hydrophobia, the range of time having been variously stated as from one day to forty years. But experience shows that the first period is too short and the second far too long, although the poison is undoubtedly slow in its action. In thirteen cases analysed by Mr. Foster the shortest time that elapsed was four weeks, and the longest, and this happened in one case only, was from five to seven years; and in others the disease developed itself at various periods within eleven months. With regard to the situation of the original wound as affecting the probable interval at which symptoms may manifest themselves, Mr. Foster considers that when the disease appears early, the bitten part is in the most sensitive and vascular region of the body, as for instance, the lip; when the wound is in the hand the period may be longer, and in other parts, as in the knee, it may be longer still; and when the clothes have been bitten through before the skin is injured some years may pass before hydrophobia occurs. Mr. Foster remarks, however, that in cases where the disease is said to have shown itself many years after the bite the accounts given are to be received with caution, because many are probably in reality not cases of hydrophobia at

all, and the evidence of so-called cures sometimes said to have been effected rests upon no solid foundation. The treatment of the cases referred to by Mr. Foster was very varied, but, as it was uniformly unsuccessful, it hardly requires description. Mr. Foster's experience does not enable him to indorse the opinion expressed by several eminent authorities as to the value of excision of the bitten parts as a preventive of hydrophobia, for in one of his cases excision was practised and yet the disease appeared in four weeks. The bites of dogs, therefore, ought to be treated on general principles, whether the animal be mad or not, for if the patient is about to have hydrophobia nothing will prevent it, except, perhaps, amputation of the limb at a part between the bite and the body, and performed immediately after the infliction of the injury.

II. *The Teeth, as Passive Organs of Speech.* By S. James A. Salter, M.B., F.R.S.—In this paper the author describes the part played by the teeth in the formation of articulate sounds, but he also enters into the consideration of the mechanism of these sounds generally, giving what he terms a physiological alphabet, the arrangement being based upon the situation of the closure of the mouth by which the sound is produced, upon the completeness or incompleteness of the closure, and other circumstances. Mr. Salter incidentally remarks that the grammatical distinction drawn between vowels and consonants is not founded on physiological data, for certain consonants may be sounded by themselves, as S and V, if whispered; and almost all articulate sounds may be pronounced in a whisper without any vocalisation. The practical part of the paper describes the injury inflicted upon the speech by the loss of teeth, and the mode adopted by nature to compensate the injury by the adaptation of the soft parts to the purpose of articulation. When an individual has accustomed himself to the exercise of speech after the loss of teeth he is at first incommoded by their artificial restoration, but those persons will ultimately have the best articulation who repair the loss by artificial means.

III. *On the Application of Physiological Tests for certain Organic Poisons, and especially Digitaline.* By C. Hilton Fagge, M.D., and Thomas Stevenson, M.D.—The investigations described in this paper were suggested by the evidence given at some recent criminal trials as to the effects produced by some organic poisons upon the lower animals. Drs. Fagge and Stevenson, in order to ascertain whether the effects of digi-

taline can with certainty be detected in the lower animals, instituted a series of experiments, which extended from the autumn of 1864 to the spring of 1865, and the results at which they arrived were laid before the Royal Society in May, 1865; and in the present paper the investigations are given in detail, together with the grounds on which the conclusions are based. The reader will find a notice of the paper in our October number, page 452.

IV. *Observations on the Pathology of some of the Diseases of the Nervous System.* By SAMUEL WILKS, M.D.—In this long and able paper, which contains the substance of a course of lectures delivered by Dr. Wilks to his class, the object of the writer is to reduce the study of the diseases of the nervous system within a more scientific scheme of classification than that usually adopted, and to substitute for such general and unmeaning terms as apoplexy, paralysis, dementia, &c., other expressions having relation to the particular structures affected or to the functional disturbances excited. The brain being a complex organ, and having parts exercising very different functions, it is evident that a particular morbid change cannot always be productive of the same symptoms. Thus, an injury of the surface of the brain will produce the most serious effects, while an injury of its substance may, perhaps, be unattended with any very remarkable symptoms; a lesion of one side of the brain may or may not produce paralysis on the other side. In order to explain many apparent anomalies between the seat of injuries and diseases and the results they induce, Dr. Wilks gives a sketch of the physiology of the brain according to the researches of recent observers, as Charles Bell, Marshall Hall, Brown-Séquard, &c.; and as he regards this organ, not only as the guiding agent in all vital operations, but also as the instrument of the mind, he would include mental diseases, or at least many of them, among the pathological phenomena to which cerebral disease may give rise. We should mention, however, that Dr. Wilks is neither a phrenologist nor a materialist, nor does he assert that all mental diseases are due to appreciable lesions of the encephalon. Among the numerous illustrations and arguments in the paper it is stated that, while disease on one side of the brain will generally, although not invariably, produce pathological effects in the other side of the body, yet that if the disease attacks the central part of the pons Varolii the consequence will be complete paralysis, because the motor-sensory tract passes through this portion of the encephalic mass. As several nerves arise from this part, various functional derangements will be caused, according to the exact spot affected; thus,

the seventh nerve (including both the *portio dura* and the *portio mollis*) being injured at its origin, the face will be paralysed or the hearing impaired, and disease of the pons may affect other nerves, as the fifth or the sixth. By disease still lower down towards the medulla oblongata the fibres of the lingual and glosso-pharyngeal nerves are involved, and special symptoms are produced. Although it is known that disease of one side of the substance of the brain may produce hemiplegia on the opposite side of the body, it is not generally known or admitted that disease of one side of the *surface* of the brain may produce the same effect, but Dr. Wilks adduces cases to prove that such is not unfrequently the case, and that unilateral arachnitis, for instance, may cause paralysis on the opposite side of the body. In the general paralysis of the insane Dr. Wilks considers the affection to have an appreciable morbid anatomy, and that it ought therefore to come under the domain of the ordinary physician, the disease consisting of a diminution of the volume of the brain, the organ becoming shrunken, the sulci between the convolutions being deep and filled with water, and the arachnoïd over them thick and opaque. As spirit-drinking often causes this form of cerebral atrophy, Dr. Wilks thinks that Shakespeare put the truth literally when, in the play of "Othello," he makes Cassio refer to drink as an agent which, when put into the mouth "steals away the brains." On the subject of epilepsy Dr. Wilks offers several useful observations, although he does not profess to write the pathology of that disease; apart, however, from organic diseases, such as tumours, exostoses, &c., he regards it as an affection of the surface of the brain, and he strongly disbelieves the theory put forward by Van der Kolk as to its cause being seated in the pons Varolii. With regard to diseases of the cerebellum, morbid anatomy has thrown very little light upon their nature; and although physiological writers have argued that this organ presides over the co-ordination of the animal movements, Dr. Wilks observes that this view is not supported, so far as he knows, by a single clinical fact. In the cases of disease of the cerebellum which have occurred, and have been recorded in hospitals, no definite or uniform symptoms relating to the harmonising or co-ordination of movements have been observed. On the important question as to whether sudden death is a frequent consequence of disease of the brain, Dr. Wilks decides in the negative, such an event being generally due to disease of the heart.

V. *Remarks on some of the Functional Diseases of the Nervous System.* By Samuel Wilks, M.D.—In this paper, which is a

kind of supplement to that which precedes it, Dr. Wilks treats of some of these derangements of the nervous system which are not accompanied by appreciable and demonstrable changes in the nervous textures. To this category belong such diseases as hysteria, and Dr. Wilks doubts whether tangible changes in the spinal cord have really been discovered in persons who have died of tetanus or chorea; and it is found that a mere increased or morbid activity of the nervous centres exists in many conditions which are considered morbid. At the same time it is not alleged that there may not exist some morbid change in the ganglionic system in many so-called nervous affections; all that is asserted is that those changes have not yet been demonstrated. In practice it is very often of great importance to discover, especially in females whose nervous system is of very delicate organization, whether symptoms are to be referred to mere functional derangement or to organic lesions, and the diagnosis is often exceedingly difficult. Dr. Wilks adduces a number of cases, some from actual practice and some from works of fiction, as showing the influence exercised over the bodily functions by the affections and emotions of the mind; and at the conclusion of the paper he refers to the delusions practised upon many feeble-minded but credulous young men by the advertising quacks who depict in exaggerated language the horrors of so-called spermatorrhœa. Dr. Wilks regards the symptoms described by the victims of this delusion as generally imaginary, and he strongly and justly condemns the conduct of those who make a market of the sufferings which their books create.

VI. *A few Words on the means to be adopted for establishing a Communication between the Bladder and the Exterior of the Body, when the Urethra has become Impermeable, the last resource available in certain cases.* By Edward Cock.—In this short and practical communication Mr. Cock does not profess to discuss the treatment of ordinary stricture of the urethra, but he shows how the bladder may be reached when the canal has become positively and absolutely impermeable to instruments of every description. Some years ago he published a paper in the 'Transactions of the Medical and Chirurgical Society' detailing his experience in the operation of puncturing the bladder through the rectum, and he still thinks this proceedure advisable in many cases; but there are instances where even this plan is not available, and where the bladder and the adjacent parts have become so much altered by chronic disease that the only chance of relief to the patient is given by establishing a free communication between the bladder and the exterior of the body. The opera-

tion now described by Mr. Cock consists in tapping the urethra at the apex of the prostate gland; but there are many practical details in Mr. Cock's operation which have been overlooked by other surgical writers, and hence the proceeding has not been very often adopted. Mr. Cock places the patient in the usual position for lithotomy, care being taken that the median line is accurately preserved; and he then places the left forefinger in the rectum, and with the right hand he plunges the knife into the part just indicated; and afterwards a director and then a canula is introduced into the bladder, and the urine drawn off. Sometimes it is possible after the operation to restore the normal channel of the urethra; but even when this cannot be accomplished the patient passes the urine, changes the artificial opening, and without much inconvenience. Mr. Cock states that he has operated upon at least half a dozen men in this manner; and they were all happy and comfortable, passing their water through the perinæum. Two of the patients are still under his observation, on one of whom he operated twenty-five years ago, and on the other twenty years ago.

VII. *On a Case of Vitiligoidea Plana et Tuberosa.* By F. W. Pavy, M.D., F.R.S.—This disease was described and named by Drs. Addison and Gull in a former volume of the 'Guy's Hospital Reports.' It presents itself in two forms—namely, as isolated or confluent tubercles, or as yellowish patches of irregular outline; and it received its name from its resemblance to the affection described by Willan as vitiligo. The case now described by Dr. Pavy occurred in the clinical wards of Guy's Hospital, and was of the double variety. The patient was a female, and was jaundiced, this disease having been previously described as sometimes accompanying vitiligoidea. She had tubercular elevations on her fingers; and there were cream-coloured patches on the hands, around the eyes, and elsewhere. Some writers on dermatology, especially Hebra, regard this disease as being of a sebaceous nature; but Dr. Pavy, from a microscopical examination of a portion of one of the tubercles, denies the correctness of this view. A coloured engraving accompanies Dr. Pavy's paper.

VIII. *Contributions to the Physiology of Binocular Vision.* By Joseph Towne.—This paper constitutes the seventh of a series of physiological papers on the subject of vision; but it is impossible to give an abstract of it without referring to the former papers, and to the numerous diagrams by which the different propositions are demonstrated. The conclusions, however, at which Mr. Towne arrives may be stated to be, that the

eyes are not the true organs of vision, but are to be regarded simply as two cameras, which gather in the retinal images; and that we see in the direction of the median plane of the head as from one central eye.

IX. *Clinical Remarks on the Treatment of Diseases of the Heart.* By S. O. Habershon, M.D.—In the first part of this paper Dr. Habershon lays down the general principles of treatment to be observed in cardiac diseases, and he concludes by giving the details of some cases which occurred in the practice of the hospital. The principles are to lessen the work of the heart, to ensure the regularity of its action, to lessen the distension of its cavities, to prevent syncope, to strengthen the muscular fibre of the organ, to hinder the fibrillation of the blood in its cavities and in the great vessels, and to prevent secondary complications and to relieve them when produced. These objects are to be accomplished partly by hygienic and dietetic means, and partly by the administration of medicines, great importance being attached to the avoidance of excitement and the regulation of the digestive organs. Alcoholic stimulants are often necessary in many diseases of the heart, both acute and chronic, in consequence of the tendency to syncope, and the preparations of ammonia are frequently employed with advantage; but sedatives must be used with caution, because, although they lessen irritability, they diminish power. Digitalis is useful, from its diuretic action, in diminishing distension, but it requires caution in its use; and tea, tobacco, and lobelia are objectionable. Dr. Habershon approves of the abstraction of blood in certain cases of over-distension of the right side of the heart; and a brisk purgative, he thinks, is also frequently useful in relieving portal obstruction. An engraving illustrates one of the cases described, and shows an adhesion of the pulmonary valves.

X. *Twelve Cases of Poisoning, apparently from the use of Copper for Culinary Purposes. Death from Poison or Disease after nineteen days' illness.* By Alfred S. Taylor, M.D., F.R.S.—Dr. Taylor was consulted in July, 1866, upon the case of an aged man who had died, as it was suspected, from the administration of poison contained in his food. The post-mortem examination revealed the presence of ulceration in the small intestines; but, after careful examination, Dr. Taylor could discover no evidence of any metallic poison, and on the coroner's inquest the jury returned an open verdict. It appeared, however, that the deceased had eaten some veal sent to him from a gentle-

man's house, where twelve persons had been seized with dangerous symptoms after eating of the same dish, prepared by a cook who had consequently been accused of administering poison, but had been liberated from the absence of evidence against her. It transpired that the gravy served up with the veal had been prepared, most probably by accident, in a copper vessel; and it was proved that the persons who partook of the gravy suffered the symptoms of poisoning, while the rest escaped. Dr. Taylor, therefore, concludes that the gravy did really receive a dangerous metallic impregnation, and caused the symptoms from which the twelve persons suffered; but it is a curious point that the aged man, who alone died, did not partake of the gravy, but only of the meat. It is, therefore, doubtful whether he died of poisoning at all; although it is possible that the introduction of some small portions of copper, accidentally laid upon the veal, may have aggravated some pre-existing intestinal disease, and thus have accelerated his death.

XI. *On Papillary Tumours of the Gum.* By S. James A. Salter, M.B., F.R.S.—Of the two cases referred to in this paper, one occurred in the practice of Sir William Fergusson, at King's College Hospital, and the other in that of Mr. Cock, at Guy's. In the first case the previous history was very imperfectly known; and as the patient, who was an aged man, died from natural decay soon after the tumour was operated upon, the question of its malignancy could not be determined. In the second case, Mr. Cock removed the morbid growth, and no recurrence of the disease was observed three months after the operation. Mr. Salter considers that the histology of the morbid growths is identical in the two cases, and he regards them as instances of a non-malignant disease, the tumours being found, on microscopical examination, to consist of hard fibrous tissue surmounted by papillæ, and the latter mainly composed of dense coherent epithelium.

XII. *On Amputation of the Cervix Uteri, and other Methods of Local Treatment, in Cases of Malignant Disease of the Uterus and Vagina.* By J. Braxton Hicks, M.D., F.R.S.—Dr. Hicks thinks that the advantages of local treatment in malignant affections of the womb have not been sufficiently appreciated, in consequence of the failure which has attended many attempts to eradicate the disease. His experience has not been favorable with regard to the extinction of the malady, but still he thinks that by the removal of the diseased parts much good is often effected, because, for a time, the secondary results are obviated, and the disease itself is retarded, and in the mean time the sys-

tem improves, the powers rally, and for a time the patient sometimes resumes a state of apparent health. Dr. Hicks possesses records of twenty-eight cases of mushroom excrescence of the cervix uteri, in every one of which after the operation the patients at once exhibited a marked improvement, which lasted for many months, and in some instances for years, before a relapse took place. He has not seen any satisfactory effects from the use of caustics, but in the employment of mechanical means he has observed much relief afforded to the patients. Of the various methods adopted for removing the morbid growth he prefers the use of the écraseur, of a form which he has previously described. He has never seen any bleeding from the use of this instrument except in one case. He recommends excision as the best mode of treatment in all cases of disease of the os uteri where the diseased mass is large and more or less of a mushroom shape: in such a condition the cervix should be removed, if possible, *in sitû*. Dr. Hicks then describes other forms of disease, some of a less advanced and others of a more advanced kind, in the former of which styptics or caustics may be serviceable when the cervix is too short to admit of removal; and in the latter surgical measures are altogether contra-indicated, but palliatives may be resorted to for the relief of the urgent symptoms.

XIII. *A Case of Intermittent Hæmatinuria; with Remarks.* By William W. Gull, M.D.—This disease is characterised by its intermittent character, and by the appearance of the urine, which looks as if it contained blood, but no blood-corpuscles are present. It has been already described by Dr. Harley, who called it intermittent hæmaturia; but Dr. Gull prefers the term *hæmatinuria*, because the urine is found to contain, not blood, but hæmatin and globulin in a free state. When the urine is boiled, it coagulates and becomes opaque, and on adding nitric acid it becomes more opaque; but if an excess of nitric acid be added, the precipitate is dissolved, and it is therefore considered that the precipitate is due, not to albumen, but to globulin. Dr. Gull's view of the pathology of the disease is that it consists in a loss of some of the dynamical properties of the kidneys. He conceives that these organs, although retaining sufficient power to cast out an excess of urea, have lost the power of converting the hæmatin into the normal colouring matter of the urine, and hence the hæmatin is expelled unchanged; but as the kidneys regain their functions, the ordinary characters of the urine reappear. The case on which the remarks are founded was that of a patient in Guy's Hospital: he was a middle-aged man, of regular habits and anæmic appearance, and the attack seemed

to be attributable to the influence of cold and wet. He left the hospital convalescent.

XIV. *Contributions to the Practical Surgery of New Growths or Tumours. Cartilaginous and Bony Growths.* By JOHN BIRKETT.—This paper forms one of a series, and includes cartilaginous and bony growths, because it is extremely rare to meet with a tumour composed solely of cartilage without bone, or with one consisting of bone without some trace of cartilage. The cases related by Mr. Birkett occurred partly in his own practice and partly in the practice of others, and appear to have been all of a chronic and non-malignant nature. In some instances the disease was successfully treated by surgical means, in some the patients died from local pressure or irritation caused by the tumour, and in a few the growth returned after an operation had been performed. One of the cases came under the notice of the late Mr. Liston, in the year 1837; but no operation was performed, and the man eventually sunk under the weight of the tumour, which was of enormous size. Mr. Birkett classifies his cases according to the regions of the body where the growths appeared, and his present paper includes only those of a cartilaginous character. Two engravings are given in illustration of cases related in the paper.

XV. *A Case of Abdominal Tumour.* By S. O. HABERSHON, M.D.—This was a case of cancerous disease involving both supra-renal capsules, and the contiguous surfaces of the kidneys and the liver. The lumbar glands were secondarily involved, and the ovaries were also implicated. The patient was a woman, aged 38, and the exact nature and situation of the disease were ascertained only at the post-mortem examination. There was some evidence during life that syphilitic poisoning had taken place, but there was no hereditary tendency to cancer. Discoloration of the skin was observed; but it was not the bronzing characteristic of Addison's disease, and existed only on the exposed parts of the face and hands. It had a defined border, and was referred to the effects of the sun. Dr. Habershon observes that the remarkable cases which are described of discoloration of the skin have existed in connection with strumous degeneration of the capsules or with chronic contraction, but not with cancerous disease, in which there is often no cutaneous discoloration.

XVI. *On the Urine in Acute Rheumatism.* By THOMAS STEVENSON, M.D.—This paper contains the particulars of a series of analyses of the urine passed by patients suffering from acute

rheumatism in Guy's Hospital. Dr. Stevenson's attention was chiefly directed to the urea, the uric acid, the phosphoric acid, and the sulphuric acid; and these substances were alone determined quantitatively. The diet of the patients was in all instances uniform; beef-tea and milk being given in the acute stage; fresh bread, butter, and milk, in the convalescent stage, and afterwards meat, potatoes, bread, butter, beer, tea and sugar. The quantity of solid matters excreted, however, varied very much, so that it was difficult to arrive at any very definite conclusions. But it appears from Dr. Stevenson's analyses, taken in connection with the clinical history of the cases, that when the excretion of solid matters is large the patient usually makes a rapid recovery, that the uric acid is always much increased while the disease continues, that the phosphoric acid is generally in greater amount during the progress of the disease than during recovery, but that the quantity really exceeds the quantity excreted in health, and that the excretion of sulphuric acid is generally, and often largely, increased. The urine being always scanty in quantity in acute rheumatism, the density is high; but in order to ascertain the true specific gravity of this fluid, it ought to be diluted to the normal bulk and then examined, and according as it is then of high or low density will the progress of the disease be probably favorable or unfavorable. The quantity of urea excreted is very variable, and it is not always greater during the height of the disease than during convalescence.

XVII. *Case of Excision of the Spleen for an Enlargement of the organ, attended with Leucocythemia, in a patient under the care of Dr. Wilks; with Remarks.* By Thomas Bryant.—The subject of this case was a youth, aged twenty, who had lived in a malarious district, but had never suffered from ague. On a careful examination, it was found that the spleen was the only organ diseased; and the symptoms which he suffered were referable to its enlargement, the whole of the left side of the abdomen being filled by it. A small quantity of blood, as a specimen, was obtained by pricking the arm; and it was found that the white corpuscles were more numerous than the red ones. As all ordinary treatment of the enlargement was unavailing, the operation of extraction was proposed to the patient, and its dangers fully explained; but he readily consented to the suggestion. Mr. Bryant performed the excision, being assisted by Mr. Cock, and succeeded in removing the diseased organ without difficulty and without any loss of blood, the pedicle being tied in halves and returned into the abdomen. The patient, however, died soon after the operation; and it was ascertained

on post-mortem examination that some secondary hæmorrhage had taken place and had caused the fatal result, but the source of the hæmorrhage could not be discovered. The enlarged spleen weighed 4 lb. 7 oz. Notwithstanding the unfavorable termination of the case, Mr. Bryant observes that the operation was, in his opinion, justifiable, and that if such another instance were to come under his treatment, he would make an attempt to save life by excision, as the difficulties of the proceeding are not great, and the immediate danger is less than might be anticipated.

XVIII. *Spontaneous Cure of Aneurysm of the Aorta.* By Walter Moxon, M.D.—The case related is that of a chimney-sweep who was suffering from cancer of the scrotum, and on whom an operation was performed for the removal of that disease. The carcinomatous growth, however, was not wholly extirpated, and the patient died about two months afterwards, the disease having rapidly extended, and being accompanied by hæmorrhage and ulceration. On post-mortem examination, it was found that an aneurysmal sac, commencing in the aorta near its origin, had projected, into the left auricle. The sac was filled with a laminated and partially softened clot, but there was no communication between the sac and the aorta. It is remarked by Dr. Moxon, that the position of the aneurysm, namely, projecting into the left auricle, is one of extreme rarity. The cure of this aneurysm appears to have been complete, and Dr. Moxon compares the case with others in which aneurysms have been spontaneously cured; and he infers that the favorable result was due (in his own reported case) to the exhaustion caused by the cancerous disease; and he argues that if the blood could be artificially impoverished in cases of aneurysm, some hope of recovery might be entertained. The practice of treating aneurysms recommended by Valsalva was by bleeding and depletion; and although this method of treatment has fallen into disuse, Dr. Moxon thinks that it is founded on correct principles and is really deserving of adoption. He concludes from the evidence adduced, that the progress of aneurysm is accelerated by the good nourishment of the sufferer, and that his chance of cure is directly as the wasting of the body.

XIX. *Notes on Cases connected with Obstetric Jurisprudence.* By J. Braxton Hicks, M.D., F.R.S.—The cases related in this paper are calculated to throw some light upon doubtful points connected with suspected infanticide. Fractures of the skull in new-born infants are usually attributed to intentional violence on the part of the mother; but Dr. Hicks has made some ex-

periments on still-born infants, showing that some fœtal crania are so constituted as to be capable of fracture by lateral pressure which the mother might exert in her hasty attempts to deliver herself. The same accident might happen *in uteró* by the manipulations of the midwife. Dr. Hicks also knew a case in which the mother, who was a respectable woman, and had had many children, wished to murder her child from the mere violence of the pain she experienced.

XX. *Cases of Acute Rheumatism treated by Lemon-juice; with Remarks.* By G. Owen Rees, M.D., F.R.S.—Dr. Rees, in a former volume of these Reports, published a few cases of acute rheumatism treated by mint-water, or rather which had received no treatment at all; but although the results were satisfactory, he considered that the patients would have been relieved earlier, and the duration of the symptoms probably curtailed, if lemon-juice had been given. That opinion has been strengthened in his mind by the cases now recorded, in which the average duration of the disease in the uncomplicated cases was 6·8 days, while in the uncomplicated cases treated upon the *expectant* plan the average duration was 8·5 days. Dr. Rees considers that heart-complications scarcely ever occur during the lemon-juice treatment, and when present they have existed before the use of the remedy.

XXI. *Select Clinical Reports. Memoir V.* By G. H. Barlow, M.A., M.D.—The present memoir contains five cases of acute rheumatism, in the treatment of which Dr. Barlow did not rely upon a single remedy nor trust to the expectant plan, but varied his prescriptions according to the exigencies of each case. In one, for instance, he first employed lemon-juice, and afterwards the bicarbonate of potash; in another he chiefly employed the last-named salt; in a third he adopted the expectant system; in a fourth he employed acetate of potash and nitrate of potash. Dr. Barlow considered that in acute rheumatism nature endeavours to eliminate the morbid matter by the skin and kidneys; and when this elimination is going on freely and safely, there is no reason to interfere with active remedies. (Since this memoir was written, Dr. Barlow's valuable life has terminated, after a short illness.)

XXII. *A Second Report of Cases of Acute Rheumatism treated in the Wards of Guy's Hospital; with Remarks on the Natural History of the Disease.* By Henry G. Sutton, M.B.—In this second report of cases, most of which were under the care of Dr. Gull, the writer expresses the same opinions as those he

offered in the previous memoir. Almost the only remedy employed was mint-water; or, in other words, the treatment was expectant. Most of the cases terminated favorably, but one (that of a young woman of twenty-five) was fatal, death taking place suddenly without any very obvious cause, although there was a slight amount of pericarditis discovered during life and verified by post-mortem examination. In reference to heart-complications in acute rheumatism, Dr. Sutton remarks that in only one case was a bruit developed while under treatment, and he doubts whether such a result is common in hospital practice. The advocates of different systems of cure in acute rheumatism are in the habit of referring to the absence of cardiac complications as due to the treatment adopted; but Dr. Sutton seems inclined to think that patients treated only by rest and light diet will generally escape suffering from heart-affection, and he suggests that this point, at any rate, should be decided before the influence of any plan of treatment in preventing such affection can be fairly estimated.

XXIII. *Cases of Operations on the Larynx.* By Arthur E. Durham.—Of the two cases here related, one was an instance of warty growths in the larynx of a child, and the other of a large cherry-stone impacted in the larynx. In the first case, the existence of the morbid growths was ascertained by the laryngoscope, though with much difficulty, owing to the resistance of the patient; and a cure was effected by making an incision from the neck into the larynx, and removing the warts by cutting them with scissors and twisting them off with forceps. In the second case, in which a cherry-stone had become accidentally introduced into a child's larynx, there was impending suffocation, which was relieved by tracheotomy and the introduction of a tube; and the foreign body was afterwards removed by making an incision into the larynx, and extracting by means of a pair of dressing-forceps.

XXIV. *On Diseases of the Retina, with Remarks on its Structure and Normal Conditions.* By C. Bader.—In this paper, which is a long, elaborate, and very able treatise on the anatomy, physiology, and the diseases of the retina, Mr. Bader brings together all the existing information upon these topics; and he not only describes the affections caused by diseases of this part of the eye, but also the functional disturbances of vision caused by diathetic morbid conditions, as syphilis, albuminuria, &c., and by certain medicinal agents, as santonin, quinine, &c. He describes minutely the ophthalmoscopic appearances observed in the diseases of the retina, and the diagnostic signs by which

one affection may be distinguished from another. But the subjects embraced in the paper extend over so wide a field, that it is impossible to give an abstract.

XXV. *Clinical Remarks on Perforations and other morbid conditions of the Membrana Tympani.* By James Hinton.—Before giving an account of the diseases to which the membrana tympani is subject, Mr. Hinton offers a brief description of its healthy structure, availing himself of the labours of Toynbee, Politzer, and Von Troeltsch; and he then gives a series of cases in which the membrane was diseased. The paper is illustrated by a series of plates, twenty-four in number, coloured and on a black ground, representing different morbid conditions of the membrane, and constituting a valuable contribution to aural surgery.

Review VII.

1. *Lectures on the German Mineral Waters, and on their Rational Employment. With an Appendix embracing a short Account of the principal European Spas and Climatic Health Resorts.* By Sigismund Sutro, M.D. 2nd Edition. 1865. pp. 419.
2. *The Climate of the Swiss Alps and the Peruvian Andes compared.* By Alexander Smith, M.D. *Dublin,* 1866. pp. 17.
3. *Australia for the Consumptive Invalid: the Voyage, Climates, and Prospects for Residence.* By Isaac Baker Brown, Jun., 1865. pp. 137.
4. *San Remo as a Winter Residence.* By an Invalid. 1865. pp. 128.
5. *A Visit to Vichy.* By M. Prosser James, M.D. 1866. pp. 50.
6. *Nice and its Climate.* By Edwin Lee, M.D. 2nd Edition, 1865. pp. 180.
7. *The Baths of Nassau.* By the same. 4th Edition. 1865. pp. 100.
8. *The Baths of Switzerland and Savoy.* By the same. 1865. pp. 138.
9. *The Health Resorts of the South of France.* By the same. 2nd Edition. 1866. pp. 213.

We wonder whether it has ever suggested itself to the imagination of any Civil Service Examiner, baffled by the ingenuity

of crammers, confounded by Mr. Panizzi, and eager to be revenged on the unlucky aspirant who may be destined to present himself for the next fiery ordeal of encyclopædiac catechism, to turn his attention to the fine field for gathering in tribulation and confusion to his victims which is presented by the science of medical topography. Fancy the horror which would seize upon the examination-mongers if it were known that Mr. A. had been requested to enumerate the principal health-resorts of Northern Germany; or that Mr. B. had been politely invited to detail the differential characteristics of Achselmannstein and Zaizon; or that Mr. C. had been asked for the latitude, longitude, and climatic advantages of Wildegg, with a brief *résumé* of the price of lodgings and most convenient routes to that fashionable locality! And yet the bewilderment which the subjects of these embarrassing inquiries would experience would scarcely be greater than that which must seize upon many a worthy practitioner of the art of healing when, in despair of benefiting by other means the obstinate ailment of some well-to-do patient whose confidence in orthopraxy is getting rapidly exhausted, he turns to the pages of any recent 'Handbook of Climates,' or 'Guide to Health Resorts,' for assistance in advising his despairing client whither to betake himself in the pursuit of that physical restoration which he is beginning to suspect that he must seek at the hands of some other disciple of Æsculapius than the one to whom he has hitherto confided himself. He takes up, for instance, "the unpretending volume," as its author modestly designates it, which we have placed at the head of our list, and which devotes "the small compass" of some 270 pages to a description of the healing properties and other advantages of the principal mineral waters in Germany alone. After cudgelling his brains in the vain endeavour to balance the relative merits of the waters of one source which are distinguished by the presence of a portion of a grain per gallon of carbonate of lithia with those of another in which the lithia is replaced by strontia, or to determine the exact value of a resort which, amongst its other virtues, includes that of being specially useful in the relief of "*writer's cramp*," "*somnambulismus*," and "*coxarthrocace*," he finds that he has still nearly 150 closely printed pages of supplementary information before him in the shape of an appendix, which, besides giving an account of a dozen or two German spas that have been omitted in the body of the work, succinctly details the sanitary merits and general attractions of at least a hundred other watering-places in different parts of Europe. Yet the unhappy man is only at the beginning of his troubles; for he is told in the preface from which we have already quoted, that "this unpretend-

ing volume is not intended to supersede larger works on the subject," to the perusal of several of which he is assuringly relegated.

Can we wonder that such a prospect as this should deter men who are already hard put to it to keep pace with the progress of the essential branches of medical knowledge, from attempting to form an acquaintance, however slight, with one which has rather the aspect of dilettanteism than otherwise; and that the whole subject of balneology should form a sort of *terra incognita* in the domain of medicine, into which the English practitioner, as a rule, does not care to pry? It is possible, too, that a certain portion of the indifference with which the medical public on this side of the Channel contemplate a department of the art of healing which our French and German neighbours have erected into an independent science, may be due to the results of the investigations which have been made of late years on the subject of dermal absorption in connection with mineral baths. So long as it was thought that the chemical constituents of a mineral water could readily find their way into the system through the skin there was no saying how much of the unquestionable good effects so often produced by the bathing element of a "course" at a Continental watering-place might be due to the mineral water, and how much to the various other conditions of a more or less favorable character under which the patient was brought. But now that it has been positively demonstrated that few if any traces of even the most soluble and readily recognisable reagents applied in solution in water to the skin can be detected in any part of the system, the utility of mineral baths seems to resolve itself into the operation of a set of causes about whose action there is not the slightest mystery, and which are just as accessible at Brighton as they are at Vichy. It cannot be denied that the establishment of the fact to which we have alluded has been a sore blow to the claims of balneology as a science, though it has done eminent service in putting the merits of mineral baths themselves upon a right footing. And it is not strange that many who may have looked upon their employment as probably the more valuable portion of the "cure," in consequence of the opportunity supposed to be afforded by them for gently introducing into the system active medicinal agents, should consider that when this source of therapeutic benefit was eliminated the other elements of the treatment were hardly worth the trouble of any special cultivation.

Still, notwithstanding these and other causes which are unfavorable to the growth of balneological literature in this country, there is a certain demand for it, apparently, and, as a con-

sequence, a corresponding supply. Of the slowness of its growth we have a signal proof in Dr. Sutro's book itself, the first edition of which was published in 1851, but was not exhausted, we are informed in the preface, until 1864. We should have thought that a much shorter period than fourteen years would have been sufficient either to make or mar the reputation of a book in any department of medicine of the size and pretensions of Dr. Sutro's, and that it would have been either consigned to the waste-paper basket or have graduated into the honours of a second edition long before the termination of its second lustrum. However, it is not so, and we congratulate Dr. Sutro upon the enlarged and improved form in which his original 'Lectures' have now made their appearance. They still retain the merit which distinguished them in their former guise, of being the first and one of the best attempts to give a *comparative* view of the principal German mineral springs, with the view of enabling the practitioner to judge of the respective advantages of each. Since the date of their first publication, it is true, other works of a more comprehensive character have appeared which left them somewhat in the background; but Dr. Sutro has availed himself so well of the opportunity which the exhaustion of the first edition offered for re-casting and enlarging the work, that it may now again fairly take rank with the few good manuals on the subject which the English language possesses. We fear, however, that we must take some little exception to the first three chapters, which are devoted to a general but rather obscure exposition of the leading principles of medical hydrology. Not only is the arrangement of the information sought to be given in these chapters confused, but the facts are in some cases questionable, especially those bearing on chemistry and physics. As Dr. Sutro's book, however, aims to be practical in its character, and is intended rather for the busy practitioner than for the philosophic chemist, we do not know that the defects to which we have alluded will detract much from its utility.

Notwithstanding the popularity which Alpine regions have of late years acquired both amongst "amateur casuals" in pursuit of pleasure and invalids in search of health, our knowledge of the general climatic characteristics of mountainous localities is still far from perfect, whilst we have much to learn as to the special peculiarities of many of these districts, particularly in the Western World. A contribution to this latter branch of the subject has lately been made by Dr. Archibald Smith, whose long residence in South America supplied him with abundant opportunities of becoming acquainted with the pathological influences of a considerable part of the central

portion of that continent, and who has thrown together, in the form of a commentary upon Dr. Hermann Weber's 'Notes on the Climate of the Swiss Alps,' some observations on the meteorological and physiological peculiarities of the Peruvian Andes which may not be without practical interest even to the great class of valetudinarians in quest of some new El Dorado of health, who become each year more exacting in their requirements and more venturesome in their travels.

The first part of Dr. Smith's pamphlet is devoted to a criticism on certain statements of Dr. Weber with regard to the meteorological characteristics of the Swiss Alps, which are certainly open to exception if intended to be applied absolutely even to the localities of which they are made, but which are still more inapplicable as general laws without a considerable amount of qualification. For instance, the statement that the temperature of Alpine districts falls at the average rate of 1° Cent. for every 545 feet of ascent, entirely fails, as Dr. Smith shows, when tested by a comparison of the climate of Lima with that of Surco, about 7000 feet above it. Much of the regularity of decline must obviously depend upon local peculiarities, especially upon the outline of the ground and the amount of shelter from chilling winds. On the other hand, we cannot accept the vague assertion of Herndon, as quoted by Dr. Smith, that on the eastern slope of the Andes the trade-winds are so *dammed up* by the mountains, that the atmosphere is compressed, and consequently heavier than it is at a lower elevation, as contradictory of the rule which Dr. Weber lays down, that "the atmospheric pressure decreases with the increase of height," without some corroborative meteorological facts. It would require a good deal of damming up, we suspect, for such a cause as this to raise the barometric column to any appreciable extent; though barometric pressure, like temperature, would probably be susceptible of some modifications in its relations to ascent in mountainous districts from causes which would influence the humidity of the atmosphere. In reference to this latter point Dr. Smith draws attention to the peculiar aridity of the western slope of the Andes, on a zone of which, reaching from about 1500 to a height of 7000 feet, rain never falls; whilst above that line to an elevation of 15,000 feet the soil is abundantly watered and correspondingly fertile.

It is well known to all who have had any experience in the treatment of tropical diseases, how persistently paludal poison retains its hold upon the system when it has once established itself, even years of comparative health being insufficient to prevent the redevelopment of periodic fever in a patient who has once suffered from it in circumstances that would otherwise fail

to call it forth. This is quite borne out by Dr. Smith's observation: at the mines of Cerro-Pasco, 14,000 feet above the level of the sea, the *terciana* caught in the malarial valleys of the coast frequently recurs, and with almost as much violence as in its primitive attack. In these cases the constitution becomes so strongly impressed with the stamp of the malarial influence, that almost any depressing agency suffices to kindle afresh the latent fire of the disease; and there is no room for surprise that what occurs frequently in the temperate climates of Europe should equally happen in the temperate regions of Peru. Again, few facts would be thought generally to be better established in medicine than the absolute limitation of yellow fever to tropical and subtropical regions. Although the disease has been frequently imported to our own shores under circumstances eminently favorable for its diffusion, it has never succeeded in domiciling itself amongst us as other forms of fever have. A similar condition of things obtains, to a certain extent, in the subtropical climate of Peru. Cases of yellow fever which had been caught on the sea-coast, when brought into the city of Arequipa ran their course in the affected individuals much as usual, death, accompanied by black vomit, being as frequent as at the seaport of Islay. But here the power of the disease ended, at least so far as the white population was concerned; its germ seems to have perished with the death of the individual, and to have been unable to propagate itself in an epidemic form. Yet, strange to say, no sooner was yellow fever introduced amongst the native population of the suburbs of Arequipa, than it assumed a distinct epidemic character, and was attended with great mortality. And this, it must be remembered, was at an elevation of some 10,000 feet above the sea, and in a cool, dry climate. At still higher elevations, we are told, the epidemic did not cease, but was observed to change its type more to the form of typhus, without any marked lesion being noticed on post-mortem inspection in the stomach or intestines—organs which are notably affected at lower Andine levels of from 1800 to 10,000 feet above the sea. This fact is curious from two points of view: firstly, as confirmative of the opinion originated by Baikie and others, and now generally accepted, that typhus and typhoïd are the modifications which yellow fever undergoes when it passes from tropical to temperate climes, and that both are manifestations under different circumstances of the same septic influences; and, secondly, as illustrative of the pathological peculiarities which in many respects distinguish the white from the coloured and dark races of mankind. What but a type of constitution essentially different in its susceptibility to and its power of reaction against the special poison of yellow fever can

explain the readiness with which the disease propagated itself in an epidemic form in one case, and its limitation to the originally infected individuals in the other? Probably a certain amount of this facility of propagation amongst the native Indians of the Arequipan district must be attributed to the favorable conditions which the social habits of uncivilised men offer for the spread of infectious diseases; but even when due allowance is made for this modifying circumstance, there is still a sufficient contrast between the effect of the disease upon the two races, to be explained only by the cause to which we have referred.

The remaining portion of Dr. Smith's *brochure* is devoted to the influence of the Peruvian climates on tuberculosis. The results of his experience here are in every respect confirmatory of those of M. Jourdanet in Mexico, which we brought before our readers in a former number of this Review (July, 1863, p. 81). On the sea-coast of Peru, as on that of the Gulf of Mexico, incipient tubercular phthisis is one of the commonest of pulmonary affections. From time immemorial, however, it has been known to the natives that a removal of phthisical patients, even in the stage of well-marked ulceration and cavities in the lungs, to the inland valley of Jauja, at a height of 10,000 feet above the sea, was followed by an almost invariable suspension of the disease—a fact which is quite corroborated by the practice of physicians at the present time. From the statistics of Dr. Fuentes, of Lima, published in 1858, it appears that nearly 80 per cent. of the cases of phthisis sent to the Jauja valley are cured. So forcibly has this fact been brought before the Peruvian Government, that it has established in this valley a military hospital for consumptive patients, and especially for native Indian soldiers, who in the capital are singularly prone to phthisis. Indeed, of the whole annual mortality of Lima, no less than $22\frac{8}{39}$ per cent. is attributed by Dr. Fuentes to this disease. So that, as Dr. Smith remarks, we must at least admit that the Peruvian physicians have abundant opportunity of testing the various modes of treating it; and the unanimous opinion at which they have arrived as to its curability by a removal of the patient to the Jauja valley, leaves no doubt as to the well-founded nature of the reputation which that district enjoys. How much of this curative power may be due to the moral influence exerted by its scenery and associations, it would be difficult to say; but a nearer approach to Elysian felicity than the lives which its population are described as leading, it would be probably impossible to find in any less favoured land. The harvest being home, we are told, the whole rural population rest from their agricultural labours for eight months in the year, which they give up entirely to amusement and feasting,

trusting to the rain of heaven during the other four months of the year to fertilise their land and yield them more food than they require. Surely Dr. Johnson must have had this valley in his eye when he wrote his pleasing fiction of 'Rasselas,' for no other of which we have ever heard so nearly realises the physical conditions under which the Prince of Abyssinia sought the perfection of human happiness. And we may mention for the benefit of such of our readers who, as valetudinarians or philosophers, may feel an interest in knowing more about this charming region than we are able here to tell them, that if the project of running regular steamers up the Amazon from Pará, for 2500 miles from its mouth, which has just been started by an American Company, becomes an established fact, they will be able to transport themselves directly into it, almost without changing vessels, after a voyage which, though long, is neither tedious nor debilitating, and which will carry them through some of the most magnificent scenery that the world can produce.

Although we are not destitute of works on the climate of Australia, the subject has not yet been nearly so thoroughly investigated as to leave no opening for a new-comer. From this point of view alone, therefore, we were glad to receive the little volume in which Mr. Isaac Baker-Brown, junior, has incorporated the experience of Australia and its climate, which he has acquired as the result of several successive voyages to that country, made in the service of Her Majesty's Commissioners of Emigration. But, apart from the fact that this work appears to be the production of an intelligent and observant medical man who, from the official position which he has filled as Surgeon Superintendent of Emigrants, has enjoyed excellent opportunities of witnessing the effects of a long sea-voyage upon the most frequent and intractable of diseases, it has the recommendation of being free from any suspicion of being dictated by those interested motives which unfortunately underlie the manufacture—if we may be allowed to use the term—of so much of the climatological literature of the day. Even if Mr. Brown did not expressly disclaim all personal interest whatever in the reputation of any particular part of Australia as a health resort, a very brief examination of his book would be sufficient to convince the reader that it is written in a thoroughly judicial and impartial spirit. This is not one of those too frequently occurring cases which are brought within the domain of criticism, in which the climate of some particular locality is written up in the most enthusiastic terms by a gentleman who is engaged in practice there, and who may therefore, without any injustice, be suspected of being at least hardly in a situation to

form an impartial opinion of its merits. Mr. Brown assures us that he has settled finally in his own country, with no hope of ever again visiting the antipodes, and that his only object is to render the information which he has acquired available for the benefit of invalids at home; and we feel it both a justice and a pleasure to say that that class of his countrymen is greatly indebted to him for the labour which he has undertaken, and for which his chief reward will probably be the thanks of those who may have occasion to profit by the information with which he has provided them. For our own part, we must say that we have read Mr. Brown's book through, from beginning to end, with the greatest satisfaction. Although written ostensibly for the non-medical reader, and a model of what a book of the kind should be—unpretending, intelligible, and full of sound practical advice—it may be perused with considerable advantage by his brother practitioners, who will find in it a great deal of useful information on the subject of the climatic characteristics of our Australian colonies. But it is to the emigrant to Australia that its utility will be most unquestionable; and we have no hesitation in recommending all such, whether their object in migrating be the search of occupation or of health, to make it an item of their library, however compendious they wish that element of their outfit to be.

There is only one objection that we have to make to Mr. Brown's work, and that certainly is a rather serious one, inasmuch as it affects the value of the most important information which Mr. Brown has to give, viz., as to the climatological features of the principal Australian resorts. This is always conveyed in a statement of the "mean temperature" throughout the year, or during the winter or summer, as the case may be. Now, as we have pointed out more than once before in this Journal, a simple statement of the "mean" temperature of any locality is by itself absolutely worthless, and often misleading; for two localities may have the same "mean" annual temperature, and yet differ greatly in their suitability as health resorts, the one being characterised by great extremes and rapid fluctuations of temperature, whilst that of the other is more or less equable all the year round. For instance, the mean winter temperature of the city of Mexico does not differ much from that of the more favoured parts of our own country; but its effect upon the constitution is much more prejudicial than that of any English locality from the fact that it varies at some seasons of the year during the twenty-four hours, and even in passing from the sunshine into the shade, between a heat which is almost tropical and a cold which is frequently below the freezing-point. We wish that we could impress upon travellers

and writers on climate that what is wanted in their descriptions is not only the "mean" temperature during any given period, but also the "extremes" and the "mean fluctuations." With these *data* it would be possible to form a pretty accurate estimate of the value of any given locality as a resort for purposes of health, and to compare it with any other, but without them all attempts to do so are mere guess-work. For it is precisely the extremes of temperature in any spot and the rapidity of fluctuation between them which form the most important consideration in determining its suitability for an invalid; and unless we know this we know next to nothing. Having thus cleared our conscience of this defect in Mr. Baker Brown's book, we can the more readily speak of its merits and refer to some of the more interesting subjects with which it deals.

It may seem rather paradoxical to many persons to be told that one of the recommendations in favour of a visit to Australia in the treatment of consumption is the length of the voyage, yet we think that Mr. Baker Brown has good reason for making the assertion. Admitting, what we believe no one will deny, that a full exposure to the fresh bracing air of the ocean, under a moderately warm sun, and with all the conditions of diet and occupation which are favorable to health, is one of the most efficient means which we possess for restoring tone to the failing powers of nature, it seems but a necessary consequence that the longer such a remedy can be applied, the more permanent are its effects likely to be. It is easy to understand how invalids, multiplying the discomforts of shorter voyages near home by the increased length of that to Australia, should hesitate to encounter what promises to be a three months' edition of the sickness and exhaustion experienced by so many on crossing the Channel. That this is a fallacy, however, hardly needs to be explained. Even at the most unfavorable season of the year, when the Bay of Biscay is swept by the equinoctial gales, the voyager who passes through it finds it often less trying than the short and chopping seas of our own coasts, and has in the large majority of cases recovered both his equanimity and appetite long before he gets within the latitude of the tropics. Mr. Baker Brown states that he has rarely in any case seen sea-sickness last severely for more than three days, and but in two instances for several weeks. It might be thought that the exposure of patients, in whom pulmonary tuberculosis had made its appearance, to the risk of hæmoptysis which vomiting carries with it, involves serious danger; but Mr. Baker Brown assures us that the occurrence of this accident is exceedingly rare. Once out in clear blue water, the invalid soon picks up his "sea legs;" and when he has done so, the rapidity with which he is restored

to health and strength is only dependent on his own prudence in avoiding the temptations to undue self-reliance on both which even on shipboard beset him.

It is unnecessary for us to follow Mr. Baker Brown into a discussion of the advantages which he claims for the Australian voyage on the score of its comparative safety, and of the size and superior equipment of the vessels which make it. Taking these matters for granted, our readers will probably be more interested by learning, in as brief a space as we can communicate them, his opinions as to the relative advantages of the different settlements in that great continent. Our author animadverts, with justice, upon the stupidity of people who, in many cases without any excuse for their ignorance, talk of "the climate of Australia" in the same way as they would of that of the Isle of Wight, forgetting that there is almost as much difference between the climate of Melbourne and Queensland as there is between that of Scotland and Algiers. Of the colonies on the Australian continent Mr. Baker Brown gives the preference to Western Australia, so far as mere climate is concerned, in consequence of the greater dryness of its soil and the equability of its temperature. But, although on these grounds very favorable to the health of settlers who are strong enough to "rough it" in the occupations of an agricultural or pastoral life, it is on social and other grounds undesirable as a resort for the invalid.

Queensland is too near the region of the tropics, and, as a consequence, too depressing to the invalid, to be recommended as a residence, even putting out of consideration the prevalence of ague, dysentery, and fever, which give its death-rate a disagreeable prominence over those of the other settlements. Victoria, though in other respects a healthy colony, is, from the coolness of its winters, not adapted for the continued residence of consumptive invalids, as may be gathered from the fact that the average annual mortality from diseases of the respiratory organs in Melbourne, which is said to be a fair type of the colony, is no less than 22·7 per cent., of which number nearly one half die from consumption. South Australia, from its sandy soil and dry atmosphere, possesses a climate in many respects analogous to that of Algiers, but very superior; the deaths from consumption in this colony are said not to exceed 7 per cent. of the total mortality. Adelaide, however, the chief town of the colony, suffers much from dry and dusty winds, as well as from other sanitary disadvantages, which render its reputation as a health resort rather doubtful. The same causes detract during the summer from the charms which Sydney possesses in the beautiful scenery by which it is surrounded; whilst during the

winter the rain falls throughout the whole of New South Wales in a continuous down-pour, which is only to be equalled in the rainiest portions of the tropics. But in the intermediate seasons, its clear, bright sky, mild atmosphere, and luxuriant vegetation, make the greater portion of New South Wales one of the most delightful countries in the world to live in.

But it is for Tasmania that Mr. Baker Brown reserves his highest meed of commendation. "It is," he says, "*the* climate *par excellence*, not only of the antipodes, but also of the whole universe, for that class of English consumptive invalids who require a climate more equable than that of our own, but not of much greater temperature, and with an ozonized, clear, and bracing atmosphere, the elasticity of which is surprising." Tasmania appears to combine all the advantages of the other Australian climates with none of their disadvantages. Its insular position and limited extent protect it from the aridity which characterises the greater portion of the neighbouring continent, whilst the mountainous nature of the country relieves its atmosphere from the enervating influence which its warmth would otherwise give to it. When we add that it has a fertile soil, beautiful scenery, and an almost uniformly cloudless sky, it will be admitted that Mr. Baker Brown's eulogies upon it are not without good grounds. Nor are the vital statistics of the colony which he quotes less satisfactory. The total annual mortality of Hobart Town, as taken from a mean of five years, is stated by Dr. Bird, in his work on Australasian Climates, to be 23 per 1000. Of this number less than 8 per cent. are referable to consumption, whilst in the rural population of the colony the percentage is considerably lower. We must not omit to mention the fact, which Mr. Baker Brown states without fear of contradiction, that "there is a larger proportion of old people in Tasmania than in any other part of the globe," as making up a sum total of evidence in favour of the oldest of our antipodean colonies, which certainly gives it a notable eminence amongst the health resorts of the world.

We have delayed thus long over Mr. Baker Brown's interesting and agreeably written little work, because, as we have before stated, it is one of the most impartial and trustworthy accounts of Australasian climates with which we have met. Moreover, as not only the tide of emigration is setting yearly more strongly in the direction of these colonies, but as the facilities for reaching them are being each year steadily improved, they demand a larger share of attention, simply when considered as health resorts, than they have hitherto received. To the practitioner who may be consulted by one of his patients on the advisability either of a temporary visit or of a permanent retreat there; to

the invalid who may propose to go there for the benefit of his health, and to the robust who may contemplate resorting thither for the sake of occupation, we cannot recommend a more useful and in every respect unobjectionable work than the present. In it will be found, in addition to the purely climatic information to which we have referred, detailed advice as to conduct on the voyage, arrangements before starting, and other matters equally essential, whether to the healthy or valetudinarians, which fairly entitle it to be considered a cheap and useful handbook for the Australian voyager.

Having already encroached considerably on the space which we had allotted ourselves for the notice of the remaining works on our list, we must deal with them very briefly—a mode of treatment which it is some satisfaction to think will not be very inadequate to their claims. In the unassuming little book entitled 'San Remo as a Winter Residence, by an Invalid,' Mr. Aspinall has endeavoured to interest the English public, and especially the health-seeking portion of it, more actively still than his predecessors in this line have succeeded in doing, in the rising little town of the Riviera, which is treading fast on the heels of its not much older neighbour, Mentone, and which its spirited inhabitants even aspire to raise before long into a dangerous rivalry with Nice. When we have said that Mr. Aspinall's book is written by a layman, that it does not aim to be more than a general description of San Remo and its environs, and that the proceeds of its sale are to be devoted to the erection in the town of an English church, we believe that we have said nearly all that needs saying about it. If any of our readers should be meditating a journey to the shores of the Mediterranean this winter, and have not positively made up their minds as to their destination, we would advise them to do both themselves and Mr. Aspinall the justice of perusing his little book on San Remo before finally deciding whither they will go. It is no small recommendation in its favour that its author is no neophyte in the search of health, carried away by the enthusiasm engendered by a first experience, but a veteran in the cause, who has wintered before in Egypt, Madeira, Rome, and other parts of Italy, and who prefers the little nest on the seacoast of Liguria to them all.

A visit to Vichy on his way to England last year from the Mediterranean has induced Dr. Prosser James to put together the information on the subject of its mineral waters and their uses which he gained whilst there. If Dr. James's pamphlet has nothing of a highly novel character about it, it has at least the negative merit of being as good an account of Vichy as most of the numerous other guides to that fashionable

resort, whether French or English. Indeed, we think that it is fairly entitled also to the title of moderation in its estimate of the value of the Vichy waters—a virtue to which some of the other historians of that spa can make but scanty claim. Intending visitors to it and medical men generally can hardly do better than avail themselves of Dr. James's pamphlet, which will put them quite *au courant* with the latest researches on its waters and with the cases in which they are likely to be useful. Dr. Lee and his various works on the health resorts of Europe are so well known both to the profession and the public as to require no introduction at our hands. The fact that all of those on our list have entered upon the career of at least a second edition, with one exception, is a sufficient testimony to their merits and to the industry of their compiler. The exception to which we allude is the 'Baths of Switzerland and Savoy,' which is, however, in some sense a second edition, inasmuch as it is a separate and enlarged edition of that portion of the author's 'Baths of Germany, France, and Switzerland,' which refers to the watering-places of the latter country. Appended to the work is a "Summary on the Action of Mountain Air on the Animal Economy," taken from Dr. Werber's 'Schweitzer Alpenluft.'

Review VIII.

Principles and Practice of Medical Jurisprudence. By Alfred Swaine Taylor, M.D., F.R.S. *London:* Churchill. 1865. 8vo, pp. 1186.

On Poisons in relation to Medical Jurisprudence and Medicine. By Dr. Taylor. 2nd Edition. *London:* 1859. pp. 863.

A Treatise on Poisons. By Dr. R. Christison. *Edinburgh:* Adam and Charles Black. 4th Edit. 1845. pp. 987.

Handbuch der Giftlehre. Von Dr. A. W. M. Van Hasselt. Nach der Zweiten Auflage aus dem Holländischen bearbeitet, von Dr. J. B. Henkel, zu Tübingen. *Braunschweig:* Vieweg und Sohn. 1862.

Handbook of Toxicology. By Dr. A. W. M. Van Hasselt. Translated and edited from the Dutch of the Second Edition, by Dr. J. B. Henkel. *Brunswick.* 2 vols.

Handbuch der Toxicologie im Anschlusse an die Zweite Auflage Von A. W. M Van Hasselt's Handleidung Tot de Vergiftleer bearbeitet. Von Dr. Th. und Dr. A. Husemann. *Berlin:* Reimer. 1862.

Handbook of Toxicology. Founded on the Second Edition of Van Hasselt. By Dr. Husemann. 2 vols. pp. 978.

Handbuch der Speciellen Pathologie und Therapie. Redigirt von R. Virchow. Zweiter Band, Erste Abtheilung. Intoxicationen—bearbeitet, von Prof. Falck. *Erlangen:* Enke. 1865.

Handbook of Special Pathology and Therapeutics. Edited by R. Virchow. 2nd Vol. Part 1. Intoxications. By Prof. Falck. pp. 336.

Die Gerichtliche Chemie für Gerchtsarzte und Juristen. Bearbeitet von Dr. F. C. Schneider. *Wien:* Braumüller.

Medico-legal Chemistry. By Dr. F. C. Schneider. *Vienna:* 1862. pp. 385.

Anleitung zur Ausmittelung der Gifte. Von Prof. Fr. Jul. Otto. Zweite durch einem Nachtrag vermehrte Auflage. *Braunschweig:* Vieweg und Sohn.

A Manual of the Detection of Poisons. By Prof. Fr. Jul. Otto. Translated, with Notes and Additions, by William Elderhorst, M.D. *New York:* Baillière. 1857. pp. 178.

Manuel Complet de Médecine Légale. Par J. Briand et E. Chaudé. 7ième edition. *Paris:* Baillière. 1863.

Complete Manual of Forensic Medicine. By J. Briand and E. Chaudé. 7th Edition. *Paris:* Baillière. pp. 1548.

A Handbook of the Practice of Forensic Medicine. By J. L. Casper. Translated for the New Sydenham Society. *London:* 1861, et. seq. 4 vols. pp. 1439.

Das Mikroskop in der Toxicologie. Von Dr. A. Helwig. *Mainz:* Von Zabern. 1865.

The Microscope in Toxicology (with Plates). By Dr. A. Helwig. *Mayence:* Von Zabern. pp. 100, pl. 16.

Die Lehre von den schädlichen und giftigen Gasen. Von Dr. H. Eulenberg. *Braunschweig:* Vieweg und Sohn. 1865.

Noxious and Poisonous Gases. By Dr. H. Eulenberg. *Brunswick:* Vieweg and Son. pp. 550.

The Principles of Forensic Medicine. By W. A. Guy, M.B. *London:* Renshaw. 1861. pp. 534.

Practical Chemistry for Students. By W. Odling, M.B., F.R.S. *London:* Longmans, Green, and Co. 1865. pp. 241.

Annales d'Hygiène, Henke's Zeitschrift, Casper (now *Horn's*) *Vierteljahrschrift, Virchow's Archiv, Reichert and Du Bois' Archiv, Henle and Pfeuffer's Zeitschrift, Moleschott's Untersuchungen, Hoppe-Seyler's Medicinisch-Chemische Untersuchungen, Canstatt's Jahresberichte, Schmidt's Jahrbucher, &c. &c.*

Resuming our survey of the condition of medical jurisprudence at the present day commenced in a previous number wherein we treated of general toxicology, we now proceed to the consideration of special toxicology, founding our observations, as before, on Dr. Taylor's recent work on the 'Principles and Practice of Medical Jurisprudence.' Once more, at the outset of our labours, we deprecate the idea that we are influenced by any feelings hostile to this work—quite the opposite; we acknowledge that it is the best English book on Forensic Medicine, and many of the deficiencies to which we have alluded, and still may allude to in connection with it, are faults rather of the subject than of the work itself. Some of the points referred to could not well be discussed in a general work; and it is a noteworthy feature that, although we have pointed out *deficiencies*, or, at all events, what may be considered as such, we have not had to correct *mistakes* — two very different things. Men often disagree as to what had better be taken up, and what it is advisable to leave out, and we should not be at all surprised if it so happened, that some of the questions we have referred to as injudiciously pretermitted by Dr. Taylor, were so after due deliberation and of aforethought. What in reality we have all along aimed at is, taking this work as an exposition of what is ordinarily received in England, to lay before our readers a conspectus of the views held on the Continent whether before or after the time of the publication of the above-named work.

Discussing the various poisons in the order in which they are treated by Dr. Taylor, we must first speak of the corrosive mineral acids, of which sulphuric acid may be taken as the type. Injuries from this substance may be either external or internal, although the latter are of course the more common; in fact, to the former of these Dr. Taylor does not at all allude, although he does so in another connection, viz., that of "burns;" although, from the increasing frequency of this crime, it might be as well to direct special attention to it in connection with the toxicology of the acid. Those interested in the matter will find an excellent report of the first case that came before a court of law, given by Dr. Christison in the 'Edin.

Med. and Surg. Journal' for 1829, and written in the elegant and accurate manner so characteristic of that eminent toxicologist. Speaking of their internal exhibition, Dr. Taylor very properly points out the immediate accession of the symptoms, as a very important diagnostic of poisoning by the concentrated mineral acids; but the reader would be also apt to infer from his statements that vomiting was an *invariable* symptom, especially after a large dose of oil of vitriol, which is by no means the case. An instance is recorded in the 'Med. Times and Gazette' for 1863, where death occurred in twenty-two hours, but without vomiting and without perforation of the walls of the stomach. The mouth is ordinarily severely injured by this acid, the mucous membrane being usually swollen and whitened at first, but, after a time, more especially on exposure to the air, becoming brownish or blackish; but neither is this constant, for cases of poisoning have repeatedly occurred where the mouth was found to be unaffected. This, as Dr. Taylor indicates, would appear to depend to a certain extent on the mode in which the poison is placed in the mouth, but not altogether so, for, as a general rule, it will be found that the local injuries are more severe, especially in the region of the larynx, in cases where the liquid has been forcibly poured down the throat, than when it has been willingly and eagerly swallowed. One very important symptom has been entirely overlooked by Dr. Taylor, viz., the state of the urine and urinary organs, should the patient survive for any length of time. In such cases there is always more or less difficulty in passing water, which is generally scanty, high coloured (blue in poisoning by sulph-indigotic acid), and, according to Leyden and Munk, containing albumen, blood, and fibrinous casts, although according to Smoler these last are not constantly present. After death by the concentrated acid, the stomach is very often found to be perforated, which, as Dr. Taylor points out, may in certain instances result from the action of the acid after death: if so, however, this post-mortem effect must be rather rare, as the acid is most frequently so thoroughly evacuated by vomiting, that no trace of it is left in the stomach after death, whilst Van Hasselt's experiments by the vivisection of rabbits show that it may undoubtedly occur during life. When perforation does occur, the adjoining organs will also be implicated, and their texture destroyed; but for this, perforation is by no means necessary, as the acid frequently thins the coats of the stomach, and finally transudes from it into the cavity of the peritoneum. On the possibility of this transudation Dr. Taylor casts a doubt, but an excellent case recorded by Dr. Rutherford Haldane in the 'Edin. Med. Journ.,' and which seems to have been entirely overlooked by our author, puts the matter beyond disputation. In the case referred to, the

omentum was greyish, and the blood in its vessels firmly coagulated, so as to cause them to resemble cords, the liver was softened, and sulphuric acid was found in the fluid contained in the peritoneum; yet the stomach was perfectly entire, although its walls were blackened. This darkening of the walls of the stomach (rarely if ever observed in the gullet), according to Casper, depends entirely on the action of the acid upon the blood; but this would not seem to be absolutely correct, for there is no doubt but that oil of vitriol has the power of charring and blackening albuminous matters, ending according to Mulder, in the production of sulphate of ammonia along with free sulphuric and humic acids. In one very good specimen contained in the museum attached to the Manchester School of Medicine, the walls of the viscus referred to, look as if they had for a time been exposed to the action of fire, being of a brownish colour throughout, whilst in another preparation a black layer of transformed tissue has been formed on the inner surface of the viscus. From the rapidity with which it proves fatal, the intestines are rarely affected for any distance downwards, and for the same reason purging is seldom observed. Some toxicologists seem to think that this constipation depends upon a constriction of the pylorus which is usually, but not not invariably, observed after death; but the true cause would rather seem to be that indicated above, for should life be prolonged for any length of time dejections of bloody or blackish matters are usually noticed. The duration of the case has also an important bearing on the post-mortem appearances observed elsewhere, for the patient sometimes survives long enough to enable the mouth and gullet to resume their normal condition. Of course, such cases must be carefully distinguished from those mentioned above, where no injury to these parts was incurred from swallowing the poison. In cases of death from oil of vitriol, the blood is usually thick and treacly, and darker than usual; whilst, again, in some instances it is coagulated, probably from the effects of acid absorbed into the veins. These clots have been analysed by Dr. Taylor, and found to contain no sulphuric acid, from which he would appear to deduce the fact that the acid is not absorbed during life, although he mentions the fact of its elimination from the body in the shape of increased sulphates in the urine, as well as the researches of Dr. Geoghegan, who proved that in the blood it took the place of phosphoric acid, setting the latter free. A remarkable series of cases are mentioned by Dr. Taylor, in which the respiratory organs were more or less implicated: chief of these is the case related by Dr. Gull, where the lungs alone were affected, and that to such an extent as to be almost entirely disorganised.

Compared with sulphuric acid, it is rare for us to encounter

cases of poisoning by nitric acid, and the employment of hydrochloric acid for this purpose is still less frequent. The symptoms produced are very much alike in all, differing only in some minor points: thus where nitric acid has been employed, the stains and vomited matters are generally yellow, and the eructations are more intense and more disagreeable in their character; while, being volatile, cases of poisoning by its vapour are not unknown. The history of nitric acid as a poison has been fully investigated by Tartra, so that we may be excused from entering into it at greater length. Hydrochloric acid is, however, on a more unsatisfactory footing, seeing that the recorded cases of poisoning by it are few in number, and have never been carefully collected. One of the best recorded cases is given by Dr. Guy in his 'Forensic Medicine,' with a drawing of the œsophagus taken from a preparation in the museum of King's College. Another tolerably characteristic case of this mode of death has been narrated by Deutsch in the 'Preuss. Vereins Ztg.,' No. 48, but has been overlooked by most systematic writers. In this instance, the mouth and upper part of the pharynx were on inspection found uninjured, but the œsophagus had assumed a dark-brown appearance at its upper part, while towards the cardia it was blackened and its mucous membrane reduced to the condition of a pulp. The stomach was reddened externally, and marked with hard, dry and blackened spots; internally it was blackened, and its mucous membrane pulpy. There were marks of effusion of blood into the other coats of the stomach, but the remainder of the alimentary canal was unaffected. The poison in this case had been the concentrated acid, but even in this form it seldom gives rise to perforation.

It will be readily understood that as the exhibition of any of these acids is *usually* followed by immediate vomiting, their detection will be no easy matter, more especially as combined with bases they not only constitute part of our food, but are constituents of our bodies. This is especially the case with oil of vitriol, and with hydrochloric acid. It will therefore be seen that the mere demonstration of the existence of these acids, in a state of combination within the body, is far from supplying the proof necessary to establish a case of poisoning, except it can also be proved that they were found in a free state. Neither is it enough to show that their salts are present in much greater excess than usual, as these are frequently exhibited medicinally. To these objections Dr. Taylor very properly replies, that the anatomical lesions are enough to establish the cause of death: still, it is of some importance to ascertain by chemical means what the toxical agent really was. In his work on Poisons the above-named author refers to several plans for separating sul-

phuric acid from its salts as they exist in the stomach of a person who has died from the effects of the acid, but he has not, we think, succeeded in indicating one which promises well. Four schemes have been proposed for separating oil of vitriol from organic substances: viz., Orfila's, founded upon the solubility of sulphuric acid and the insolubility of its salts in ether; but to this it is objected that only a portion of the acid is so taken up, whilst the ether containing this acid differs so little in specific gravity from the watery portion that there is very great difficulty in effecting their complete separation. Devergie's process is still more objectionable, founded as it is on the reduction of that portion of the sulphuric acid which, from its tendency to combine with albumen, is inseparable by water, to the condition of sulphurous acid, and the detection of this by the blue colour it produces with iodic acid and starch. All albuminous tissues, however, contain sulphur; and this, by exposure to a high temperature, may be converted into sulphurous acid — a fact which vitiates the whole process. Christison, again, precipitated by carbonate of lead, separating the excess of this reagent by nitric acid, and then passed a stream of sulphuretted hydrogen through the purified sulphate: the great objection to this process is that it offers no distinction between the acid in a free and in a combined state, which is in reality the most essential point. Schneider's process is founded on the solubility of sulphuric acid in strong alcohol, its salts being left behind; but it is urged, in opposition to this, that alcohol also abstracts a portion of the acid from acid salts, such as alum—an objection which is not very easily disposed of: the presence of acid sulphates, however, in contradistinction to that of the free acid alone, is readily shown by evaporating a portion of the purified fluid. Such being the obstacles in the way of an exact determination of the amount of acid present, it is certainly a matter for congratulation that the post-mortem appearances are usually so decisive in their character.

For the reduction of the sulphate of baryta to the condition of sulphide of barium, as is done in the process generally adopted, Dr. Taylor recommends ferro-cyanide of potassium as the reducing agent, and also very properly enumerates as one of his tests the action of this reduced sulphide on nitro-prusside of sodium, the purple colour thus evolved being one of the best means of diagnosing the existence of sulphuretted hydrogen. The difficulties enumerated above prevail equally in the cases of nitric and hydrochloric acid. Probably, to enable us to distinguish between the free acids and their salts, Schneider's plan is the best we can have

recourse to in the case of nitric acid, and distillation for hydrochloric acid. The latter process may also be employed for the separation of nitric acid, but with less advantage. Talking of this acid, there is a test sometimes employed in Germany for the detection of it in small quantities which is not alluded to by Dr. Taylor. This is the peculiar action which a trace of it has upon a solution of narcotine in sulphuric acid, turning it immediately from a faint yellowish-green into an intense blood-red colour. In performing this operation, however, care must be taken to have the sulphuric acid perfectly free from nitric acid, that no heat be employed in forming the solution, and that this be always freshly prepared when about to be required, or the result may be fallacious. Altogether, we are justified in saying that, so far as regards the mineral acids at least, pathology is a far better basis than chemistry whereon to found an opinion, and should therefore be most attended to.

Closely allied to the mineral acids in some respects, and yet differing from them as widely in others, is the poison we propose discussing next, viz., oxalic acid. This substance, from its cheapness and the readiness with which it may be procured, might be expected to occupy a high place in the list of popular poisons—an anticipation certainly verified in this country, but by no means so on the Continent, Casper, for instance, never having seen a case. The strongly caustic taste of sulphuric acid prevents to a great extent its employment as an implement of murder, and the extreme sourness of oxalic acid operates in a very similar manner; in fact, neither are generally employed to effect such a purpose, except where the victim is a powerless infant, or so intoxicated as to be unable to distinguish one thing from another. From the extensive application of oxalic acid to manufacturing purposes, and the great resemblance borne by itself and some of its dangerous compounds to several medicinal substances, it might also be expected that accidents should occur, nor are these, unfortunately, by any means unfrequent. As to the symptoms, one very important distinction between oxalic and the mineral acids is, that while they both have a distinct local action, as is evinced by the post-mortem appearances in fatal cases, as well as the general symptoms or constitutional reaction manifested more especially by the latter, oxalic acid has in addition a very powerful effect on the nervous system, which to great extent conceals the latter, and which appears to be the most frequent cause of death when it has been taken. Hence arises the highly important practical distinction, that whereas everything necessary to arrest the destructive effects of a mineral acid is its dilution or neutralisation, oxalic acid to be rendered harmless must not only be neutralised (dilution having quite the opposite

effect), but become insoluble, as its soluble salts are quite as poisonous as itself. There are certain other notable peculiarities connected with oxalic acid to which it is desirable to refer, such as the comparatively speaking large dose required to produce fatal effects, and the rapidity with which these result after taking the poison. The smallest fatal dose recorded is sixty grains, but the average amount necessary to destroy life is about half an ounce or an ounce. When such a quantity is taken, death follows in a very short time, in one case, recorded by Dr. Ogilvy, within three minutes, and in most cases within an hour. Death is not, however, invariably so speedy, for in a case recorded by Dr. Page the patient lived till the fifth day. In this instance, strangely enough, the stomach-pump was employed for getting rid of the offending substance—a practice to be greatly deprecated, owing to the local effects of the poison. It was not till 1814 that this substance was known as a poison, and the first thorough investigation it underwent was at the hands of Christison and Coindet. These gentlemen came to the general conclusions with regard to its operation referred to above; but their statements with regard to its special action, local or general, according to the amount of dilution it has sustained, have not been entirely confirmed by subsequent observers. There is no doubt, however, that this acid does exercise a very powerful influence upon the nervous system, and as little that this is the most frequent cause of death, the fatal result appearing to depend upon the intense prostration which the poison produces. This is probably also the cause of the weak voice and sight sometimes noticed, though Dr. Taylor seems inclined to refer the former at least to a special effect of the poison. Death being usually speedy, diarrhœa is seldom observed except life be prolonged for some days, and the post-mortem examination usually shows that the stomach has been more immediately affected than any other part of the body. According to Christison, this poison attacks the gelatinous tissues in an especial manner, and reduces the stomach in some cases to a pulpy consistence, occasionally even perforating it. This last result is considered by Dr. Taylor to be very uncommon; but he seems to have overlooked one of the best cases we have seen recorded, viz., one (with plate) narrated by Dr. Littlejohn of Edinburgh, in the 'Edinburgh Medical and Surgical Journal' for July, 1861. In an American case given by Dr. Wood, there seems to have been some doubt as to how much of the opening was due to the effects of the poison, and how much to the act of removal, but in the case just referred to Dr. Littlejohn took great care to observe the parts *in situ*, the result being that an extensive aperture was found in the greater curvature of the

viscus. The rectum was also found to be inflamed, although the small intestine, with the exception of the duodenum, was healthy.

A very unique case was tried in Scotland (vide 'Edin. Med. and Surg. Journal,' vol. viii, p. 93) a few years ago, the subject of the trial being a woman, who was said to have caused the death of a very near relation by giving him small doses of oxalic acid from time to time. Vomiting usually occurred about an hour after each dose was administered; but the suspicions of the neighbours were aroused before the case ended fatally, and the old man was removed and placed under proper treatment, but died from irritation of the bowels fifteen days after the last dose. This very curious case seems also to have been overlooked by Dr. Taylor.

In one way, poisoning by oxalic acid comes home to us all, as maid-servants are often accustomed to clean copper vessels used for cooking with it. In a case which came under our own observation, a whole family was seized one night with symptoms of irritant poisoning, in one or two of a rather severe character. No food had been taken after tea, and the only substances used by them all in common were tea, bread and butter. These last were examined, but nothing faulty could be detected in them. Part of the matters vomited, which were of a dark-brown colour, had, however, been preserved, and on testing this a small quantity of oxalic acid was discovered. The same day, the medical men in whose practice the accident had occurred, announced that they had just discovered the cause of the mishap, in the fact that a servant had cleaned the copper kettle used for boiling the tea water with oxalic acid, and had not been at sufficient pains to remove every trace of the poisonous substance.

The other irritant vegetable acids are not of any very great importance from a toxicological point of view; their action is very much milder than that of oxalic acid, though some observers seem to think that citric and tartaric acids resemble it in kind if not in degree.

Although poisoning by *phosphorus* is in this country a somewhat uncommon event, abroad it is far otherwise; for in Germany and France it would seem, more especially of late years, to be employed more frequently than any other toxical agent, especially for the purpose of suicide. This may, to a certain extent, depend on the comparative facility with which it is procurable, most other poisons lying under police restrictions as to their sale; but there is without doubt a fashion in these things as well as in matters of dress and ornament, and at the present moment phosphorus would seem to be the fashionable Conti-

nental poison. It is most frequently used as such in the form of paste, a preparation employed for killing rats, or it is separated from the extremities of matches by soaking in water. Sometimes it is taken in the solid state, sometimes dissolved in oil. But the poisonous effects of phosphorus may also be produced in an altogether different way, as may be seen among the workmen exposed to its fumes in a badly-ventilated match manufactory. Two well-marked kinds of poisoning by phosphorus are thus indicated,—the one intentional or accidental, the other what might be called *technical;* or, which is probably the better distinction, the one acute, and the other chronic. But of the former, two very well-marked subdivisions have been pointed out by Falck: the one he terms *Phosphorismus intestinalis,* the other *Phosphorismus cerebro-spinalis,* the division depending on the peculiar character of the symptoms evolved in the different cases. The account given by Dr. Taylor of poisoning by phosphorus is far from satisfactory; but it must be remembered that the rarity with which cases occur in this country renders it, comparatively speaking, of little importance that several points should be omitted, while the nature of the volume demands the utmost condensation. With regard to the action of phosphorus applied to the external surface, little need be said: it produces burns of a very troublesome character, as we ourselves have seen in a case where a strong solution of phosphorus in oil came in contact with the skin, and a case recorded by Mertens would seem to indicate that when applied to a raw surface, gangrene may even be the result. This action on the external surface furnishes the key to certain, but by no means the whole, of the symptoms which phosphorus produces when taken internally, for when the poison is exhibited in substance, they are readily referable to causes similar to those just referred to. When, however, it is given in solution, or even after a time, when the solid substance has been employed, it is absorbed and sometimes even excreted without any change: thus Casper refers to one case (CCVI) where luminous vapours were exhaled even from the vagina and anus, and numerous instances are recorded where some one or other of all the secretions evidently contained the poison in substance. As above indicated, the train of symptoms usually observed varies excessively, the dose and the form in which it is administered being the most important modifying agents. To take the division of Falck, although his two forms are very, if not most, frequently combined, we find that in his former division pain and distress begin in the stomach, and gradually extend over the abdomen. Vomiting and purging of matters luminous in the dark, especially on heating, are of very common occurrence; but blood is comparatively seldom seen in these dis-

charges. When the phosphorus is absorbed, almost every one of the excretions becomes more or less luminous; even the breath assumes this characteristic. The vascular, circulatory, and nervous systems begin to respond to the attack made upon them, partly as constitutional reaction, but partly also as a direct effect of the poison. This seems at first to stimulate the nervous system, and through it the others just mentioned, but only for a very short time if the dose be excessive, for depression soon follows; the pulse is quickened (to as much as 140 beats per minute sometimes), becoming at the same time very irregular and scarcely perceptible; the respirations are as numerous as 56 per minute, and stertorous in their character, while the temperature of the air expired is but little above that taken into the lungs. In this variety of poisoning by phosphorus, affections of the kidney are by no means uncommon. Nitsche says that the urine is usually albuminous, and contains fibrinous casts. In many instances, the sexual organs become excited, both in males and females; but Ehrle thinks that this excitation is produced by medicinal (as contradistinguished from poisonous) doses only.

The other variety of poisoning by phosphorus is usually observed in cases where the dose has been very small, or the poison has been given in solution.

With regard to the cause of the variation in the symptoms, various opinions are afloat; some think it is owing to the conversion of the metalloid into phosphorous and phosphoric acids, while others think that phosphoretted hydrogen is the poisonous agent. Be that as it may, although, as will be seen, it has a rather important bearing on the treatment to be adopted, there can be no doubt but that the appearance of tingling and weight of the limbs, gradually passing from the lower extremities to the upper, more especially when the sight becomes affected (in three cases out of twenty-two—Ehrle), is of very evil omen. Although the mind is but little impaired at first, the head is by no means unaffected; anxiety and restlessness are of common, if not universal occurrence, the pupils are dilated, and the patient becomes wholly apathetic. Convulsions are not unusual, and if they appear at all, usually give place to paralysis, general or partial, which rapidly closes the scene. Altogether this variety of poisoning by phosphorus is more dangerous in its character than the former, but from neither is recovery immediate—irritability of the alimentary canal commonly following the former, and paralysis frequently succeeding the latter. Where a post-mortem examination is made of the body of one who has died by phosphorus, there are certain rather unusual appearances which may be looked for, such as extensive ecchymoses on the

surface of the body from disorganisation of the blood, which, according to Casper, contains all the colouring of the red corpuscles in solution. In one case recorded by Möller, these sugillations were so strongly marked as to be taken for the effects of scurvy. Sometimes the whole skin appears jaundiced from a peculiar affection of the liver occasionally seen in poisoning by this substance. The alimentary organs are very variously affected in different cases, but both stomach and bowels are usually inflamed, although erosions are rather rare. The heart generally contains but little blood, whilst the other viscera are usually hyperæmic. The liver seems to undergo in many instances a very rapid fatty degeneration somewhat similar to that known as "acute atrophy," whilst in other instances it is enlarged and congested. V. Hauff affirms that such degeneration is seen in eleven cases out of nineteen; but other authors seem to think that the conjunction is accidental, too little time being allowed for such a change after taking the poison.

Poisoning, at least in the acute form, by phosphoric vapours, is not of very common occurrence, and when it does occur the symptoms are by no means invariable: of course, bronchitis or bronchial catarrh is most frequently observed, but these are often accompanied by gastric complications. In other cases again, as observed by Dietz, the circulatory organs may be affected; whilst in a case recorded by Huss, the cerebro-spinal axis was evidently implicated, the sexual organs being also excited, but the symptoms of stimulation soon gave way to those of depression and paralysis. From acute poisoning by phosphorus in the form of vapour, the transition is very natural to chronic *intoxication* by the same meaus. This peculiar affection was first observed by Lorinser in Vienna in 1845; but so many others have since recorded cases, that it stands as well authenticated as if it had been known for hundreds of years. The part that appears to be almost if not altogether invariably attacked first of all, is the lower jaw; but this would seem to be unusual as long as the teeth remained sound, and the vapours could not obtain direct access to the bone. Strangely enough, the upper jaw is rarely if ever affected. The exact cause of this strange disorder has been much disputed: some French authors maintain that the phosphorus has nothing to do with it; some say it is the constant draught of the workshop: Martius maintains that it is the arsenic contained in the phosphorus; Ebel, that it is merely the effect of scrofula; while Bibra holds that the ozone set free by the phosphorus is the active agent. To all of these views counter arguments are readily procurable, and the real cause seems to be the solution and oxidation of the phosphoric vapour in the saliva, with the

direct application of the acids thus produced to the jaw, laid bare by the diseased tooth. Along with this local manifestation, there are often indications of a general dyscrasia by no means of a pleasant kind. As an indication that caries of the jaw in any case depends on phosphorus-poisoning, Lorinser is inclined to put great weight on the appearance of osteophytes at the beginning of the disorder.

As to the treatment of chronic poisoning, it may be said that with sufficient care as to sanitary regulations it will rarely, if ever, occur; for acute poisoning by phosphorus, no antidote really exists. As already indicated, there is considerable difference of opinion as to the exact nature of the products of its decomposition: some hold that phosphoretted hydrogen is the worst of these, and that when this is decomposed the great danger is over, to this end prescribing an oxidizing hypochlorite, as of magnesia. Others, again, say that the products of the oxidation of phosphorus are most to be dreaded, and prescribe plain magnesia. A third division combines the treatment of the other two, and holds that the hypochlorite of magnesia with excess of magnesia is the only safe remedy. Certain it is that any substances capable of dissolving up the solid phosphorus, such as oils of every description, should be carefully avoided.

In the recently published work of Hoppe Seyler, we have at the hands of Dybkowsky an account of his, the latest, researches into the poisonous action of phosphorus. He maintains that the toxical agent is phosphoretted hydrogen, not the kind ordinarily produced and which is spontaneously inflammable, but another variety not so easily affected by the outer air, and which is therefore possessed of far more fatal effects than the one which can only act by the phosphoric acid which it produces by its own decomposition. Phosphorus, either in substance or combined with hydrogen, is absorbed from the stomach into the vascular system, where it is oxidized as long as any oxygen is left in the blood, which being thus depurated, becomes unfit to nourish the body, death being the consequence. The blood-corpuscles are destroyed, the colouring matter taken up by the blood, and the superfluous phosphoretted hydrogen sometimes exhaled by the lungs: if found in the urine, it is as phosphoric acid, and phosphorus, as such, is seldom detected in the blood.

Various processes have been recommended for the detection of phosphorus either in the *ejecta* or in the post-mortem remains. Of course, the simplest is the separation and demonstration of the phosphorus in a solid form, which may sometimes be done by careful washing with hot water. Should, however, the substance be very much divided, one or other of two plans may be

adopted—either agitating the suspected substance with bisulphide of carbon, as recommended by Schneider and adopted by Taylor, or the method of Lipowitz may be employed. The latter consists in heating the suspected fluid along with certain pieces of sulphur at a temperature not exceeding 150° Fahr., by which means a union is determined between the two, and the phosphorus thus removed may be converted by hot nitric acid into phosphoric acid, the presence of which may be determined by the ordinary tests. But neither of these processes is so certain or so delicate as that proposed by Mitscherlich, more especially as now modified. The mode of detection first pointed out by this chemist consists essentially in heating the suspected substance along with sulphuric acid in a flask, from the cork of which a tube leads to a jar constantly kept cool by a stream of water. Here the vapour of the phosphorus condenses, and in so doing produces a flash of light readily observed if the room be partially darkened. The two methods of Lipowitz and Mitscherlich readily admit of combination, as pointed out by Van Hasselt, the one simply preceding the other; while a third test, discovered by Dusart, may be superadded to these two. This test (probably the best), as modified by Blondlot, consists in adding to the fluid which has been used for Mitscherlich's test (for the presence of certain vapours may prevent the luminosity and thus deceive the operator) a little more sulphuric acid and a few pieces of pure zinc, when the hydrogen thus set free will combine with the phosphorus and form phosphoretted hydrogen. This is to be conducted into a solution of nitrate of silver, which will be precipitated chiefly as metallic silver. The precipitate is to be washed, and the phosphorus, by means of the zinc and sulphuric acid, is again to be transformed into phosphoretted hydrogen, which, however, is this time to be burnt. This it does with a brilliant green flame. By this process Blondlot affirms that he was able to obtain highly characteristic results with two match-heads. Other tests have been proposed; one by Wiggers, to convert the phosphorus into hypophosphite of potash and phosphoretted hydrogen by means of caustic potash, and another to cause it to flame up below the surface of its containing fluid by means of sulphuric acid and chlorate of potash. We have, however, said enough to show that although but little has been done to elucidate the subject of poisoning by phosphorus in this country, it has received quite its due share of attention on the Continent.

Poisoning by the caustic alkalies and alkaline earths is, comparatively speaking, a rare event; and as the subject is very well discussed by Dr. Taylor, we may at once dismiss it with a solitary reference to a curious case of what, however, could

scarcely be called poisoning, reported in a former number of this Review by Prof. Ogston. The subject was a boy, who was seized by a labourer, and held with his face thrust into a heap of quicklime for some time. He was in consequence seized with violent inflammation of the respiratory tract, from which, however, after tracheotomy, he eventually recovered. This case is, we believe, unique in its way.

On the subject of poisoning by the salts of the alkalies, more especially by saltpetre, we have a few observations to make. For a long time nitre was supposed to be harmless, an opinion endorsed by the authority of Tartra, but recent investigations and cases leave no doubt of its poisonous properties when given in tolerably large doses, the fatal event often occurring in a shorter time than in poisoning by arsenic or antimony. Fifteen cases are recorded (Husemann), whereof four have proved fatal, but the number has been increased since the publication of the above-mamed author's work. The ordinary symptoms of poisoning by nitre are those of irritation, conjoined with a certain effect which it appears to exercise on the nervous system. Vomiting and purging of bloody matters are usual symptoms, combined in some instances with diuresis, or even hæmaturia (Deutsch); but there is also loss of strength, apathy, hallucinations, disordered sight and speech, convulsions, &c. To these last, rather than to the former, the patient, if not relieved, usually succumbs in two or three hours. Nor is the dose required to produce such symptoms a very large one, although probably larger than could be taken without suspicion should it ever be employed for the purpose of murder, since in a case recorded by Chevallier, not very long ago, the dose was an ounce, and death occurred in two hours. In this case, which is not mentioned by Dr. Taylor, the stomach was greatly congested, its walls contained extravasated blood in some quantity, and were partially destroyed by numerous erosions. Most frequently cases of poisoning by nitre occur by the substitution of it for some one or other of the purgative salts, most likely sulphate of soda, whose crystals it somewhat resembles. No cases have yet been brought forward of its employment for suicidal or homicidal purposes, its dangerous character being unknown to the public. With regard to the effects of iodide of potassium, the opinions of medical men are also greatly divided; but one case recorded by Clarus, to say nothing of those mentioned by Dr. Taylor, where troublesome effects arose from the exhibition of thirty-six grains, shows that it is not to be tampered with to a very great extent. We do not, however, propose to enter here into this subject, or that of iodism—*i. e.* the effects of long-continued medicinal doses of iodine or some of its com-

pounds. We may refer our readers to Dr. Taylor's work for some interesting cases of the irritant effects of iodide of potassium, but meantime we must hasten on.

Poisoning by a salt of barium is not by any means common; yet those cases which have occurred, as well as the results of experiments on the lower animals, are so important as to excite great interest. Probably, from the symptoms it excites, it would be most reasonable to place it among the narcotico-acrids, although it is customary to collocate all mineral poisons. Christison says that neurotic symptoms occur more constantly as a consequence of the exhibition of baryta than from any other inorganic poison. At one time it was the subject of much inquiry, but it has for a time attracted little attention, insomuch that Dr. Taylor, in the last edition of his work 'On Poisons,' makes but a passing reference to it; although in his recent work a short account is given of its toxical effects. These may be said to be of two kinds, the local and the constitutional or general, the latter only being observed when the poison is directly introduced into the system. Of the former we need say nothing, but the latter are of some importance. All the older, and in fact we may say also the most recent observers, state that this substance has a powerful influence upon the nervous system, but that the immediate cause of death after its exhibition is arrest of the action of the heart. With regard to the kind of influence it possesses on the nervous system, various opinions prevail, some stating that convulsions are invariable, others that paralysis only is observed: still, there can be no doubt that it does act upon these centres, the only doubtful point being the way in which the effect is produced. Very recently (Virch., 'Archiv,' Bd. 28), an account of a series of investigations conducted under the superintendence of Hoppe Seyler in Tübingen, so rapidly becoming famous for medico-chemical research, were published by Onsum. This gentleman asserted that baryta was merely a mechanical poison, that it caused death by precipitating the sulphates which exist in the blood, and that the sulphate of baryta thus formed accumulating in the pulmonary capillaries gave rise to something like embolism, cerebral disturbance, and death. Such a conclusion was quite opposed to the researches of Brodie and Blake, who maintained that it had a direct influence on the heart, and even to the opinion of Orfila, who held that it acted chiefly on the nervous system. Only one means existed of establishing the truth of this doctrine or the contrary, viz., actual experiment; and this has been resorted to by Dr. Cyon (vide Reichert and Du Bois' 'Archiv,' 1866, No. 2), but with the latter result. In the first place, only a very small quantity of alkaline sul-

phates exist in the blood, and even when this was very considerably augmented by injection, no such thing as a precipitate could be detected in the lungs, still less when the blood was in its normal condition. Cyon was thus able to come to the conclusion that this precipitation was certainly not the cause of death, which had therefore to be ascertained. All, with the above indicated exception, are of opinion that paralysis of the heart is the immediate cause; but this might be brought about in various ways—either from the direct action of the poison on the muscular tissue, by paralysis of the sensory or motor nerves, or of the governing nerve centres. The muscle, however, was found to react properly, and the nerves to convey stimuli after poisoning by a salt of baryta, so that it would appear to destroy life by annulling the power of the nervous centres. A somewhat similar theory of the action of oxalic acid upon the system was advanced by the same gentleman (Onsum), viz., that it kills by precipitating the lime-salts contained in the blood, which accumulating, obstruct the pulmonary capillaries. The same method of disproof was had recourse to by Cyon, and with a like result; for he found that whilst there was nothing to indicate the existence of pulmonary embolia, there was every reason to believe that the sulphates in the one case and the lime in the other were powerful in retarding rather than in causing the fatal event.

The subjects that next invite attention are the four great mineral poisons, arsenic, antimony, mercury, and lead. It might, in fact, be said that up to a comparatively recent date the history of these four, especially of the first, constituted the history of poisoning, particularly with regard to this country; though latterly vegetable poisons, of which we shall speak anon, have to a considerable extent usurped their place in criminal cases. The plan taken by Dr. Taylor in treating of arsenic is to describe first of all the characteristics of poisoning by arsenious acid, following this up with notices of the other poisonous compounds of the metal. Probably a better plan, seeing that the symptoms produced are so much alike in all these, would be to discuss first the various substances containing arsenic which are capable of giving rise to toxical symptoms, and so lead on to an account of their local or general manifestations. Of course, the most common cases are those arising from the administration of arsenious acid, arsenic acid, or some of the more soluble compounds of the metal; but besides these we have examples of poisoning by insoluble arsenical preparations, as metallic arsenic itself (?), the sulphides (orpiment and realgar), arsenical green pigments, as well as the more uncommon substances, chloride of arsenic, arseniated hydrogen, and kakodyl.

That pure metallic arsenic is poisonous, has yet, we think, to be proved. On the one hand, Schroff maintains that it is so; whilst, on the other, Schmidt and Bretschneider hold that it is not when perfectly pure, and affirm that Schroff's mistake arose from using an impure substance, in which they are borne out by the experiments of Bayen. Certain it is, however, that the impure metal, as ordinarily found, is highly poisonous, for numerous cases are recorded where fatal effects have arisen from its use or from that of a compound of cobalt, sometimes as well as the ordinary impure metal called "flypowder," containing it. There is one case mentioned by Dr. Taylor where it was used for the purpose of murder, and another (by Deutsch) for that of suicide. The sulphides of arsenic, which are insoluble in water, are not so, however, in the juices of the stomach, so that cases of poisoning by them are occasionally met with, more especially in India (Chevers); but there can be no doubt, as Dr. Taylor points out, that their destructive properties (especially in the case of orpiment) depend chiefly on the amount of arsenious acid with which they are usually contaminated. To this same division of insoluble compounds of arsenic belong the substances so often employed as green pigments for wall-papers, tarlatans, artificial flowers, &c., more especially Scheele's, Brunswick, and Schweinfurth's greens. As to the dangerous effects produced by these there can now be no doubt; but as to how these are immediately produced there is still some difference of opinion. Some authors have supposed that the colouring matter was converted into arseniated hydrogen or into kakodyl before it gave rise to the symptoms of *intoxication*, some affirming that the light of the sun direct or indirect, others that the carbonic acid exhaled from the lungs combined with ammonia (Kleist) gave rise to this gas, and so to the symptoms of poisoning. Notwithstanding this, even supposing they were not supported by the exact researches of Krahmer, we should be inclined to adopt the view of Dr. Taylor, who maintains that the symptoms are due to rubbing off particles of the colouring matter, which, forming an impalpable powder, are in this state or form inhaled into the lungs. This opinion is still further confirmed by the experiments of Schmidt and Bretschneider (Moleschott's 'Untersuchungen,' vol. vi), who found by direct investigation that no arseniated hydrogen was developed from these coloured papers when exposed to the ordinary atmosphere of a dwelling. It must further be borne in mind that all arsenical wall-papers are not equally dangerous: the most so are those which have the colouring matter but very imperfectly adherent, and which presenting a smooth surface, are easily affected by rubbing;

whilst many of the flock-papers may be considered at least tolerably safe.

Poisoning by arseniuretted hydrogen is far from a common event, insomuch that Dr. Taylor speaks of it as having caused death but four times altogether. The most recent of these, so far as we know, occurred in the practice of the French physician Piorry, the subject being a chemist working in a laboratory, into which he had allowed a quantity of arseniated hydrogen to escape. Pain in the head and epigastrium, with vomiting and prostration of strength, compelled him to suspend his labours; he was carried to La Charité, where the majority of the symptoms, although at first exacerbated, gradually improved. Urine was passed at first, but suppression soon came on; this was relieved on the third day by a slight discharge of reddish urine, but no more appeared, and the sufferer died on the fifth day. In this case the colour of the skin became strangely altered, being first jaundiced, then bronzed. There is no mention of purging in connection with the case, nor of morbid appearances in the bowels; the liver and kidneys were congested, but the cells of the former were unaltered ('Gaz. des Hôpitaux,' 1863, 128). Death seems to be longer in following the inhalation of the poison than when it has been swallowed.

But all of these poisonous compounds of arsenic are of secondary importance, arsenious acid, or, as it is often called, white arsenic, is the one that causes more deaths than all the others put together, and to it we must chiefly direct attention.

In his classical work on Poisons, Dr. Christison divides the symptoms usually observed as resulting from the exhibition of an over-dose of arsenic into three groups. The first and most ordinary set of symptoms is that resulting from irritation or inflammation of the alimentary organs; the second, that usually considered characteristic of the action of a narcotic, where signs of inflammation may be almost or altogether wanting, and death results in five or six hours; and third, that where the ordinary symptoms of inflammation are succeeded by those of nervous irritability.

Dr. Guy, again, describes four of these divisions. The first of these comprehends all cases of the ordinary type of arsenical poisoning; the second, those characterised by collapse and rapid death: in the third set, coma is the most marked feature; whilst the fourth has but few distinguishing points from ordinary English cholera.

Falck, whose division has been generally adopted throughout Germany (by Husemann, Böcker, &c.), divides all cases of poisoning by arsenic into two groups—the acute and the chronic;—the former, and more important, consisting of four

subdivisions. The first of these, where the skin is chiefly affected, as by local applications, is termed *Arsenicismus cutaneus acutus;* the second, where the symptoms are those of inflammation of the *primæ viæ,* he calls *Arsenicismus intestinalis acutus;* the third group consists of all those cases where cerebro-spinal symptoms are most manifest—this is denominated *Arsenicismus cerebro-spinalis acutus;* whilst the fourth is commonly known as *Asphyxia arsenicalis.*

Dr. Taylor, again, adopts no classification whatever; and in this he is to a certain extent right, for it must be remembered that there is no exact division or line of demarcation between one group of symptoms and another; but, looking at the question in another way, we would be of opinion that were it only for the convenience of remembering what one may look for, as well as the advisability of accepting any classification, however imperfect, provided it be clearly understood to be merely provisional, that one or other of these groupings should be adhered to.

We propose adopting the arrangement of Falck, chiefly because of its having well-marked names applicable to the various divisions, partly also on account of its extensive employment on the Continent, and, lastly, on account of the ease with which it may be made to correspond with our English system.

The first of these divisions, *Arsenicisimus cutaneus,* comprehends two sub-classes—the acute and the chronic, the former occurring but seldom when there is no breach of surface, the latter being excessively common among all workers with arsenical preparations. These Dr. Taylor essentially recognises, though not by name; and we may safely refer our readers to his work 'On Poisons' for any special information they may require. One thing should be remembered, which is the fact of arsenic affecting the alimentary organs even when absorbed through the skin; so that vomiting and purging must not be taken as a certain indication of the poison having been taken into the stomach. As might be expected, the great majority of cases of arsenical poisoning begin with symptoms of gastro-enteritis; but it is comparatively rare to find these and these alone persisting until death, for most frequently some nervous symptoms are superadded. Another very common complication appears to be nephritis, the urine being usually diminished in quantity, and most frequently containing fibrinous casts. Dr. Taylor does not allude to this fact, only mentioning that the quantity of urine passed varies considerably. One very important fact, however, is pointed out by this author, viz., that as arsenious acid is usually sold in this country mixed with soot or indigo, the vomited matters may assume any colour capable

of being formed of these in combination with the colouring matters of the bile.

Turning next to the cerebro-spinal, or, as it is termed by Van Hasselt, the paralytic form of poisoning by arsenic, we find most Continental writers insisting on the connection of this with the form in which the poison is exhibited, putting the quantity or dose altogether out of sight; but Dr. Taylor demurs to this, and in confirmation of his opinion cites a number of cases of a very puzzling caste. It cannot, however, we think, be denied that in most cases the symptoms do bear a certain relation to the quantity absorbed and the rapidity of the absorption: when these are great, the form just mentioned or the next is most likely to occur; if not, which usually happens when the substance is exhibited in the solid form, irritant symptoms are likely to predominate. Pains in the head and joints, cold extremities, prostration, delirium, coma, anæsthesia, partial paralysis, convulsions, ending in general paralysis and death, constitute the usual train of symptoms in the third form, which usually but not invariably begins with vomiting, although the post-mortem examination seldom shows traces of irritation. Falck's fourth and last form, or arsenical asphyxia, corresponds in a great measure with the last division of Dr. Guy, and appears to occur when a large quantity of arsenic has been very speedily absorbed into the circulatory system. The symptoms tolerably closely resemble those of cholera, and the post-mortem appearances are such as indicate death by asphyxia, viz., congestion of the lungs and abdominal viscera, and accumulation of dark syrupy blood in the right side of the heart. Besides these, there is, of course, the chronic form of arsenical poisoning, which may be either accidental or intentional. To the former class belong those unfortunate cases where arsenic has been inadvertently absorbed for some time, as from wall-papers, &c.; and no better instance of the latter can be given than a case recorded by Flandin, and transcribed by Dr. Taylor ('Poisons,' p. 363). Accidental poisoning in an acute form is by no means uncommon, arsenious acid having been mistaken for Epsom salts; and in one extraordinary case, mentioned by Dr. Ogston, a quantity of arsenic belonging to a rat-catcher was mistaken for flour. A party of women made it into a cake, of which they all partook; but, fortunately, vomiting was so prompt that they all escaped with their lives. Mistakes have also frequently occurred by sending wheat which had been pickled (washed with arsenic), for sowing, to the mill. In one instance recorded by Köffler, 183 persons were thus affected, of whom one died. If, however, we are to accept Dr. Draper's statements, most of our flour contains arsenic; for he says that all wheat cultivated with

phosphate of lime contains the poison, having received it from the impure hydrochloric acid used in preparing the above-named manure. One very remarkable subject connected with this substance is that of the arsenic-eating peasants in Styria. Great doubt has been thrown upon the existence of such a custom, and it has been usually affirmed that habitual use does not weaken, but rather strengthens, the effects of mineral in contradistinction to vegetable poisons—notably, alcohol, hachish, and opium. The recent investigations of Dr. Craig Maclagan must, however, set the matter at rest if it is possible to do so, since the above-named gentleman saw a man eat a poisonous dose without any evil effect, and was able to detect arsenic in his urine afterwards. (See also Boston 'Med. and Surg. Journal,' June, 1866.) These people are also said to give arsenic to their horses, on the ground that it improves their coats and tends to fatten them. But how can these things be? and how can one bring himself to take a poisonous dose of arsenic two or three times a week with impunity? It cannot be done, however, with invariable safety; for a case is recorded by Dr. Packer, of Halifax, Nova Scotia, where a photographer labouring under secondary syphilis, and consequently in weak health, hearing of this Styrian practice, had recourse to it, thinking it would improve his bodily condition. He began with small doses, but rapidly increased these to two or three grains a day, sometimes even exceeding the latter quantity. Symptoms of poisoning, mostly of a neurotic character, however, supervened, and the patient rapidly sank. The stomach, more especially towards the pylorus and the duodenum, was much congested, the latter almost black, and containing effused blood in its walls: the rectum was also inflamed.

Any attempt at reply to the question just put, necessitates an inquiry into the mode in which arsenic acts upon the body. Heller and Liebig supposed that it decomposed the albumen of the blood and tissues; whilst Edwards, Rendall, and Herapath, suppose that it forms a compound with it—the arsenite of albumen. More recent investigators, however, as Schmidt, Bretschneider, Stürzwage, and Cunze, have founded altogether another theory upon their observations, by which they believe themselves capable of accounting for both arsenic-eating and arsenical poisoning. The investigations of the two first-named authors began with a refutation of the theory advanced by Schroff to account for the phenomenon of arsenic-eating, which he did by supposing that arsenious and arsenic acids, when taken into the system, formed insoluble compounds with some of the lime-salts already there, and were thus prevented from doing any mischief. This they succeeded in showing was incorrect, as both arsenite and arseniate of lime,

when given as such, are poisonous salts. The next point was to ascertain whether the arsenious acid underwent any change while within the body, for some supposed that it was converted into arsenic acid. To settle this definitely, a horse—an animal singularly insusceptible of the action of arsenic—was selected and dosed with the poison for some days; but neither during life nor after death was there any indication of a change of arsenious into arsenic acid either in the blood or in the urine. When the coagulated blood was examined, no trace of arsenic could be detected in the serum, although abundance was discovered in the clot. Hence it was shrewdly suspected that arsenious acid might have some peculiar action on the corpuscles, some of which were accordingly exposed to the action of a solution of it, when it was found that after some days no change had taken place; whereas some which had been exposed to the action of water only had become greatly decomposed. The same preservative effect was observed when a piece of intestine was exposed to the influence of a similar solution. These results, combined with the antiseptic effects of arsenic so well known to all, opened up a new field for investigation. That arsenic prevents putrefaction by opposing oxidation was proved by subsequent experiments of the same investigators. But this destructive process may be hindered in at least two ways, either by combining with the oxygen itself, which, as we have seen, it does not, or by rendering the animal tissues less combustible, which is more likely. A series of inquiries were instituted by Schmidt and Stürzwage as to the effect of small quantities of arsenic on the changes which food undergoes in the system, the results of which have been subsequently confirmed and applied by Cunze ('Zeitschrift für rationelle Medizin,' 1866, Heft i). A number of animals were subjected to experiment, whereby it was universally found that the quantities of carbonic acid and urea usually excreted, were materially lessened by the exhibition of arsenic, and the rapidity with which this diminution was effected depended entirely on the rapidity with which the poison became mixed with the blood. From these observations we may deduce the obvious inference that it is owing to the diminished formation of urea and carbonic acid, that such a saving of material as is implied in the reported good effects from arsenic-eating results. Cunze established certain other facts—viz., that he temperature is always materially lessened after a dose of arsenic, which is simply the corollary of the diminished oxidation of Schmidt and Stürzwage. But he further proved that the part to which arsenic is applied, as in the case of the frog's heart, remains longer capable of action than when left alone, provided only a small quantity has been employed. Should, however, the quantity be very great,

the saving becomes in excess; in other words, no action can take place, the part is paralysed, and death may result. By these inquiries we are enabled to understand the different sets of symptoms produced by poisonous doses of arsenic somewhat better, but renewed investigations are still necessary before we can see our way quite clearly.

As to the post-mortem appearances usually observed in poisoning by arsenic, they, as might be expected from the symptoms, vary greatly; but, as might also be anticipated from the rearches of Schmidt and Cunze, one of the most marked is a tendency to resist putrefaction almost universally observed. The varying aspects of the stomach and intestines after death are well described by Dr. Taylor in his work 'On Poisons,' who further tells us that perforation of the stomach very rarely occurs in poisoning by arsenious acid, and this is no doubt true of the human subject and of such animals as can vomit, but we are by no means sure of such being the case with animals which do not possess the power referred to.

The treatment of poisoning by arsenic is not discussed by Dr. Taylor in his recent work, and is only very shortly given in that on Poisons: as there laid down, it may be shortly summed up as the production of vomiting or the use of the stomach-pump; hydrated oxide of iron, if the poison be in solution, but no diuretics. (?) To promote or cause vomiting, this gentleman recommends the more irritant emetics, as the sulphates of zinc and copper; but in this respect we are inclined to disagree with him, giving in our adherence to our Continental brethren, who limit themselves to ipecacuanha. But it must never be forgotten that vomiting alone may be totally insufficient to expel the poison, and a case occurs to us where this was strikingly manifested. A man had taken a large quantity of arsenious acid for suicidal purposes, but was not very long after subjected to skilful medical treatment, which, however, did not benefit him much, for he died after vomiting almost incessantly for upwards of twenty-four hours. Nevertheless, on examining the stomach distinct patches of arsenic could be detected adhering firmly to its walls, notwithstanding all the efforts made to expel the offending substance. The chemical treatment by the use of sulphides with the intention of converting arsenious acid into orpiment has now been abandoned; whilst in America the use of tobacco as a physiological antidote has been had recourse to, though with no very great success.

Turning to the chemical detection of arsenic, we find that Dr. Taylor has pretty well exhausted the subject: there are, however, a few points on which we may make some observations. As a general rule, arsenic has to be sought in combina-

tion with various organic substances, and various plans have been recommended for separating it, some of which require the preliminary destruction of the organic matter, some do not. Of the former a very good illustration is afforded by the well-known process of Fresenius and Von Babo, where the organic matter is oxidized by acting upon chlorate of potash with pure hydrochloric acid. In the original process there are several other steps, so that it is but seldom had recourse to in its entirety, but most certainly no other plan is half so useful in getting rid of troublesome organic compounds. Strangely enough, it is not even referred to by Dr. Taylor in his recent work, and in that on Poisons he does not give it the credit it deserves. Another process, sometimes known as that of Orfila and Jacquelain, provides for the destruction of such substances by passing a stream of chlorine gas through them. Wöhler has improved upon this scheme by digesting in caustic potash for a time, and gently acidulating by sulphuric or hydrochloric acid previous to passing the gas. D'Anger and Flandin oxidized by sulphuric acid, Orfila by nitric acid, Rapp by saltpetre (especially useful as improved by Wöhler when a large amount of putrid matter has to be examined), whilst Devergie and Gaultier de Clautry prefer nitrate of lime. Two processes which in a modified form appear to be great favorites with Dr. Taylor, require no previous preparation; they are Reinsch's and Schneider's. We should have thought that his experience in the Smethurst case would have inclined the gentleman just mentioned to look upon the former as a final test with some considerable degree of suspicion. True, no better test can be employed as a preliminary to another process, but we certainly prefer a modification of Fresenius and Von Babo's to Reinsch's as a conclusive means of determining the presence of arsenic. In the practice of Reinsch's process by those who are not accustomed to employ it, certain precautions mentioned by Dr. Odling in his excellent little work on 'Practical Chemistry' should be taken, viz., to arrest the action of oxygenants by sulphite of soda, and to test the purity of the acid and copper employed. The latter, he says, is best done by distilling a few fragments of the copper to be employed, with some acid perchloride of iron, when the arsenic, if present, may be obtained and tested in the form of chloride. Dr. Taylor, on the other hand, recommends the exposure of the copper along with hydrochloric acid for some time, and its subsequent distillation in order to collect the chloride. One very good practical hint given by the last-named author is that the operator should, when testing by Reinsch's method, not be in too great a hurry

to remove the copper slips from the suspected liquid; but it must also be remembered that prolonged boiling, especially when fats are present, gives a tarnished appearance to the copper slightly resembling a very minute metallic deposit.

But the process that Dr. Taylor seems to rely on most is a modification of that generally known as "Schneider's process," differing from it only in as far as hydrochloric acid, and not sulphuric acid, along with common salt, is employed. The substance to be tested must always be thoroughly dried before adding the acid, which must also be highly concentrated, the chloride of arsenic not being formed with the dilute acid under ordinary circumstances. Although this is usually accounted a foreign process, we are by no means certain that it is so in reality. Only having been claimed by two different chemists, one of whom accused the other of borrowing it from his laboratory, we do not care to attribute it to either. With this process Dr. Taylor combines that of Marsh, which certainly, although sometimes so employed, is scarcely practicable without the previous destruction of the organic matter. To this well-known means of separating metallic arsenic, which seems to be the one most frequently employed in France, M. Blondlot recently brought forward an objection, viz., that should the sulphuric acid employed contain a trace of nitric acid, the arsenic combined with the hydrogen to form a solid rather than a gaseous hydride, so that the greater part of the metal remained in the substance examined. Dr. Arthur Gamgee has since, however, investigated the subject, and completely contradicts Blondlot. We confess that, for our part, we are inclined to side with our countryman.

These may be considered the great processes for separating arsenic, but there is another, to which, strangely enough, Dr. Taylor does not seem to refer; and considering the stress he lays upon the importance of avoiding every chance of impurity, this one should certainly have been mentioned. Professor Bloxam has pointed out the possibility of making use of the electrolysis of water for the production of gaseous hydrogen, instead of acting upon zinc by sulphuric acid, and thus producing what is essentially Marsh's process, but without the danger of contamination from zinc, which in most cases contains more or less arsenic.

Turning to the individual tests for arsenic, there is little that is new; the only thing of importance—and this is a matter to which we should have previously alluded—is the introduction of a number of plates into Dr. Taylor's work, some of which are devoted to the crystalline forms of the various poisons. Dr. Guy, in his 'Forensic Medicine,' was one of, if not the

very first, to insist on the value of crystalline shapes as a means of diagnosis, giving a variety of diagrams of these, more especially in connection with this very subject of arsenic, in an excellent but lengthy article on that poison. This scheme has been still more extensively carried out in Germany, where, as we have already noticed, a work by Helwig has recently appeared which is entirely devoted to the microscopical determination of crystalline poisons. These forms have been carefully photographed; so that, although the representations might in some instances have been clearer, we have none of that artificial sharpening so likely to be encountered when they have been figured by the pencil.

Turning next to antimony, we have to consider it chiefly now-a-days as employed in the perpetration of the so-called secret poisoning, cases of acute poisoning from its effects being comparatively rare. When we come to consider the subject more closely, we find that we have to deal with, first, its local irritant action, whether exercised on the skin or on the alimentary canal; secondly, its specific effects on the stomach, however exhibited; and, lastly, its depressing effects on the heart, &c., exercised through the nervous system. As to its local action, this has been supposed by Mialhe to depend upon the conversion of tartar emetic into chloride of antimony by the alkaline chlorides contained in the tissues; whilst others, with perhaps greater show of reason, affirm that it owes its local effects to its power of coagulating albumen. Be that as it may, there can be no doubt that, however exhibited, tartar emetic can give rise to a peculiar train of symptoms, whether absorbed through the skin or through the mucous membrane of the alimentary tract. When given by the mouth, it is not unusual to encounter aphthæ as well as salivation, the former sometimes extending into the stomach and bronchi. These Van Hasselt thinks indicative of chronic poisoning, but he is scarcely borne out by facts. Vomiting and purging are among the most common symptoms; but in one very curious case mentioned by Dr. Taylor, an emetic had actually to be administered before the poison could be got rid of. Collapse is also a very well marked feature in acute poisoning by this substance: the respiration is generally greatly oppressed, prolonged expiration and hasty inspiration being usually observed (Ackermann), the result partly of congested lungs, partly of enfeebled muscles, and, it may be, of a specific action on the larynx. The heart is also remarkably affected; its beats become slower, the arteries are badly filled, and the temperature is lowered, first externally, then internally, whilst convulsions and cramps are of no unusual occurrence. Two things have been referred to as diagnostic between arsenical

and antimonial poisoning: these are the occurrence of urinary suppression in the former, not in the latter; whilst Dr. Taylor lays considerable stress on the great tendency there is to recover from the prostration caused by antimony, provided the patient has a chance to do so. The post-mortem appearances will not detain us long. Among the most marked of these, according to most observers, is the congested condition of the lungs. Ackermann, however, maintains that they are not so, but often emphysematous, and containing small clots of blood. The bowels usually contain a number of small ulcers, and occasionally the brain is congested. When looking for the poison, as has been well shown by Dr. Richardson, it may be found in any part of the body, and in all the ejecta, even although it has been introduced through the skin or by the lungs. D'Anger and Flandin thought that it had a special affinity for the liver, as it was usually found there in greatest abundance; but we have already shown that this is nothing more than what might be expected from the anatomical characteristics of that organ.

Chronic poisoning by antimony may now unfortunately be considered as regularly established among us; and we have only too many cases, as recorded in public prints, to fall back upon with regard to the symptoms usually observed. Nevertheless, we can hardly look upon the crime as being so very common as Dr. Taylor would have us believe; and we would remind that gentleman that, when listening to the tale of suspicious circumstances that have occurred in the career of any criminal, he should remember that, were they not looked at through the lurid light of later experience, his sage informant would likely be the first to pass them by as of no importance. Such a lesson can, we think, be read from the case of Pritchard, and at least one of the medical men intimately connected with it.

On chloride of antimony as a poison we would only make one remark. Recorded cases of death from this substance are but few, and in those the post-mortem appearances resemble rather the effects of the mineral acids than of antimony. This may be accounted for by the precipitation of the oxy-chloride of antimony from the chloride, by the water in the saliva and the gastric secretion, thus setting free the hydrochloric acid which may give rise to the local mischief, the precipitate, which may also be absorbed producing the constitutional symptoms.

The antidote for antimonial poisoning is simple; any substance containing tannin having the power of throwing the metal down in an insoluble form.

As to the various processes for detecting antimony, we have but little to say: we may, however, observe, *en passant*, that Reinsch's process, which seemed all-sufficient to Dr. Taylor in

1859, has been quietly put in the background by the same gentleman in 1865. As a rule, dogmatism is inapplicable to medicine. After all, the plan now recommended, viz., by suspending a piece of platinum foil, surrounded by a coil of zinc foil in the suspected liquid, when the antimony, if there be any present, will be precipitated on the platinum, whence it may be removed and tested in the ordinary manner, is as good as any.

Turning next to the subject of mercurial poisoning, we find that by far the greater number of cases of this kind are referable to corrosive sublimate, so that practically we may limit our considerations to that substance. Falck, however, who is followed by most German writers, divides all mercurials into two groups—the *mild* and the *corrosive:* to the former belong calomel, the black oxide, white precipitate, &c.; to the latter, corrosive sublimate, the cyanide, the nitrates, and the red precipitate. The division is founded on the fact that although both groups give rise to the peculiar symptoms known as mercurialism, the former is limited to this, the latter producing also local irritant effects wherever applied. Belonging to the former group we have an immense train of disorders, of which our space will not permit us to speak; we shall therefore only enumerate them from Falck's work, as some of them are scarcely recognised as distinct disorders in this country. We have first mercurial fever; second, the skin affection sometimes known as eczema mercurialis, by Falck termed hydrargyria, and separated into three subdivisions—the mild, the febrile, and the malignant: the third group is constituted by mercurial ulcers, and the fourth by salivation. On this last subject we should like to dilate, but must hurry on, pausing only to remark that this symptom is by no means pathognomonic of mercurial absorption, but may be seen after the use of antimony, arsenic, iodide of potassium, and a great variety of other substances; in one case which we lately came across, from a dose of the ordinary tincture of the perchloride of iron. But to resume the long list of mercurial affections, we have next, diarrhœa with lesions of the *primæ viæ*, lesions of the lungs and bones, pains of the joints, affections of the eyes (chiefly amaurosis), hypochondria, idiocy, epilepsy, tremors, paralysis, aphonia, chlorosis, erethism, and general cachexia. These are most commonly observed in what might be termed chronic poisoning with mercury, but may also occur in connection with the more acute forms. As to the ordinary symptoms of poisoning by corrosive sublimate, we need say but little: the powerfully styptic metallic taste, followed by speedy vomiting often of bloody matters, the purging rarely long delayed, the frequent suppression of urine, and the subsequent salivation, are all sufficiently well known.

But when we turn to inquire into the mode in which corrosive sublimate acts, we find rather more dubiety. Most authors are, however, agreed as to one thing; and that is, that its affinity for albumen furnishes the clue to this action, but there would appear to be certain changes which take place within the body as to which we are not as yet very well informed. Voit's researches would make it appear that the salts of the oxide of mercury were changed into the chloride by the alkaline chlorides in the body; whilst Von Baerensprung shows that all sub-salts are converted into proto-salts by the precipitation of a portion of their metallic mercury—an idea borne out by the composition of the so-called spinach stools, so often produced by calomel, which owe their colour to a mixture of the colouring matter of the bile with sulphide of mercury and the pure metal. Many are of opinion that all are converted into corrosive sublimate in the blood. Voit has shown that calomel with albumen, in the absence of air, is converted into metallic mercury and corrosive sublimate, but when blood or air is present none of the mercury appears in the metallic form. There still appears to be the same doubt, however, whether or not metallic mercury is absorbed as such when rubbed in as ointment. Polotebnow, one of the most recent investigators of this subject, has ascertained that corrosive sublimate, when added to defibrinated blood, prolongs the sinking of the corpuscles and separates their colouring matter. The corpuscles themselves are speedily destroyed by its action, especially with an elevated temperature, and the colour goes quicker in proportion to the strength of the solution. The blood also seems to lose the power of absorbing oxygen. These facts give some clue to the nature of its poisonous action.

Within a comparatively recent date, certain of the compounds of cyanogen and mercury have attracted notice from their toxicological properties: thus the symptoms of acute *intoxication* from cyanide of mercury greatly resemble those from corrosive sublimate, only the pupils appear to be greatly dilated in the case of the former, and not in the latter (Moos). We have also had recently introduced among us as playthings a highly deleterious compound—the sulpho-cyanide of mercury—in the shape of what are called Pharaoh's serpents. One case is recorded of a young man suffering slightly from the residue of this, which he mistook for a *bonbon;* but certain investigations would seem to show that when burnt, only their vapour, consisting of metallic mercury and carbonic acid, is really injurious. We must also mention the fact that corrosive sublimate, when applied locally, may give rise to all the symptoms of mercurial poisoning superadded to those of local disturbance. One very

notable case is recorded by Dr. Taylor where symptoms of poisoning appeared in from thirty to forty minutes after the application of an ointment containing corrosive sublimate for scald head. Death followed in the two children so affected. A similar case, not mentioned by the above authority, occurred in America, in which the œsophagus was lined with false membrane, whilst the walls of the stomach were swollen and covered with ecchymoses. But a third case, of still greater interest, is on record: in this instance an injection containing corrosive sublimate was used as a remedy for ulceration of the os uteri, but with the almost immediate effect of superinducing violent symptoms of mercurial poisoning, including tenesmus and bloody stools, salivation, fœtor of the breath, &c., coupled of course with the indications of violent local inflammation. The patient ultimately recovered. Salivation from the application of the acid nitrate of mercury as an escharotic to the same situation had previously been observed by Breschet. We need scarcely allude to the poisonous properties of the other compounds of mercury.

Speaking of the chemical detection of corrosive sublimate, it should be remembered that in most cases it is sufficient to show the existence of the noxious base; here such is not the case, as, for instance, the ordinary medicinal dose of calomel contains far more mercury than a quantity of corrosive sublimate sufficient to cause very severe symptoms, or even death itself. The plan recommended by Dr. Taylor for showing its presence is to dissolve up the mercury when in that shape (corrosive sublimate) by means of ether, and then to test for both chlorine and mercury; but it is evident that this can only be done when the poison is in solution, which, from what has been said of its mode of action, can but rarely be the case. Most frequently, however, if carefully performed, this process will furnish sufficient material for demonstrating the existence of the poison. Should it not, however, we have something else to fall back upon, viz., the symptoms and the post-mortem appearances, which even by themselves are sometimes sufficiently diagnostic. In separating mercury from organic solids, Dr. Taylor still seems to shrink from the employment of chlorate of potash and hydrochloric acid, apparently preferring nitro-hydrochloric acid alone, in which, however, there are few that follow him. Reinsch's and the galvanic tests are also and more deservedly favorites with him. There is one little point in which Dr. Taylor would have done well to have been clearer—a complaint which we have not to lay on the shoulders of Dr. Odling—that is, in describing the effects of sulphuretted hydrogen on a solution of a salt of mercury, he neglects to state the various stages

of colour observed during the process of reaction. These are first white—then brown, dark brown, and, finally, deep black; but the first appearance might deceive an unwary and inexperienced experimenter.

The consideration of lead-poisoning leads us into the regions of accidental or economical toxicology rather than into those of crime. No doubt, it is highly criminal on the part of a dishonest tradesman to sweeten his sour wine with litharge, or to add chromate of lead to confectionery; but in these cases, although the ultimate result may be serious, except a very large quantity be exhibited at one time, the immediate consequences are not very noticeable. Thus, and for the reason hinted at, viz., the slight effect produced by most salts of lead except in large doses, cases of criminal or even of acute poisoning are of rare occurrence. One, however, of murder by acetate of lead is recorded by Schniewind in Casper's 'Journal' for 1862. Some have even doubted the possibility of acute symptoms of a dangerous character arising from the use of carbonate of lead, but there is one related by Snow where death was the result of a single dose of this substance. There can be no doubt that the continued intussusception of this preparation or of any preparation of lead will give rise to very distressing results. Even the metal itself may produce these, as is seen in the case of plumbers, compositors, certain classes of weavers, miners, &c. A case is also mentioned by Percy where severe symptoms followed the use of a quantity of shot as a remedy for obstruction of the bowels; but in this instance, the arsenic combined with the lead had probably quite as much, if not more, to do with these than the latter metal. Certain very anomalous cases of this kind had occurred in Germany without any known cause being ascertained. Ultimately it turned out that the poisonous substance had been taken into the system along with snuff which had been packed in thin sheet-lead instead of tinfoil, or been kept in leaden canisters. In America, not long ago, a number of individuals were affected with chronic lead poisoning, by using meal manufactured at a mill where the mill-stones, being old, were in some places filled up with lead. In France also, symptoms have been observed to follow the use of silk-thread adulterated in a new fashion. As it is sold by weight, a plan was adopted for increasing this, viz., soaking in a solution of acetate of lead, and then passing sulphuretted hydrogen over it, when a considerable quantity of sulphide of lead was left between its fibres. But, of all classes of workmen, painters are most subject to the evil influence of lead, using so much of it in the form of carbonate or oxide, and frequently working in rooms kept at a high temperature. Under such

circumstances, it seems to be absorbed both by the skin and the lungs, as well as along with the food if cleanliness be not attended to. Even sleeping in a newly painted room has been followed by illness, whilst the bad effects of water long kept in leaden pipes and cisterns is sufficiently well known to all. It should be remembered, however, that the purer waters, or such as contain only carbonic acid or chlorides, are those most likely to produce partially soluble salts, whilst sulphates form compounds almost if not entirely insoluble.

As to the mode in which the poison acts, as usual, opinions vary. Some say it combines with and precipitates albumen; others (Falck), that it precipitates pepsine, and so induces disease from malnutrition. Mialhe, again, will have it that all lead compounds are changed into chlorides, or compounds of this with common salt, in the stomach; no one now holds with Thomson that it always exists in the blood as carbonate. According to the most popular opinion, viz., that it acts by precipitating albumen, the two combined are thrown down in a greyish mass on the surface of the alimentary canal, preventing absorption, and consequently producing malnutrition, anæmia, &c. Some are inclined to think that all the disastrous effects of lead-poisoning may be accounted for in this way, but others seem to believe that when fairly within the system it attacks special organs. Thus, Gusserow found it invariably present in the muscles and joints; sometimes in the urine and brain, oftener in the liver, and rarely elsewhere. This peculiar affection of lead for the joints seems, according to Schäffer, to be shared with arsenic, both being found in them after they have disappeared from other parts of the body. The state of combination, however, differs. Arsenic appears to exist as arseniate of lime, whilst lead, being isomorphous with lime, takes the place of the latter metal. Lead would seem to act upon the muscles directly, and not alone through the medium of the nerves. Its peculiar partiality for the extensors of the forearm is well known; but we sometimes find the whole arm, including the muscles attached to the scapula, completely wasted. Such a case has been recorded by Dr. Moore of Dublin. The paralysis which thus results is commonly partial—not, like that which sometimes follows arsenical poisoning, affecting all the extremities. This last is in reality a worse complaint, it usually lasts from four to ten months, and sensation goes as well as the power of motion, temperature is lowered, cramps are common, and the excitability of the muscles by electricity is lessened: at the same time, the organs suffer from general atrophy (Leroy). Since the days of Tanquerel des Planches, the ordinary symptoms of chronic lead poison have been well known, and we do not here

propose to discuss them. There is one peculiar symptom, however, to which we would allude: this occurred in a case related by Santlus, where the patient suffered severely from asthma consequent on the dressing of a wound with litharge. Three cases are recorded by the same author where similar effects followed the internal use of iodine.

We should have liked to follow Dr. Taylor in his account of some other poisons, as copper, zinc, &c.; and to say something of Moreau's researches as to the former, and of Falck's as to the latter. Bichromate of potash as investigated by Chevallier should also have come in for some notice; but in this connection we must content ourselves by directing the attention of our readers, as well as of Dr. Taylor, to a case of poisoning by this substance excellently reported in a former number of this Review by Dr. Ogston.

We shall not delay with vegetable irritants, except to add to Dr. Taylor's list a case of poisoning by savin, where the stomach in its great cul-de-sac was perforated in two places (May). In connection with this substance, we notice that Dr. Taylor does not mention the discovery of the so-called glandular woody tissue as a diagnostic of savin, in which we most certainly concur with him, as the powder is usually prepared from the leaves, in which we suspect there will be some difficulty in finding the tissue referred to.

Various cases are recorded of death from croton oil, but we think there are not many recorded where recovery has taken place after a stronger dose than in one which came under the notice of Dr. Smoler, where half an ounce was taken, yet the patient escaped. Under the heading of vegetable irritants, Dr. Taylor discusses colchicum and the hellebores; but we think we are amply justified in relegating them to the narcotico-acrids, where we shall hereafter speak of them.

Turning to the animal irritants, we find in Dr. Taylor's recent work two very good representations of cantharidin, which will be useful to those engaged in searching for a substance not easily, if at all, recognisable by chemical means.

By far the most important of the animal poisons, looking at them from an economical point of view, are those articles of food which, under certain circumstances, give rise to dangerous symptoms, and often to fatal consequences. As to the conditions which produce the changes referred to, nothing very absolute is known. Shell-fish will sometimes give rise to very troublesome effects; a number of fishes living in tropical seas are also said to be dangerous, and almost any of them may when imperfectly cured, and consequently partially decomposed, produce vomiting and diarrhœa. Probably, however, the most

virulent symptoms we know are consequent on the ingestion of the so-called "sausage-poison." Of this kind of poisoning, up to 1853, according to Schlossberger, 400 cases had been recorded, and of these 150 had ended fatally. In a former number of this Review (Jan., 1860, page 197), a very good account of the symptoms thus produced is given by Dr. Tripe of Kingsland, who shortly before had a number of cases under his care.

The exact nature of the change which takes place, or the active principle in the poisonous sausage, has never been very clearly made out. Some authors have attributed its effects to irritants accidentally added; others, to the production of some substance analogous to creasote; others still, to the formation of something of the nature of prussic acid, picric acid, some fatty acid, or a poisonous organic base. Many men of eminence, again, hold that it is a fermentation poison which cannot be isolated; whilst a few attribute everything to the formation of fungi. Recent discoveries have, however, thrown a flood of light upon at least one variety of poisoning by sausages; and to this source Dr. Taylor refers, but not very distinctly. If we examine reported cases, we shall find that there are two tolerably well-marked varieties of sausage-poisoning: in the one, the symptoms come on speedily,—within twelve or twenty-four hours after the ingestion of the poison; but in the other nothing unusual may appear for five, six, twelve, or even thirty days. The former group we would still attribute to a poisonous substance formed in the sausage, but we do not see how the latter can be attributed to anything else than to that now well-known parasite the trichina. We do not mean to say that the former group may not result from the same cause as the latter, but observations are still wanting to establish this: certainly, however, the latter group corresponds in almost every particular with the effects of the ravages produced by the trichina.

Dr. Taylor next discusses, as the last of the irritant poisons, putrescent food, in connection with which is quoted the same author's work 'On Poisons,' but without any page being cited: most readers will, therefore, be rather surprised, on referring to the book in question, to find that no animal irritant is discussed except cantharides, and that the reference above alluded to must be looked for in the Introduction. In this last connection occurs the following passage:—"In the chapter on ANIMAL IRRITANTS some other facts will be mentioned, from which it would appear that the milk of cows fed in certain districts of America is poisonous;" but, on turning to the chapter in question, everything except cantharides is omitted; so that, as of the famed Decoctum Ulmi B. P., we must say *non est*. Our author will, however, find in the 'Edinburgh Medical Journal' for 1861-62, some cases which occurred in Malta, where goats' milk became

poisonous from the animals having eaten *Euphorbia paralias* and *E. helioscopia.*

All mention of the poison of serpents is also wanting: but as this has scarcely any bearing on medical jurisprudence, we could scarcely look for it in the more recent work; although in the former we think it should have been included. We do not now propose going into this subject further than to refer to the so-called antidote of Bibron, which consists of a combination of bromine, iodide of potassium, and corrosive sublimate. This was at one time held up as a specific, and was recommended in the case of rattlesnake bites by Dr. Hammond, U.S.: but subsequent investigation has shown that results scarcely justified such high encomiums.

The latest addition we have to our knowledge of animal irritants, or at least of animal poisons, is by Dr. Zalesky, who has been investigating the poisonous substances secreted by such animals as the toad and the salamander. It has long been a vexed question whether the milky substance exuded from these animals justified the popular opinion as to its poisonous nature; and when we mention such high authorities as Dr. John Davy and Mr. Rainey, to say nothing of Continental observers, as having taken part in the investigation, we may consider that an exact series of inquiries leading to a definite result must be of some considerable value. The author of whom we have just spoken has limited his experiments to the substance, or rather the active principle of the substance, produced by the land salamander, and which he terms *samandarin.* We cannot here enter into full details: suffice it to say that this samandarin is obtained by Sonnenschein's method (precipitating the alkaloid (?), by phospho-molybdate of ammonia, and again separating it from this by means of caustic baryta); and that, when exhibited internally or injected subcutaneously, it exercises a well-marked toxical action—something similar to that produced by strychnine, but differing in some points. Convulsions of an epileptic nature are speedily produced, gradually becoming more and more violent, opisthotonos follows, with great dilatation of the pupils of the eyes, and death supervenes, apparently from paralysis. It appears to act specially upon the nervous centres, the muscular structure of the heart being but slightly affected, and the other muscles not more so than can be accounted for by the excessive exertion they have undergone.

We had intended to discuss the recent additions to our knowledge of vegetable poisons; but we have been compelled to say so much of mineral substances, that space fails us, and we are unwillingly obliged to postpone our view of the other groups until a more convenient period.

PART SECOND.

Bibliographical Record.

ART. I.—*The Danger of Deterioration of Race from the too rapid Increase of Great Cities.* By JOHN EDWARD MORGAN, M.A. Oxon., Physician to the Salford Hospital, &c. *London,* 1866, pp. 65.

THIS paper, which was read at the Social Science Congress held at Sheffield in 1865, will well repay perusal, as showing, what, indeed, is generally admitted, that city life and in-door employment and city habits are less conducive to health, strength, and the production of a vigorous offspring, than a country life and its adjuncts under the most favorable circumstances when connected with agricultural occupations; and therefore that a deterioration of race must be endangered from the too rapid increase of great cities, and that a nation's greatness, mainly depending on the mental and bodily energies of its people, must thereby be proportionally hazarded. In our own country, to which our author's remarks are limited, the increase in question is remarkable. At the end of the seventeenth century the dwellers in towns were estimated at 1,000,000, the dwellers in the country at 4,100,000; whilst in 1861, according to the Registrar-General, the dwellers in populous towns amounted to 10,930,841, and those of small towns and country districts to 9,134,386. Of the causes to which our author attributes most injurious influence in connection with town life, he selects three especially, and, as we think, with good reason, viz., vitiated air, constitutional syphilis, and the abuse of alcohol. Had we any doubts as to the justness of his views on this matter, the facts he adduces would entirely remove them.

The presence of ozone is a tolerable criterion of the purity of the atmosphere, and its absence is pretty well ascertained in that of great towns; this is only one of the many tests of the difference between the air of such towns and that of the country. As to syphilitic influence, some idea of its amount may be

formed from the number of inhabitants so tainted; from returns which he names, he thinks he is justified in stating the number for Manchester during two years at about 6000, exclusive of others not admitted in the documents referred to. Of the third cause, alcoholic excesses, the evil is notorious in towns, and especially in the female sex in the great manufacturing towns, owing to the wages they receive and their early employments. Besides these causes, there are others of a minor kind to which he adverts; the one he chiefly dwells on is the condition of mothers in the manufacturing districts, and especially their continuing at full work till immediately before the birth of their children, and a return to their employments as soon as convalescence is established, with the addition of bad nursing and the other neglects to which their offspring are exposed. He is of opinion that confirmation is afforded of the deteriorating influence of city life in "the altered type of prevailing disease." We agree with him that this may be true partially, that is, restricted to the diseases to which the labouring class are subject in cities under the deleterious influences there constantly acting, though, as he well points out, not in the diseases of the inhabitants of country districts exempt from such influences. As an example of the latter, writing from his own knowledge, he mentions the western islands of Scotland, where he found the prevailing diseases of the sthenic character of the inflammatory class, and needing vigorous treatment by "depletion, antimony, and mercury."

The practical question, "What steps can be taken to prolong the duration and brighten the prospects of city life?" next has his consideration. All he can suggest are palliatives, being persuaded—and who can resist the persuasion?—that a populous town can never be as wholesome as country villages. As palliatives, he lays most stress on giving more breathing space to towns by the widening of streets, &c., and the prevention of house-crowding, by emigration, the deportation, if possible, of the sickly and worn from over-work to our colonies or other lands where there is a demand for field labour, with the prospect of benefit from a sea voyage and renewal of vigour from healthy work in a healthy climate. To this purpose he asks, might not the funds of a union be better applied than in making the same class of persons "the slavish recipients of pauper charity"? Another palliative which he points out is the curtailing city diseases by measures effectually taken to prevent the spread of those which are infectious. Scarlet fever, for instance, alone, in Manchester, in the course of the last two years, carried off about 2500 persons out of about 20,000 attacked.

On account of the vast importance of the subject in a national

point of view, and our persuasion that it is men of the medical profession who are most likely, from their calling and humanity, to aid in carrying the suggestions into effect, we are induced to copy *in extenso* the following concluding remarks of the author:

"To sum up the foregoing suggestions, we may say that on some subjects accurate and general observations are more urgently called for, such as those relating to the composition and temperature of the atmosphere, not taken in the airy suburbs, but in centres of industry, where men and women are congregated together. So, likewise, in addition to what the registers tell us respecting the result of every man's last illness, we require to know the nature and extent of the invaliding which precedes it; and such information we can only obtain from disease returns. So, too, if we would set a check on those maladies which are truly preventible, we should organize special wards for the reception of the sufferers from contagious diseases, and retain our patients until convalesence is established. We must likewise supply more accommodation at our Lock hospitals, and adopt stringent measures for preventing the class of patients who attend from becoming a general source of infection. The employment of married women, also, more particularly when they are the mothers of young children, should in every way be discouraged. Schemes of emigration specially calculated for the relief of the mother country should be brought into operation. Settlements along the railways should be established for the working classes, with special cheap trains at early hours in the morning and at breaking-up time in the evening. Cellar-dwellings should be closed up, courts and alleys cleaned out, and the sites they occupied left open to serve the double purpose of air-shafts and playgrounds; streets also should be widened; factories and workshops, instead of being piled up in the centre of our towns, should be scattered in the country in healthy and airy localities."

With much propriety he adds—

"If these suggestions are in any degree practicable, and if the importance I have attached to them is not overdrawn, it will be admitted that they can neither be too often nor too urgently pressed on those who have the power of enforcing them."

Appended are some useful statistical tables in support of the author's conclusions.

ART. II.—*The Use of the Laryngoscope in Diseases of the Throat, with an Appendix on Rhinoscopy.* By MORELL MACKENZIE, M.D. 1866, pp. 153. Second Edition.

THE exhaustion of the first edition in eighteen months is a

pretty conclusive proof that this work has been appreciated by the profession. In a former number of this Review, attention was directed to Dr. Mackenzie's book, amongst others on the same subject; and a very favorable opinion of its comparative merits was then expressed. We have only now to add that the new edition, whilst possessing the advantages of its predecessor —completeness, conciseness, and perspicuity—has been further improved by careful revision and amplification of the text, and by the addition of several new woodcuts.

The following outline of the work will give the reader a fair idea of its scope and general character. The first chapter is devoted to a history of the invention; and, as remarked in the article referred to above, it is the most complete history yet given. To Dr. Babington is assigned the credit of inventing a laryngoscope; to Czermak that of rendering the instrument more generally applicable, and of impressing upon the profession the advantages of its use. In Chapter II, the instruments required for laryngoscopy are described, and particular attention is given to the subject of "illuminating," for which purpose Dr. Mackenzie prefers his own "light concentrator," applied to a gas-burner, a paraffin, moderator, or reading-lamp. The principles and practice of the art form the subject of the third chapter, and these are detailed with great clearness and precision. In reference to one of the most common of the difficulties in laryngoscopy, that arising from an unusually irritable condition of the fauces, Dr. Mackenzie remarks—

"The internal administration of the bromides of potassium and ammonium has been recommended; but my experience has proved the total inutility of their employment. Some advise that the patient should be directed to inhale a few whiffs of chloroform; but in those rare cases which present much difficulty I have found the best effects result from sucking ice for about ten minutes before the mirror is to be introduced. The most irritable fauces cannot resist this plan." (P. 58.)

Chapter IV describes minutely the appearance of the healthy larynx, seen by the laryngeal mirror, and two excellent woodcuts illustrate the anatomical details. Chapter V is headed "Accessories of Laryngoscopy," and comprehends auto-, recipro-, and infra-glottic laryngoscopy. The application of remedies to the larynx is next considered. Of the various "spray-producers" now in use, Dr. Mackenzie prefers that of Mayer, with which solutions of carbolic acid, tannin, and perchloride of iron may be rendered serviceable. Similar solutions may be applied by means of the laryngeal brush; the one for which the author has a preference is the perchloride of iron.

Dr. Mackenzie has the merit of inventing a ready means of applying galvanism to the vocal cords; and in illustration of the occasional advantage of this agent, cases are given. The remaining chapters have reference to operative proceedings, many of which have been simplified by instruments of the author's own invention. These subjects are also adequately illustrated by cases. In the Appendix the history, theory, practice, difficulties, and use of rhinoscopy are clearly described, and illustrated by cases and woodcuts.

As the title of the work indicates, its object is to discuss the means of investigating throat diseases, and of mechanically applying the remedies which their nature requires. In as far as it goes, it is undoubtedly the best work of the kind hitherto published.

ART. III.—*Clinical Notes on Uterine Surgery, with special reference to the Management of the Sterile Condition.* By J. MARION SIMS, A.B., M.D., late Surgeonto the Women's Hospital, New York. *London*, 1866.

THE perusal of the well-printed and illustrated volume whose title we have given has impressed us with a strange mixture of respect for its cleverness on some points and profound disgust for what may be termed its filthiness on others. Dr. Sims is undoubtedly a clever man and a skilful operator, and withal possessed of a good deal of originality of resource in providing surgical means for the cure of those diseases of which he treats; he also possesses the greatest regard for delicacy in the management of female complaints, but at the same time enters into some of the most extraordinary details we ever saw published in a respectable work. His chief object appears to be the prevention and cure of sterility, which he divides into "natural" and "acquired;" and however much of truth there may be in many of his observations, the author has overstepped the bounds of prudence and risked the favorable reception of his book amongst those who deem it necessary to refrain from obscene and disgusting revelations. Besides a few introductory observations on uterine examination, the work comprises eight sections, under the following heads:

1. Conception occurs only during menstrual life.
2. Menstruation should be such as to show a healthy condition of the uterine cavity.
3. The os and cervix uteri should be sufficiently open, not only to permit the free exit of the menstrual flow, but also to admit the ingress of the spermatozoa.

4. The cervix uteri should be of proper size, form, and density.

5. The uterus should be in a normal position, *i. e.* neither anteverted nor retroverted to any great degree.

6. The vagina must be capable of receiving and of retaining the spermatic fluid.

7. For conception, semen with living spermatozoa should be deposited in the vagina at the proper time.

8. The secretions of the cervix and vagina should not poison or kill the spermatozoa.

In the early part of the work the author makes some very sensible remarks on the form and uses of the sponge-tent, which he says is to us "a sort of necessary evil, and that he who gives us an efficient, pleasant, and cheap substitute for it will confer a great boon on surgery." He remarks that those found in the shops are large clumsy things, thickly coated with wax, tallow, or suet, are difficult to introduce, and often slip half out of the cervix into the vagina, there exciting an unnecessary amount of irritation. The author recommends them to be made in the following manner:

"The sponge should be of good quality, but not too soft or yielding; it should be thoroughly cleaned, but not bleached, for the bleaching process deprives it of all elasticity. It should be cut into slightly tapering, conical pieces, from one to two inches long, some smaller and others much larger than the thumb. A pointed wire or a slender awl should be passed through the centre of the long axis of the sponge, which should then be thoroughly saturated with a thick mucilage of gum arabic. A small twine or cord is then to be closely wrapped around the sponge as it is held stiff by the wire, beginning at the smaller extremity, and gradually winding on to the larger; then the wire may be withdrawn, and the new-made tent laid aside to dry. If we are in a hurry it may be dried in the sun or by the fire, taking care not to injure the texture of the sponge by too great a heat. When it is thoroughly dry the twine is to be unwound, and the little circular elevations made by it on the surface are to be rubbed down by fine sand-paper. Without further preparation it is then ready for use."

His directions for the application and removal of these tents are practical and useful, and he moreover asserts that the power of the sponge-tent to modify the uterine surfaces with which it lies in contact is truly wonderful.

"It dilates the neck of the womb, it softens it by pressure and by a sort of serous depletion; it reduces the size, not only of the neck, but of the body of a moderately hypertrophied uterus; it destroys not only fungoid granulations, but even large mucous

polypi; and in one instance I saw a sponge tent destroy wholly a fibrous polypus as large as a pigeon's egg."

One of the greatest objections to the use of sponge for vaginal purposes is the disgusting smell of the discharge it produces, and this, he says, may be prevented by the use of Price's glycerine:—"I have now often used this as a disinfectant of the sponge, and find it infallible in its results."

The work we are considering contains a great number of illustrations, mostly drawn by the author himself, which greatly facilitate the explanations of the form and uses of the various instruments described, as well as the diseases and displacements for the cure of which they are required; and here we must observe that Dr. Sims has exhibited a degree of aptitude of contrivance and facility of improving instruments already in use such as we have seldom met with; and the different ways in which he has been gradually led to these inventions and modifications are throughout described in the plainest and most intelligible language. Indeed, this part of the work cannot be considered otherwise than extremely interesting. His observations, for instance, concerning the speculum, sponge tents, the écraseur, pessaries, and his modification of the usual operations for incising the os uteri, for amputating the cervix, narrowing the vagina, for the cure of prolapsus, &c., all exemplify the greatest mechanical tact and practical acumen.

On the subject of uterine displacements a great variety of opinions has existed as to whether many of them are of importance or not; but the author insists upon their exercising a very marked influence in producing sterility, and has devoted a considerable portion of his work to their consideration. He says—

"When we remember that about every eighth marriage is sterile, we see the necessity of investigating all particulars that can by any possibility bear upon the elucidation of this important subject. When we call to mind the fact that of 255 cases of acquired sterility, 111 had retroversion and 61 anteversion; and of 250 cases of natural sterility, 68 had retroversion and 103 anteversion, we may have a right to suspect that the position of the uterus, is a matter of some importance in the treatment of the sterile condition. Of course, many of these cases of malposition were complicated with fibroïds, or flexures, or engorgements, or hypertrophies, or a conical cervix, in those who have never borne children. But even if all these be rectified, we may still have sterility as a conseqnence of malposition alone. At all events, the frequency of malposition renders it an important element in the treatment of the sterile condition." (P. 273.)

The experience of the writer of this review fully corroborates

the opinions above expressed, and the following case which recently came under his observation may fairly be considered a proof amongst others that acquired sterility may depend upon even a moderate degree of uterine displacement. The patient was a clergyman's wife, about forty years of age, in good general health. She had one child soon after marriage, a son, who was then seventeen years old, and she had never been pregnant again. She complained of pelvic uneasiness, sometimes amounting to pain, in the groins and lowest part of the back, with occasional difficult defecation and inability to walk with comfort; the sympathetic symptoms consisting of periodical attacks of facial neuralgia, uncertain appetite, occasional nausea; she had also a certain amount of dysmenorrhœa and leucorrheal discharge. On examining the uterus it was found to be partially retroverted, the fundus pressing upon the rectum below the promontory of the sacrum, but the cervix flexed so as not to irritate or incommode the bladder by pressure under the pubes. It was not a very difficult matter to raise the fundus into its natural position, and this was followed by immediate relief; she was advised to rest in the recumbent posture always immediately after exercise, and to pay great attention to her bowels. In about a month she applied again for advice, owing to a slight return of her symptoms; the displacement had slightly returned, but she said she had been very much better ever since the former operation. The fundus was again replaced, and cold-water injections, or rather vaginal washings, were recommended to be used daily; the engorgement of the cervix which had previously existed was gone, and she said she had locally a much more healthy feeling than for years past. After another interval of a month or two she made her appearance again, not for advice as to her former complaint, which had entirely disappeared, but on account of a new train of symptoms which she could not fully understand, but which her friends suspected to be pregnancy. On strict investigation these suspicions were confirmed; and although astonished at the fact that she was about to have a second child after an interval of seventeen years, she received the somewhat unwelcome intelligence with becoming resignation and a degree of thankfulness that she was not the subject of incurable disease.

With regard to the management of these various displacements, nothing except actual experience can be more profitable than a careful perusal of the descriptions given by Dr. Marion Sims; his modification of Simpson's uterine sound, by which he makes a dangerous instrument as a *redresser* for the cure of retroversion a perfectly safe and effective one; and the operation for narrowing the vagina above for the relief of procidentia

uteri, which he advises in lieu of the perineal operation of Mr. Baker Brown, are especially worthy of attention. We will venture once more to quote the author's words when reflecting upon the gradual improvements that have taken place in some of these operations, and his remarks may be said to apply to almost all the operations of surgery.

"It is always interesting to watch the slow degrees by which the true principles of treatment are established. The idea of narrowing the vagina for the cure of procidentia was first suggested by Marshall Hall. Then I carried out the principle by cutting away the whole of the redundant portion of the anterior wall of the vagina, This I afterwards modified by simply denuding a large oval surface on the anterior wall, and uniting its lateral edges by silver sutures. This was further modified by making a V-shaped scarification, and producing a veritable fold in the wall of the vagina. Then I made the V trowel-shaped, by turning its upper ends inwards across the axis of the vagina. Then Dr. Emmet made this a complete triangle, and eventually an accident showed him that merely a narrowing of the vagina just at the anterior *cul-de-sac*, at least in one case, answers every purpose of holding the uterus in its place." (P. 313.)

We have thus far expressed ourselves much in favour of the author as a thorough mechanician and good operator, possessed of an unusual degree of sagacity, with a mind fertile in useful expedients and resources. But we have now arrived at the latter sections of his work, and these we would willinging treat with silence. We cannot think him authorised in publishing the minute details which they contain. There may be some truth in his delineations, but they are not of a nature to meet the eye of the public, neither does the necessity of the case, we think, warrant their being put in practice. He has stretched his point too far, and in his eagerness to cure sterility he has related experiments and modes of practice which are not only abhorrent to the imagination of the pure-minded, but scarcely justifiable in their performance where decency, modesty, and virtue, ought to prevail.

ART. IV.—*Clinical Histories, with Comments.* By HENRY DAY, M.D., Stafford. Churchill and Sons. Pp. 254.

THE title of this work pretty accurately designates its character and contents, for it consists of a series of cases illustrative of thirteen different topics in practical medicine, upon which the author gives his opinion and reflections. In a very modest preface Dr. Day lays claim to no credit higher than that of

industry; but after a careful perusal of his work we have found that besides being a record of industry, it bears high testimony to the thoughtful and observative habits of the author.

The subjects treated are the following:—"Cerebro-spinal Meningitis;" "a rare case of Ovarian Disease," in which the cyst appeared to discharge its contents by the stomach, the patient recovering; "on Secondary Cancer of the Lung," the record of two cases, and an account of some experiments of Dr. Richardson, in which cancer-juice was injected into the cellular tissue of rabbits, without, however, producing a cancerous sore; "Rheumatism treated by Blisters," a treatment which the writer, in corroboration of the experience of Dr. Davies of the London Hospital, holds to be useful by relieving pain and preventing peri- or endo-cardial inflammation. Dr. Day seems also of opinion that the blistering treatment in rheumatism speedily renders the urine alkaline or neutral, but he does not seem to have had more success than others in ascertaining the nature of the "materies morbi;" "Rheumatic Fever without Pain;" Chorea from Spinal Irritation," a case in which the symptoms of chorea were associated with rheumatism; "Pelvic Hæmatocele;" "Epilepsy from Peripheral Irritation," a case in which the epilepsy was clearly traceable to masturbation; "Epilepsy from Hepatic Congestion," a very interesting case, on which Dr. Day makes the following remarks:

"But still further—there is yet another form of 'mal-nutrition' which may give origin to precisely similar symptoms: for instance, 'blood disease.'

"Blood may be diseased from containing matter foreign to its normal constitution; or some of its ordinary elements may be present in excess; or, on the contrary, they may be deficient. The latter condition is well seen in the epileptic attacks which arise from 'chlorosis,' in which the iron or hæmatosin seems wanting. The former state may be typified by the presence of bile in the circulating fluid.

"I consider that it may fairly be inferred that this was the case with J. E—, the history of whose illness I have just given; the bile being present in his blood as a consequeuce of considerable and serious hepatic obstruction, this again being produced by an error of nutrition, such error having been inaugurated by habits of intemperance."

The remaining topics are, "Alternating Leucocythemia;" "Leucocythemia;" "Hysterical Facial Paralysis," a case in which, although there was an admixture of hysterical symptoms, there was also present one of the most frequent exciting causes of this paralysis, viz., exposure to extreme cold, the

patient being an engine-driver; "Cardiac Apnœa," an account of two interesting cases of this awful form of disease, followed by some judicious and instructive remarks upon its treatment.

We have pleasure in commending Dr. Day's work. It is pleasantly written, and is instructive and suggestive on the various subjects of which it treats.

ART. V.—*On the Nature, Cause, and Treatment of Tuberculosis.* By HORACE DOBELL, M.D. 1866. Churchill and Sons. Pp. 84.

THIS little work is a reprint of several papers published in the 'Lancet' and 'British Medical Journal.' Its author aspires to indicate a new theory of the nature, cause, and treatment of pulmonary consumption. He has sought to connect one special disorder of the primary digestion with the causation of tuberculosis, and has been led to fix upon the pancreas as the organ whose function is at fault. All recent thinkers upon pulmonary consumption regard it as a derangement of nutrition (in the wide sense of the word), in some way dependent upon imperfect digestion and assimilation. But it would certainly be a step in advance if we could ascertain what was the special link in the chain which was at fault. This discovery Dr. Dobell supposes he has made. In his own words, his theory is as follows:

"Tuberculosis is due to defects in the action of the pancreas on the fat taken as food (especially the solid fat). The supply of properly prepared fat is cut off from the blood—I. By the fats not beeing brought into a proper condition by the pancreas. II. By loss of absorbing power in the small intestine, due to the contact of unhealthy pancreatic juice and of defectively prepared food with its mucous membrane. Thus, the blood becomes deficiently and defectively suppled with fat-elements from the food; is unable to afford those required for direct combustion; does not replace those taken up during interstitial nutrition; but, on the contrary, takes up more to compensate the deficient supply from the food. This having gone on up to a certain point, the fat-elements of the albuminoïd tissues are seized upon, and these tissues are minutely disintegrated in the process. This disintegrated albuminoïd tissue is nascent tubercle, and this process of disintegration is tuberculisation.

"Tuberculisation will take place first wherever the following combination of conditions is most marked:

"I. Greatest activity of interstitial nutrition.

"II. Smallest amount of fat-elements able to be spared by the tissues.

"III. A double process going on, consisting of—(*a*) ordinary

interstitial nutrition in albuminoïd tissue, (*b*) interchange of oxygen and carbonic acid or carbonaceous matters through this tissue.

"The tubercle thus formed may be allowed to remain on the spot where it is formed, constituting a *primary deposit at the point of origin*, and this will especially occur when formed under the three conditions specified; or it may not be allowed to remain on the spot where formed, but be at once carried away by the lymphatics in a minutely divided condition, and either arrested in the lymphatic glands or carried on into the blood to be deposited from it, constituting a *primary deposit distant from the point of origin;* or having been primarily deposited in either of these ways, it may be taken up by the lymphatics and deposited in the lymphatic glands, or carried into the blood and deposited from it, constituting a *secondary deposit.* In the *advance* of these processes any part of the body in which nutrition is going on in albuminoïd tissue may become both the source and seat of tubercle; and any part, whether albuminoïd or not, if supplied with lymphatics or blood-vessels, may become the seat of tubercle.

"Pure tuberculosis commences when fats, properly acted upon by the pancreas, cease to pass in normal proportions into the blood.

"Tuberculisation commences when albuminoïd tissue is abnormally seized upon for its fat-elements.

"A secondary state is superadded when tubercle has been carried into the circulation, which constitutes tuberculæmia or tuberculous blood-poisoning." (P. 4.)

Having been able definitely to fix upon faulty pancreatic action as the cause of the phenomena in question, we naturally turn first to the part of the work which discusses the relations of the pancreas to tuberculosis, expecting there to find some clinical or post-mortem observations bearing on the question. It might be supposed that there existed some chemical or microscopic indications of disease occurring in phthisis which had hitherto been unrecognised. But no such evidence is furnished. The experiments of Bernard, showing that the pancreas, like other glands, may be stimulated through its nervous supply to increased and, finally, depraved secretions, are referred to. The observation of a butcher, "who deals largely in calves' sweetbreads, and is a shrewd and intelligent man—that there is a great difference in the qualities of sweetbreads, dependent upon the previous state of the nervous system of the animal"—is mentioned. Again, it is argued that from the effect of nervous influences upon secretion generally that of the pancreas would be no exception. We do not for a moment doubt that the pancreatic secretion may be temporarily or even permanently interfered with through the nervous system, but what evidence have we that in tuberculosis the innervation of the pancreas is at fault? Dr. Dobell has attempted to make experiments upon

the pancreatic juice of persons who had died of phthisis, but on account of insuperable difficulties the attempt was fruitless. We look forward with interest to the promised observations of Dr. Fenwick, showing that the pancreas in man and animals is peculiarly liable to various forms of degeneration. Still, supposing this to be the case, the laws of scientific induction demand that these be shown to stand to tuberculosis in the relation of cause and effect.

So much for the *direct* evidence of the pancreatic causation of phthisis. Let us examine the *presumptive* evidence which Dr. Dobell brings forward in proof of his theory.

The first point on which particular stress is laid is the importance of fat in the economy. Various quotations from chemical writers are given, all to the effect that fat is very essential to life. Next, it is pointed out that in the blood of phthisis saponified fatty matters are defective,[1] and that tubercle may be regarded as protein from which five atoms of carbon, one of hydrogen, and one of oxygen have been removed.

The symptomatology of phthisis is then referred to. In the author's opinion the emaciation, hæmoptysis, and arrest of menstruation, are all consistent with his theory. In the hæmoptysis he sees a sign of the invasion of albuminoïd tissue and of breaking up of organic structure. In other words, he sees an announcement of tubercular deposit. This fact is readily granted, but we fail to see its bearing upon the hypothesis that "tuberculosis is due to defect in the action of the pancreas." Again, the arrested menstruation in phthisis he views as a proof that the system can no longer spare the surplusage of carbon which (it is held), it is one of the objects of menstruation to eliminate.

The order in which primary tuberculisation occurs is supposed to be favorable to the hypothesis—"The lungs are the organs in adult life in which *par excellence* the required combination of conditions for precedence in tuberculisation exist." The small intestines and mesenteric system are the parts most nearly resembling the lungs in this respect. A difficulty occurs to the author in the case of the bronchial glands which in children are frequently the primary seat of tubercle. He supposes that by the absorption of oleaginous matters through the portal system the lungs may be temporarily protected. By this compensatory action of the portal system may be explained the

[1] In one of the most modern works on animal chemistry it is stated that in chronic diseases and in tuberculosis there is an *increase* in the quantity of fat and cholesterine in the blood, also that no correct observations exist proving *reduction* in the amount of fat in the blood. (See 'Lehrbuch der Physiologischen Chemie,' by Gorup-Besanez, Bd. iii, p. 339.)

origin of fatty liver. An inordinate quantity of oil being absorbed from the food, it is deposited in its passage through the liver. But Dr. Dobell finds the most remarkable evidence of the truth of his theory in the explanation it furnishes of the mass of apparently incongruous facts in the etiology of tuberculosis. He conceives that the formation of tubercle may depend upon such causes as disappointment, home-sickness, poverty, defective expansion of the chest, persistence of young persons in a diet deficient in milk, inattention to cutaneous function, chronic alcoholism, crowding together of people, cyanosis, diabetes, acute diseases, childbirth, superlactation, weaning, and hereditary taint. Now, we must differ from Dr. Dobell in regarding this list as comprehending the real or proximate causes of phthisis. It is rather a summary of the conditions under which the proximate cause operates. In all these conditions our author discovers pancreatic relationships; thus, he gives us examples of causes acting directly on the pancreas—hereditary tendency to pancreatic diseases, occupations causing pressure on the pancreas, acute diseases, especially continued fever and inflammatory affections of neighbouring parts. Has he observed any cases of phthisis in which he could conclusively assign the disease to the operation of such conditions? His second group of causes comprehends those acting indirectly on the pancreas by diminishing the elimination of carbon from the blood, and thereby reducing the normal call for the introduction of fat from the food into the blood, and as illustrations he gives cyanosis and chronic alcoholism. In both these general nutrition is interfered with irrespective of any special effect upon the pancreas, at least none that has yet been demonstrated. The third group includes causes which deprive the system of carbon to such an undue extent that the pancreas cannot keep pace with the demand made upon its function, as exemplified in diabetes. The fourth heading absorbs those remaining causes whose influence on the pancreas occurs through the nervous system.

The second part of the work is devoted to the treatment of phthisis, and three reports of cases—sufficiently numerous—are given in which emulsified preparations of various forms of fat were administered. These cases support the proposition that pancreatic emulsion of solid fat may be most advantageously given in phthisis. It would appear to possess the advantage of agreeing with the patient when cod-liver oil does not. Dr. Dobell's favorable experience of the remedy is such as to make it worthy of trial by others, and we trust that it may be fairly tested by the profession.

Reserving our opinion that the pancreatic theory of the origin

of phthisis is one which remains "not proven," we sincerely thank Dr. Dobell, who is already most favorably known to the profession from his work on the chest, for having laboured honestly and hard to bring forward all available evidence in favour of the view he adopts, and for having given this to the profession in the collected form of a memoir.

ART. VI.—*The Journal of Anatomy and Physiology.* Conducted by G. M. HUMPHREY, M.D., F.R.S., Professor of Anatomy in the University of Cambridge; WM. TURNER, M.B., F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh; ALFRED NEWTON, M.A., F.L.S., Professor of Zoology and Comparative Anatomy in the University of Cambridge; E. PERCEVAL WRIGHT, M.D., F.L.S., Professor of Zoology in the University of Dublin; and (as Editor) J. W. CLARK, M.A., Fellow of Trinity College, Cambridge, and Superintendent of the University Museums of Zoology and Comparative Anatomy. No. I, November, 1866. Pp. 188.

CONSIDERING by whom this new Journal is conducted and edited, it is unquestionably begun under favorable auspices. Whether it succeed and have an enduring life, or fail and have a short existence like so many other praiseworthy undertakings, must depend, we will not say on the abilities of the gentlemen who have the charge of it, for their abilities cannot be questioned, but on the interest they take in it, and the care and industry which they exert in supplying it with valuable and attractive matter, such as can hardly be obtained in other and cheaper publications.

From an advertisement which preceded it, we learn that it is to appear half-yearly, and that the greater number of its pages will be devoted to original articles. These articles, we apprehend, should constitute its strength, and, if not disappointing, should ensure its vitality.

Whether an annual volume would not have a better chance of success, we hardly venture to offer an opinion. Taking into account the comparatively small number of persons in this country who are engaged in physiological inquiry, or who feel an interest in physiological discoveries, we are inclined to think that a yearly volume at half the price of the two half-yearly numbers might be in more request, and answer every useful purpose so far as the interests of science are concerned.

We are doubtful, too, whether another part of the design of the Journal as announced might not be omitted with advantage, viz., reviews and critical notices of works; these, at least of all

books of any notoriety, being attainable in other periodicals, weekly, monthly, and quarterly; and, consequently, unless those contributed to this Journal prove of superior literary and scientific value, they will not add to its credit or enhance its claims for a wide circulation.

The third part of the design, that comprising reports on anatomy and physiology, if of wide scope, including notices of all new facts brought to light in either science both at home and abroad, cannot fail of being acceptable and highly useful. We need hardly remark, that it is this portion of the undertaking which will test the industry, learning, and judgment of the gentlemen who engage in it.

Of this first number we shall now say a few words. Its contents are—a preliminary Address on Physiology which was delivered by Professor Humphrey at the last meeting of the British Association for the Advancement of Science held at Nottingham; nine original communications; two Reviews—one of Mr. Owen's 'Anatomy of Vertebrata'—one of Todd, Bowman, and Beale's 'Physiological Anatomy;' followed by a report on Anatomy by Mr. Turner, and on Physiology by Dr. Rutherford; concluding with translations from Professor Donders, by Dr. Moore, and with analytical, critical, and other notices.

Professor Humphrey's Address we had the pleasure to hear at Nottingham, and we are glad to see it in print in so conspicuous a place in this new journal. It is thoughtful, able, and even eloquent, very properly insisting on the importance of physiology, its high aims, its comprehensive nature, and its great difficulties. We agree with him that even for the purpose of mental culture, irrespective of other and higher considerations, it is deserving of forming a part of university education, especially in our old universities, in which he seems to apprehend that "the old educational soil upon which so many generations have been trained is in some degree wearing out," and that, to be productive, new elements of fertility are needed, such as the natural sciences amply afford. In glancing at the present state and prospects of physiology, Dr. Humphrey, like Dr. Beale, expresses want of confidence in the new views regarding the hypothesis of the transmutation of species, partly founded on speculative considerations, partly on paucity of reliable facts. He thinks it not improbable that as in the organic world there are definite compounds, each perfectly distinct and unalterable without their destruction, so there may be analogous ones in the organic. Whilst Professor Beale's objection seems to be mainly founded on the differences of intimate structure observable in animals of distinct species, even, he remarks, "the anatomical differences between the corresponding tissues of closely allied

species are often so distinct, that the anatomist familiar with them could distinguish one from the other." "For example," he adds, "there is a recognisable difference between the unstriped muscular fibres of the bladder of the hyla, of the common frog, and of the newt. So with regard to the chemical composition of the corresponding solid matters, fluids, &c., of closely allied animals, remarkable differences may be demonstrated."

Of the original communications, all yield proof of good and reliable work. Some of them are better adapted for careful study and after reference than for cursory perusal. Such are Mr. Wood's article on the Comparative Anatomy of the Muscles of the Shoulder; Professor Huxley's on Human Crania; Mr. Turner's on Variation of Buccal Nerve; Dr. Cleland's on the Action of Muscles. Of the two papers on the Eye, Dr. Lightbody's on the Anatomy of the Cornea, and Mr. Hulke's on the Retina of the Amphibia, we can say more—that they deserve to be read by all who wish to have clear ideas of the remarkable structure of these parts. Mr. Turner, in his paper on the Gestation of Aries, a fish of Ceylon, makes us acquainted with another instance of the hatching of ova in a part seemingly abnormal, viz., in the mouth and brachial chamber of the male. It had been previously known that the same mode of gestation was peculiar to three American species. Why this peculiarity exists is a question for inquiry. It would be desirable to know more of the habits and habitats of these fishes, and whether the protection of the ova or their aëration is most advantaged by the hatching locality. Dr. Foster, in his contribution to Amylolytic Ferments, controverts some opinions on the subject; he maintaining—1st, that the ferment is not an albuminoïd body; 2ndly, that it is unchanged by its catalytic action; and, 3rdly, that there are other bodies and their secretions besides the salivary and the pancreatic, which contain the ferment, such as the blood and liver of certain animals, the pericardial, pleural, and peritoneal fluid, &c., and that putrefaction is not essential to the action of either of them. That the ferment itself is not necessarily wasted by its action, we had proof, we may remark, many years ago, in experiments on the fermentation of solutions of pure sugar excited by the leaven of the grape. The last paper we have to mention is that of Dr. Norris on Rigor Mortis, which he holds to depend, not on muscular contraction, but on some peculiar alteration in muscular tissue conducive to the immobility of its constituent elements, such as that stated by Brücke and Kühne, viz., the coagulation of a material in the interfibrillar juices of the muscles.

Of the other portions of the number—the reviews, reports, and critical notices—we have only to remark that they are excellent examples of the kind, and well adapted to leave a good name for the Journal.

If we view, as we have done, the names of the gentlemen who have inaugurated the work as of good omen, we cannot but see in the same light the university which has given it birth; at the same time, we feel some regret that Oxford is not associated with it in name, believing as we do, that there, as at Cambridge, there is an increasing feeling of the necessity of cultivating the physical sciences, of which, we may add, such substantial proof has been afforded by the former in the noble museum there erected, and in the excellent choice which has been made of professors to conduct the studies in question. Let it not be forgotten that there was a time when both these universities were the nurseries of the sciences, and that both have on their rolls the names of some of the illustrious men who took a lead in their cultivation. Whilst Cambridge can lay claim to Bacon, Newton, Harvey, and Cavendish, Oxford can reckon amongst her distinguished sons Bacon the Franciscan, Mayow, Locke, and Willis, and we might add Boyle, if a residence in that city for some years by choice might entitle him to be so considered.

ART. VII.—*History of a successful case of Amputation at the Hip-joint.* By J. SAMPSON GAMGEE, Surgeon to the Queen's Hospital, Birmingham, &c. &c. *London,* Churchill, 1865. 4to, pp. 33.

THIS monograph gives an account of a remarkable case in which amputation at the hip-joint was performed by Mr. Sampson Gamgee, at the Queen's Hospital, Birmingham.

Amputation at the hip-joint must always be regarded as an interesting operation, both on account of its comparative rarity and also on account of the great danger which attends it. It would have been well for surgical science if every case of the kind had been carefully reported. We should then be in a better position than we now are to ascertain the exact mortality which attends the operation, and to estimate its real value. But unfortunately here, as elsewhere, there is too much reason to think that the successful cases have been reported, while the unsuccessful ones have been allowed to pass unnoticed; and hence it happens, that any conclusion which we might draw from the published statistics of the operation would be very apt to mislead.

Much has been done during the last few years to promote a

regular and uniform system of registration in our public hospitals; and in proportion as this system becomes general, may we hope to arrive at the truth in reference to the value of this and other operations—so far, at least, as statistics can enable us to arrive at the truth on these subjects at all.

But Mr. Gamgee's case is one which deserves more than a mere statistical record, or a passing notice in one of our professional journals. It is a very remarkable case of its kind, perhaps *the* most remarkable on record; and we are not surprised that our author should wish to bring it prominently before the profession by the publication of the present monograph.

The operation was performed for the removal of an enormous tumour of the right leg. It reached from a little below the hip almost to the ankle, measured forty-eight inches in circumference, and weighed no less than ninety-nine pounds! It was diagnosed to be an ossifying enchondroma, and this diagnosis was confirmed by the minute examination of the growth which was made after amputation. The mechanical difficulties of the operation were considerable, but they were successfully overcome by means of some simple and ingenious contrivances devised for the occasion. The hæmorrhage, which has always been regarded as one of the greatest dangers in the performance of this operation, was controlled by an aortic tourniquet, so that the amount of blood which was lost was quite insignificant. The aortic tourniquet is one of the most recent additions which have been made to the surgeon's *armamentarium*, and its value has, as yet, been only partially ascertained. But already it has introduced great improvements into practice. It has enabled Dr. William Murray, of Newcastle, to cope successfully with that formidable disease, aneurysm of the abdominal aorta. It has enabled Mr. Syme to lay open a large gluteal aneurysm, and tie the ends of the artery—a simpler method of treatment than would otherwise have been possible; and it has enabled Mr. Gamgee, in the present instance, to remove a limb which was so enormously enlarged that it weighed more than the whole of the rest of the body, without the loss of any considerable amount of blood; and, in the after-treatment of the case, when secondary hæmorrhage supervened, it may almost have been said to have saved the patient's life. How far the aortic tourniquet may yet be found of farther use to us, by enabling us to deal with aneurysms or morbid growths about the pelvis or upper part of the thigh, it is impossible to say. This much, however, seems certain, that it will strengthen our hands in dealing with several very formidable diseases, which before its introduction were almost entirely beyond our control. Had it

not been for the aortic tourniquet, the difficulties and dangers of the present case would have been enormously increased; but as it was, the current of blood was so completely arrested that, as our author assures us, there was not much more hæmorrhage than in an ordinary amputation of the forearm.

Five days after the operation a profuse hæmorrhage took place from the stump, which was stopped by the application of the aortic tourniquet. If this instrument had not been at hand the bleeding might have been immediately fatal; at the best it would have required the wound to have been opened up and a ligature placed upon the bleeding vessel, and while all this was being done, the loss of blood would probably have been so great as to have reduced the patient beyond the hope of recovery.

The after-treatment of the case was conducted upon the supporting plan which is in vogue at the present day. From the first the patient was allowed an abundant supply of light nourishing food, with a small quantity of wine or brandy. The dressings were of the simplest kind, and more seems to have been done by accurate opposition of the flaps and well-regulated pressure than by any other means.

Under this treatment the wound healed kindly, and the patient rapidly improved in health. At the time this monograph was published—two years after the operation—he continued perfectly well, and had increased in weight more than a hundred pounds.

On the whole, the case reflects great credit on Mr. Gamgee. The accuracy of his diagnosis, the skill and courage which he showed in performing the operation, and the wisdom which regulated the after-treatment, are all points which deserve commendation. But we cannot say as much for the way in which the case has been reported. The literary merits of the volume before us appears to be small. For example, is it correct to say, "the opportunity *is availed of* for a commentary?" and yet this expression occurs twice. At page 30, in describing the characters of the tumour, our author says, that spiculæ of bone were found, "many of them transverse the whole depth of the tumour, and intersect in a variety of ways!" At page 27, he speaks of "teaching an enlighted and self-acquired experience." How can we teach self-acquired experience? Is not this a contradiction in terms? Besides these faults, there are many inelegancies,—broken and abrupt sentences, which are all very well in a case-book, but which are not in harmony with a work like this. We may be told that these are trifling points, which are not worth mentioning. But when a book comes before us which is prefaced by a subscription-list containing many distinguished names—a book which is printed with the clearest of

type, on the finest toned paper, and accompanied by the best of illustrations, we have a right to be critical, and to demand correct, if not elegant, writing.

The author has taken this opportunity to introduce a chapter on surgical dietetics, and another on the use and value of pressure; but his remarks on these subjects are rather commonplace, and might have been left out or abridged with advantage. On the other hand, his report of the case is far from being so complete as one could have wished. In reading it, many obvious questions arise in one's mind to which one can obtain no reply. For example, we are told that the catheter had to be used on one occasion a fortnight after the operation, but no explanation is given of the patient's inability to pass his water. Again, we should like to know whether the aortic tourniquet when it was applied with full pressure for ten minutes to stop secondary hæmorrhage, gave much pain; or was chloroform administered? Again, we are not told when the patient was able to leave his bed, and begin to move about on crutches. Again, when the operation was performed, there were many enlarged glands in the groin—what was their condition at the time the patient was discharged from the hospital, or at the date of publication—that is to say, two years after the operation? Again, was the patient able to bear his weight on the stump at the time this account was written? All these are interesting points upon which we should have liked information, and others of a similar kind might have been added to the list. We are therefore of opinion that Mr. Gamgee ought to have given us a fuller and more detailed account of the progress of the case than he has done. If he had been reporting the case in one of our weekly journals, he could hardly have given a more meager account of it than the one which we find in this monograph. And yet the case itself is so remarkable, and is so likely to be often referred to hereafter, that it ought to have been recorded with the utmost minuteness and with the fullest detail.

It is remarkable and worthy of notice that this patient had on the left leg a couple of small exostoses, connected apparently with the tibia. They were said to be congenital. It would seem, therefore, that he had a constitutional tendency to err in the deposition of bone—a tendency which culminated in the enormous ossifying enchondroma which was the subject of this operation.

We regret to see that the tumour which was removed in this case has been deposited in the Musée Dupuytren, at Paris. Would it not have been more appropriate if it had been preserved in the Hunterian Museum of our own College of Sur-

geons? It would there have been as well cared for, as open to inspection, as accessible to scientific men, as it can possibly be in the Musée Dupuytren. The author assigns no reason for preferring the Paris to the London museum.

The four photographs with which this volume is illustrated are some of the best applications of the photographic art to pathological purposes that we have seen.

ART. VIII.—*Miscellaneous Observations on the Blood.* By JOHN DAVY, M.D., F.R.S., &c. (From the 24th vol. of the 'Transactions of the Royal Society of Edinburgh.')

THE author considers, first, the action of water on the red corpuscles; then the changes which take place in the blood when excluded from the air; then the action of the air-pump on the blood; then the effect of a low temperature and of ammonia; and concludes by remarks on the coagulation of the blood.

1. *On the action of water on the red corpuscles.*—Selecting the blood of the common fowl and duck, he found that when one measure of water was added to one of serum holding red corpuscles, few only underwent change; but on addition of three of water, none of a normal form could be seen, all that were visible (originally elliptical) having become rounded, lessened in size, and less distinct; many had a jagged outline, and some a slight projection suggestive of rupture. Dried by evaporation, many recovered their original form and size; some were ruptured, their nuclei being retained or not, and all were wasted. On the addition of four of water, the corpuscles were less clearly seen, but were visible; and when more water was added the corpuscles were not more affected.

The longer the blood is kept, the smaller is the quantity of water required to alter their form. As showing the hygroscopic properties of the corpuscles, one expiration of the warm breath will define them, previously dried, of their elliptic form. This action of warm vapour, or even of water, is arrested if the corpuscles be coloured by a weak solution of iodine; and Dr. Davy asks whether it may be, therefore, conjectured that iodine medicinally used may operate in a degree similarly, and thus arrest undue metamorphic disintegration.

2. As regards the *changes which occur in blood when excluded from the air,* from several experiments on the blood of the common fowl, duck, turkey, and bullock, the results seemed to

be nearly identical with those which occur when blood of the same temperature is exposed to the air, the only difference being in degree. This appears to be owing to the fact that blood contains oxygen in a state perfectly free to act and give rise to putrefaction. Dr. Davy is inclined to think that the freedom from changes which blood often enjoys when retained stagnant in the living body is owing to the fact of the blood, having been exposed to the action of surrounding tissues, by which, though a change may have been slowly going on in it, the degraded or altered particles may have been carried away as they were produced, leaving the residue in its natural state.

3. As regards the *action of the air-pump on the blood* (that of the fowl, duck, sheep, pig, and bullock), the author observes as follows:

"1. From the blood of the common fowl the quantity of air disengaged has commonly been less than from that of the duck, sheep, bullock, and pig.

"2. The blood of all the animals, when taken from them shortly after feeding, has commonly afforded more air than from animals of the like kind when fasting.

"3. Florid blood, which it may be inferred is chiefly arterial, has yielded less air than dark blood, which probably is chiefly venous, and, accordingly, that which flows first, when an animal has been blooded to death, less than that which flows last.

"4. In a small number of instances, those of animals killed after a fast of many hours, the fresh blood yielded no air. In some of the trials which gave this result, the blood was mixed as it flowed with an equal quantity of water deprived of air.

"5. In no instance have I witnessed the disengagement of air from fresh serum, proving that the air, when extricated from the blood, is derived from the clot, and, it may be presumed, from the red corpuscles which are entangled in it.

"6. As might be expected, I have found the disengagement of air from the action of the pump more copious in summer than in winter; and also more copious from blood the fibrin of which has been broken up by having been agitated with shot previously freed from adhering air, than from the clot left entire. In the instance of the blood of the common fowl, which coagulates rapidly, affording a firm coagulum, even the puncturing of it makes a difference; air then escapes which before was retained.

"7. In many instances, blood which had yielded air on exhaustion has, after exposure for a few hours to the atmosphere, on repetition of the exhaustion ceased to yield air, and this when the first trial was stopped before the exhaustion of the air was nearly complete. This result, seemingly paradoxical, may have been owing to ammonia formed, which may have fixed carbonic acid; and that ammonia was formed, was proved by the hydrochloric test and the production of muriate of ammonia. It has been witnessed in the instance of both

venous and arterial blood, but most remarkably in the latter, and in warm weather oftener than in cold."

Again—

"What struck me as most remarkable in these experiments with the air-pump was the comparatively small quantity of air, in most instances disengaged from the blood, and its total absence in others, taking into account the quantity of carbonic acid liberated in the lungs during life in normal respiration, and also the quantity of air, both oxygen and carbonic acid, found in the blood by the German physiologists. Difference of temperature, comparing that of the hot blood circulating in the lungs in birds as high as 106°—108°, and in the sheep, ox, and pig as high as 104°—106°, with that of the blood of the same animals cooled to 50°—55°, may partly account for the result first referred to, but the second adverted to I cannot attempt to explain."

4. Regarding the *effect of a low temperature* on blood, he found that this, like a high temperature, appeared to promote, not only a change of form of red corpuscles, but also a change of composition, as indicated by the production of ammonia and the solution of the colouring matter in the serum. The disengagement of ammonia, and apparently its formation, appear to be independent of putrefaction and the action of oxygen; to be owing to a new arrangement of elementary parts. Meat, however, does not appear, from Dr. Davy's researches, to be liable to change from freezing. Manures, stale dung, &c., are, however, liable to this change; also lithate of ammonia and the mixed excrements of birds. He asks whether these facts do not suggest the propriety of reconsidering the treatment of manures, and if not the time of their application to the land, at least whether an addition should not be made to them, to fix the ammonia.

5. Regarding the action of ammonia on the blood, its effects on the ENTIRE BLOOD was found to vary as to the quantity used:

"31 per cent. occasioning a thick adhesive coagulum, with a change of form of the red corpuscles, without the separation of any serum; 2·5 per cent. retarding the coagulation many minutes, but not preventing the separation of serum and a certain contraction of the crassamentum; 0·44 per cent. retarding the coagulation, and rendering the coagulum soft and viscid, barely semi-fluid, with little separation of serum, and that viscid; lastly, 0·17 per cent. had little effect, except that of retarding for a few minutes the coagulation—the coagulum, when formed, having very much its normal appearance."

Its solvent effects on the dried or moist FIBRIN of the blood

were found to be very feeble indeed; whilst it renders the *serum* of blood "less viscid, prevents its putrefaction, and modifies in some degree its coagulable property." Its action on the *red corpuscles* of the blood of the bullock is more decided, their colour being quickly darkened, their size much reduced, and their shape rendered globular. After some days the corpuscles disappeared, as if in great part dissolved, only minute granules remaining. Adverting to the hypothesis (that of Dr. Richardson) that the coagulation of blood is owing to the escape of ammonia, Dr. Davy proceeds to observe—

"Seeing that ammonia in so large a quantity as that used (in his experiments) did not prevent the coagulation of the blood, or, in other words, of its fibrin—its coagulable part—it would be strange indeed if the escape of a very minute quantity of the volatile alkali —hardly an appreciable one, at most—should be the cause of the phenomenon."

He finally alludes to the view taken by Professor Lister, that the blood's coagulation as mainly depending, out of the body, on a kind of catalytic action produced by the contact of any foreign substance, and within the body, as owing to an analogous cause—contact with a part either dead or *quasi* living, as he supposes a tissue to be, under the influence of inflammation. This view he thinks open to the following objections :—

"1. Were it true, ought not the phenomenon of coagulation to take place in every instance in which dead matter comes in contact in the living body with the blood? Instances of ossification, in which concretions of phosphate of lime are formed in the arterial coats, and often project into the vessels themselves—concretions differing but little from the 'tartar' deposited so often on the teeth, and inorganic—are familiar to every one acquainted with pathological anatomy, and yet in the majority of these cases the coagulation of the blood has not taken place.

"2. In instances of aneurism, with a rupture of the vessel, the seat of it, a coagulum of blood is invariably formed, though in contact with parts which, it may be presumed, until the contrary is proved, still retain their vitality.

"3. Examples of the coagulation of the blood in the veins, in the arteries, and in the ventricles of the heart, during life, in persons reduced to a feeble state by disease, are not of unfrequent occurrence, and this often without any apparent lesion in the coats of the vessels themselves, or in the lining membrane of the ventricles.

"4. Confirmatory of the last, many examples are on record of the blood, in its coagulated state after death, having been found broken up in the left ventricle of the heart, proving that its coagulation must have taken place whilst the heart was still forcibly acting, and this in cases in which the organ appeared to be sound.

"5. Certain poisons influence the coagulation, some accelerating

it, some retarding it. As an example of both may be mentioned the poison of a snake, the tic-polonga of Ceylon (*Daboia Russellii*, Gray), which on fowls acts with extreme rapidity, so much so that simultaneously with their death it occasions the coagulation of the blood in the heart and great vessels, and this even before the former has ceased to act; whilst in larger animals, such as the dog, in which it takes effect less rapidly, causing death in an hour instead of about a minute, it has a contrary influence, that of preventing the coagulation of the blood. There are other considerations which seem to me to cast a doubt on the accuracy of this hypothesis. To reconcile it with certain facts, its author is under the necessity of assuming that a clot is a 'living tissue in relation to the blood.' If so, then does it not follow, in strictness of reasoning, that such must be its state under all conditions, whether formed within the body during life, or in blood abstracted by the ordinary operation of bloodletting? And he is further under the necessity of assuming that inflamed parts are *quasi* dead parts, or, in other words—and they are his—'have lost for a time their vital properties, and comport themselves like ordinary solids.'

"The vagueness, moreover, of the hypothesis renders it open to objection. The referring the phenomenon to a catalytic action seems to be little more than the accounting for what is obscure by that which is equally or hardly less obscure.

"To conclude, I fear it must be confessed that, strictly speaking, the theory of the coagulation of the blood, its *vera causa*, is still an unsolved problem, there being, to all the hypotheses which have hitherto been propounded, opposing facts logically in strictness prohibiting the establishment of any one of them."

ART. IX.—*The Common Nature of Epidemics, and their Relation to Climate and Civilisation, &c., from writings and official reports by Southwood Smith, M.D.* Edited by T. BAKER, Esq. pp. 130. 1866.

Notes on Epidemics. By F. E. ANSTIE, M.D., F.R.C.P. pp. 179. 1866.

EPIDEMIOLOGICAL subjects are now attracting much more attention than they did five and twenty years ago, and it is fortunate that such is the case, considering their great scientific and practical importance when pursued aright. To arrive at sound conclusions respecting the chief favouring causes of the maladies that are most destructive to life, it is needful to look at them not merely in individual patients, or in small and limited groups of patients, as must be done in the ordinary course of professional practice, but also in large and dis-

seminated aggregates of cases, and under the varying conditions of human existence in different places and in diverse communities and regions. The study of the social and domiciliary conditions of peoples, of seasons and climates, and of topographical and geographical peculiarities, on their development and spread, together with the operation of those obscure cyclical changes to which all the departments of animate existence seem to be liable, affords fruitful topics of curious and most instructive research which, if patiently and thoughtfully carried out, are sure to issue in valuable results. It is by pursuing such inquiries as these that the science of medicine may justly claim to be one of the most attractive and useful branches of natural history, as well as to be indispensably necessary to the right solution of some of the most important problems of social and public economy. Hitherto the great value of State Medicine has not been duly recognised in this country; but, thanks to the labours of some distinguished members of the profession, its high importance is yearly becoming more apparent.

The first decided impulse given of recent years to the subject was unquestionably by the establishment of the general registration of births and deaths, with the signal success of which, and with the manifold public benefits which have flowed therefrom, the name of Dr. Farr will ever be associated. The labours of the Board of Health, instituted seven or eight years later, served to throw much light on the connection of hygienic neglect with the prevalence and fatality of the ordinary epidemic fevers of the country, and thus powerfully to promote the study of sanitary medicine. To the late Dr. Southwood Smith the merit of this work is pre-eminently due. The characters of his thoughtful mind, ever animated by pure philanthropy and benevolence, are strongly reflected in the book (a reprint) now before us; it well deserves perusal, not only by the medical profession, but by the educated public generally. An extract or two is all that we can afford space for. Respecting the change in the general type of disease in the metropolis during the last thirty-five years, Dr. Smith remarks, after citing the description by Sydenham of what was observed in London for several months before the advent of the great plague:

"In 1831, in the wards of the London Fever Hospital, I observed and recorded a precisely similar change in the general type of the fevers in London, six months before the first visitation of Cholera. Anterior to that period, fever in London, for a long series of years, had been essentially an acute, inflammatory disease, for which blood-

letting and other depleting remedies were indispensable. At this period it ceased to be an inflammatory disease; it became a disease of debility, in which no one could think of bleeding; and so closely did the prevailing fever now put on the general character of the approaching plague, which was as yet six months distant, that the fever into which those cholera patients fell, who were not killed by the first stroke—the consecutive fever as it was afterwards called—could not be distinguished from the primary fever in the wards of the Hospital when Cholera was at its height, which had appeared there for the first time six months previously, and which has never disappeared since.

"It is further very remarkable that the Professors of Veterinary Medicine and Surgery in London noted at the same time a similar change in the type of the diseases of the lower animals—horses, cows, sheep, and all domestic creatures;—a change requiring a similar modification of the remedies which they had been in the habit of using."

On the much agitated question of the value of Quarantine for preventing the importation by ships or otherwise of epidemic diseases, it is justly remarked:

"It is necessary to examine the questions of contagion and quarantine apart from each other, because there are points of obscurity, and therefore grounds for controversy, which, in the present state of our knowledge, may be reasonably considered as belonging to the former, that do not attach to the latter. The inquiry with reference to quarantine, indeed, is simple and lies in a narrow compass. The sole question to be determined is, whether or not it accomplishes, or is capable of accomplishing, its professed object, and this is a mere question of evidence and experience."

The following useful precaution merits special attention:

"The sanitary regulations of the ships themselves—a measure of the utmost importance to the seafaring classes of the community—would accomplish far more than could be hoped for or pretended to be accomplished by any known system of quarantine, and would have, moreover, a beneficial effect upon popular opinion by removing the fallacious appearances which favour the belief in imported disease, while they divert attention from the true causes of disease, the removable and preventible causes that exist on the spot."

Dr. Anstie's little work, which is an expansion of a recent interesting article in the 'British Quarterly Review,' shows him to be an able physician and a clever writer. Whether he is right in having it announced on the front binding, as well as on the title page, that it is "for the use of the public," is questionable. Much of it may doubtless be read by unprofessional persons with advantage, for the descriptions of diseases are generally

clear and simple, and most of the advice as to treatment is thoroughly sound. Few medical men, however, we presume, will go quite so far as our author as to the amount of professional knowledge and skill that are desirable among the public in general. In Dr. Anstie's opinion—

"The mother of a family should always be skilled in the application of the instrument (thermometer), since nothing is more uncertain and delusive than the sensations of young children who complain of feeling ill, and the application of the hand to the skin is a very poor and imperfect method of determining real temperature."

And not content with wishing that mothers, and heads of households generally, should have "a sound working acquaintance with the value of premonitory indications, and especially of such infallible criteria of the severity of threatened mischief as are supplied by the indications of the thermometer," Dr. Anstie deems it desirable that "the study of really good pictorial representations of the characteristic symptoms of the contagious fevers should become general amongst women of every class."

We would suggest to Dr. Anstie to make this little work the nucleus of a larger and more comprehensive one, to be patiently elaborated during successive years of study and research. The subject is a good one, and, if well handled, such a work would be acceptable and useful to the profession.

ART. X.—*The Physiological Anatomy and Physiology of Man.* By ROBERT B. TODD, WILLIAM BOWMAN, and LIONEL S. BEALE, Fellows of the Royal Society, &c. A New Edition, by the last-named Author. *London.* 8vo. Longmans. 1866. Part I.

WE have much pleasure in welcoming a new edition of this valuable text-book, appearing as it does under such able auspices. The works on Histology in the English language are singularly few; and at the time when the original edition of this book was published, it was hardly possible to direct the student to any good and comprehensive treatise on the structure and functions of the several organs and textures of the body. Even now, after the lapse of four-and-twenty years since the first part of Todd and Bowman's 'Anatomy' was issued, no work has taken up precisely the same position or has presented so scientific and lucid an exposition of the minute anatomy combined with the physiology of the tissues. Both Dr. Car-

penter's and Dr. Kirke's 'Physiology' are almost exclusively occupied with the consideration of questions of pure physiology, and give but few details of microscopical anatomy. Kolliker's 'Handbook,' on the other hand, is almost entirely taken up with histological details somewhat dryly given, and contains but little information on physiological points.

The extreme originality and accuracy of both the late lamented Dr. Todd and Mr. Bowman in their microscopical investigations caused their joint work to be everywhere, immediately after its publication, accepted as a standard for reference; and it is gratifying to find that in some of the most important of their observations—as, for instance, those of Mr. Bowman on the kidney and on muscle—although they have been vigorously assailed within the last three or four years, the progress of inquiry has triumphantly established their general accuracy, and we may venture to say that on this point at least, whilst in preparing the new edition scarcely a word of the old need be left unsaid, a very few and trifling verbal alterations and additions will bring up the description to the level of the most recent observations.

Dr. Beale, it is well known, has spent much time in the solution of histological problems; and from his intimate acquaintance with the authors, and from his having been already engaged in the publication of the later parts of the original work, we believe him to be excellently fitted for working up the new edition which is now appearing.

The present part contains—1. The Introduction; 2. A chapter on Structure; and, 3. A chapter on Chemical Composition. In the first, considerable additions have been made to the original edition; the second and third are almost new. The chapter on Structure contains an exposition of Dr. Beale's views on the nature and properties of cells and intercellular substance.

We think that Dr. Beale's views deserve more attention than they have yet received. Broad generalisations like that which he has proposed are of great importance in forming the basis of science; but before they can be accepted and any superstructure raised upon them, it is of the utmost importance that they should be examined on every side, that they should be tested in every possible way, and that they should be found capable of furnishing a full and satisfactory explanation of every instance that falls within their scope. That there are parts of the body which can scarcely be called living is unquestionable. The surface of the skin, the hair, the nails, and enamel of the teeth—and, as Hunter with great acuteness perceived, the fat—are to all intents and purposes dead matter; the former living altogether without the body, and being alike destitute of vessels and nerves, whilst the last is only freely supplied with vessels

in order that it may be either taken up or deposited as circumstances may require.

In many other parts, as the bones, the tendons, and ligaments, it is generally acknowledged that the changes which take place are slow and gradual; and that, whilst exhibiting few vital phenomena, the main purposes which they subserve in the body are of a mechanical nature.

Dr. Beale seeks to show that throughout the animal economy, in every organ and in every tissue, dead matter—matter which has ceased to manifest purely vital phenomena—is present, and is present, moreover, not in the mode that was formerly supposed—namely, as disintegrated particles of the tissues, which, having performed their work are *en route* to be discharged from the body—but in the form of fixed and definite parts of each tissue. Thus, in every case he regards the cell-wall as dead matter, or, as he has termed it, *formed material*; whilst in the cell-contents he finds the true living substance or germinal *matter*. The latter exhibits vital phenomena, is abundant in young and growing organs, and is capable of being stained with carmine; the former has ceased to exhibit vital phenomena, though it may be the seat of energetic physical change, is formed at the expense of the germinal matter, and is not coloured by solution of carmine.

The cases of nerve and muscle will at once arise in the mind of every thoughtful man as means of testing the soundness of Dr. Beale's theory. If muscle be composed of formed material, and therefore of dead matter, we find it difficult to comprehend why an isolated fibre should contract when mechanically irritated; and we are not yet convinced that during and after exertion its substance does not undergo renewal, extension, and development. If not, whence come the hypertrophied muscles of an opera-dancer if it do, unless Dr. Beale be a convert to Dr. Radcliffe's views of the nature of muscular contraction, we must admit the formed material to be the seat of some of the most vital phenomena with which the physiologist has to deal. Again, if the cell-wall of a nerve-cell be dead matter, we cannot understand how a nerve, as in the case of the spiral fibre of the sympathetic ganglion cells, can take origin from it, as Dr. Beale has himself particularly described and depicted. If the cell-wall in other tissues be incapable of exhibiting any purely vital phenomena, we are at a loss to account for the movements observed in various cells (cornea, connective tissue, corpuscles, &c., Kühm), or for the division and subdivision and other changes observed in the germinal vesicle, &c.

We have made these comments in no unfriendly spirit to Dr. Beale, but rather with the object of calling his attention to what

appear to be weak points in his theory, and which must be fully explained before it can be generally accepted.

A short but interesting section is devoted to the preparation of the tissues and of morbid growths for microscopical examination with high powers.

The chapter on the chemical constitution of the principal compounds found in the body is very well brought up to recent knowledge; and the plates, of which there are eight, several of them coloured, are very clearly and well drawn. We look forward to the remaining numbers of the work appearing with regularity and despatch; and we are sure that the revision to which they will be subjected by Dr. Beale, while it will extend his well-deserved reputation as a physiologist and a careful and conscientious observer, will confer additional value upon this well-known and most useful work.

ART. XI.—*On the Nature of Cholera as a Guide to Treatment.* Second Issue, with a new Section on Treatment. By WILLIAM SEDGWICK, M.R.C.S., Surgeon to the St. Marylebone Provident Dispensary. *London,* 1866. Pp. 200.

MR. SEDGWICK'S work, a new issue of which we have in a previous number of the 'British and Foreign Medico-Chirurgical Review' alluded to, has deservedly attracted attention, as no one can read it without being a gainer thereby.

The work contains a *résumé* of several highly important theories regarding the pathology and treatment of cholera. The main features of the disease are referred by Mr. Sedgwick to a disordered condition of the sympathetic nerve and of the stomach, and he displays considerable ability in the reasoning and analogies whereby he endeavours to reconcile the prominent features of the disease to this line of thought.

"The very indefinite information we possess respecting the functions of the sympathetic nerve in health must necessarily increase the difficulty of investigating its diseases, and the strongest arguments adduced on some points connected with the subject will consequently be found to amount to scarcely more than good presumptive evidence in favour of the theory proposed. Although this may at first sight appear unsatisfactory, yet the same is the case with many other diseases of frequent occurrence, which we are equally at a loss to refer to their primary seat, and can only guess at their probable nature from symptoms which often admit of more than one interpretation, and which are neither uniform nor constant in their occurrence. No difficulty of this kind can generally be complained of in

cholera, as its symptoms are, for the most part, sufficiently definite in their character and regular in the order of their occurrence as to leave the observer in no doubt respecting what disease the patient is suffering from. This is a point of considerable importance in the pathology of cholera, and contrasts favorably with many other diseases referable to the nervous system, and more especially with the Protean character of the symptoms in hysteria. It indicates that cholera proceeds from some uniform cause, which, however mysterious it may be in its process, is certain in its results," &c. (P. 2.)

The following is quoted by the author from the 'Bombay Reports on Cholera.'

"The united testimony of all who have witnessed it (cholera) seems to show that there is a *somewhat* which presses heavily and suddenly on the vital functions, and on many occasions resembles the effects of a poison taken into the stomach or applied to the blood; but whether it acts more immediately upon the circulatory system or the nervous, we cannot determine."[1]

Mr. Sedgwick regards cholera as dependent on the influence exerted upon the sympathetic nerves, denying the Snow theory and the contagiousness of the disease.

He attributes the altered condition of the blood to the influence of the nervous system.

Speaking of the relation between collapse and the amount of loss of fluid from the stomach and bowels, he quotes Dr. Gull, who states—

"That although, in a large number of instances, the intensity of the symptoms is in a general way proportionate to the amount of the effusion, yet that this will not in part explain the attendant collapse, which often appears to be in no inconsiderable degree due to the adynamic state of the ganglionic nervous system."[2]

Mr. Sedgwick bears strongly upon the nerve influence in cholera throughout his clearly written work. He observes (after having spoken of the occasional fluidity of the blood continuing to the last)—

"Consequently, the changes which occur in the blood during life cannot, correctly speaking, be said to originate in the blood itself, but to be dependent on the condition of its containing vessels, and effected only by their agency."

This sole reference of the state of the blood to the condi-

[1] 'Bombay Reports on Cholera,' Bombay, 1819, Preface, p. 81.
[2] 'Report on the Morbid Anatomy of Epidemic Cholera,' 1854, p. 134.

tion of its containing vessels is hardly justified by our present knowledge. Mr. Sedgwick deserves credit, we think, for at the present time drawing attention to the consideration of taking blood in cholera, but we cannot help feeling anxious to see returns of cases in which he has found it actually of use, as we fear there is ground for the very general abstaining from the practice, notwithstanding that he takes Dr. Gull's report to task for arguing that disuse of the practice inferred its general inadmissibility.

We have before quoted[1] the practice in Paris of leeching the mastoïd process when meningitis occurred in the stage of reaction, and we will now quote previous authors on taking blood in cholera.

Dr. Aitken[2] observes—

"With respect to bleeding, it may be stated that in every country the patients bore bleeding badly in any stage, and that the practice in Europe was at length limited to a few leeches occasionally to the head."

Dr. Maclean in the same work says, "The secretion of urine may be promoted by dry-cupping over the loins." In the New Sydenham Society's 'Year Book,' 1864, we find the following from the 'Reports' of the Vienna Hospital,[3] 1860-61-62, in reference to bleeding in pneumonia; and it has a bearing on the general effect of the measure in other diseases, and is worth remembering in connection with cholera.

"Depressing mental conditions, in an atmosphere laden with malarious exhalations and impure matters, or that of great cities, especially in narrow streets, damp dwellings, deep cellars, crowded hospitals, hinder the good effect of bleeding, and make it even injurious."

In Dr. Gull's report we find the following:—

"These scattered facts correspond with common experience, and lead to the conclusion that, in the premonitory and early stages, bleeding is in general to be avoided. In the stage of collapse it is for the most part impossible to remove an amount of blood sufficiently rapidly to produce any obvious effects. A few cases, however, are contained in the communications received, and similar ones occurred in the practice of most who were occupied with the disease, in which the abstraction of a few ounces of blood relieved the cramps and sense of oppression complained of at the præcordia, and was followed by favorable symptoms of reaction."

[1] July, 1866, p. 147.
[2] 'The Science and Practice of Medicine,' 1866, p. 663.
[3] Schmidt's 'Jahrb.,' vol. 123, p. 244.

The following case reported by Dr. Haycraft, of Greenwich, is remarkable:

"In one severe case a (male, æt. 27), which ended fatally in ten hours, and which from the beginning was marked by great prostration and extreme oppression at the epigastrium. we tried cupping. The blood, contrary to expectation, flowed freely to the extent of eight ounces, and the patient expressed himself strongly and thankfully as much relieved; but within five minutes he fell back and died. Before the cupping, the voice was husky; but immediately afterwards its tone was clear and distinct."

The report goes on to say, that though

"In the consecutive fever local depletion seems more directly to be indicated, nevertheless experience shows that its employment requires much caution. Leeching the temples, or cupping the back of the neck, were sometimes of service in obviating the cerebral congestion of this period; but if carried to any great extent, were injurious by exhausting the patient. Dr. J. C. Müller[1] reports that of 69 cases in the typhoid stage treated by general and local depletion, 48 died. In only 2 did the treatment produce any improvement, the results for the most part being negative or unfavorable. In the experience of others, leeches to the temples sometimes relieved the coma, and the patients were so much benefited as to date their recovery from their application.

"In a note subjoined to the report from the Obuschowschen Hospital, it is stated that bloodletting in the stage of reaction requires much discretion, though well-marked signs of pneumonia may exist. Of 18 cases of pneumonia in the consecutive fever, 6 treated by bleeding died; of 12 not bled, 5 died and 7 recovered.

"Leeches were applied with occasional advantage to the epigastrium in the consecutive fever, when there was tenderness, vomiting, and hiccup. Occasionally, under these circumstances, local depletion by cupping was employed with good effect. The general inference, from all we have observed of the disease and collected from the experience of others, is that large losses of blood in the consecutive fever are injurious, but that occasionally good results follow its local abstraction, according to the indications of the symptoms."

We would draw attention to the following, at page 205 of the 'College of Physicians' Report.'

"Again, the employment, without definite scope, of one remedy after another, with the vague hope of at last finding a specific, is to be deprecated, not only because it can lead to no good result, but because it deprives the patient of that assistance which established experience affords. The state of the patient in the collapse of

[1] Op. citat.

cholera is so unfavorable to the absorption of medicines, that even if we knew the remedy in itself most appropriate, we could not anticipate great results from its administration by the mouth at this period.

"Every consideration of this formidable malady urges upon us the paramount importance of obviating the causes which give rise to it, and of arresting the symptoms at the onset."

As mentioned in a previous number of this 'Review,' we have experienced in the only case in which we resorted to venesection, apparently excellent effects from it; and other observers have had like experience. We must not, however, overlook the amount of unsatisfactory evidence on this subject. At the same time, we have learned that some have considered that cupping the loins has been productive of benefit, and we hope that evidence on this point will be forthcoming ere long.

M. Leudet[1] observes, at Rouen as at Paris, the cholera presented the adynamic form. Vomiting in certain cases was not severe, and rapidly ceased: this, however, by no means indicated improvement, but the commencement of failure of power (*adynamie*), which terminated gradually in death. At the commencement of the epidemic, M. Leudet used emetics; afterwards he left off their use, and resorted to tonics and revulsives in the algide period, and antispasmodics in the typhoïd stage.

We cannot, therefore, recommend bloodletting in cholera, unless in exceptional cases, though sanctioned by so scientific an author as Mr. Sedgwick.

In a future number of the 'Review' we hope to be able to recur to a consideration of the important portion of the treatment proposed by this writer, when, no doubt, we shall be able to bring forward additional information on the subject of a recent nature.

[1] 'Archives Générales de Médecine,' Nov., 1866, p. 527.

PART THIRD.

Original Communications.

Art. I.

The Functions of the Tympanum. Part I. *Eustachian Tube.* By James Jago, M.D. Oxon., A.B. Cantab.; Physician to the Cornwall General Infirmary and to the Truro Dispensary.

The third review in the last July number of this Review considers "Recent Works on the Diseases of the Ear." It concludes that we have of late years witnessed a real progress in the knowledge of the pathology and treatment of these diseases, though it dwells upon many important discrepancies in such respects among the different authors.

It also affirms that we have at the same time acquired some additional anatomical and physiological information on the ear. As the latest discovery of the latter sort, it states that "Lucæ and Schwartze (Archiv. für Ohrenheilkunde) have shown that not only during the act of swallowing, but also during every act of inspiration and expiration, there is a slight opening and closing of the" (Eustachian) "tube, as indicated by a very slight falling in of the membrane" (of the drum) "at each inspiration, and bulging out of the same at expiration."

In the two set treatises on the Diseases of the Ear[1] which have most recently proceeded from the English press, it is agreed that it has fallen to my lot to point out the more especial reasons why the Eustachian tube is shut except in deglutition; though the fact itself, and the mechanism by which it is accomplished, were discovered by the late Mr. Toynbee.

The little essay[2] which propounded my inductions on this subject was not published in a way likely to bring it generally under the notice of aural students, though it was on its first issue commended in this Review. The Review, too, neglected

[1] Toynbee, p. 192; Hinton, 'System of Surgery,' ed. by Holmes, vol. iii, p. 158.

[2] "The Opening of the Eustachian Tube limited to the Act of Deglutition, now first rightly explained," in the 'Report of the Royal Institution of Cornwall for 1853.' "Eustachian Tube, why opened in Deglutition," bound up with "Ocular Spectres, &c." Churchill, 1856.

to tell its readers where it was to be met with; and the two books, which I have referred to as adopting its leading idea, neglect to tell theirs.

Besides, they do not detail the observations on which I founded my conclusions; and both one and the other mix up, more or less, preconceived notions of the authors with their enunciation, such as I cannot but repudiate.

When, again, I find it even now taught by some physiological writers and aurists that the Eustachian tube is rigidly patent, and a presumed discovery of Lucæ and Schwartze which would show it to be open not only when we swallow, but when the breath is going into the chest, and when it is leaving it (which amounts to much the same thing as a constant patency) accepted, I cannot resist the conviction that the contents of my essay remain unknown to the bulk of the profession to this day. I am bold to speak in this way because it will transpire that I owe my thoughts to an accident that deprives me of a great deal of the credit of entertaining them.

However this may be, the fact that the minds of physiologists are unsettled upon the normal condition and uses of the Eustachian tube must be my apology for venturing to lay before them what may be regarded as rather a revised edition of matter already published than as new. Still, in doing so I hope to interest any reader who may have looked into my earlier accounts, by the statement of particulars never yet published, and by a more correct exposition of my observations and inferences, as the result of subsequent reflection.

When I shall have fulfilled this intention, I hope, in a continuation of this paper, to submit also to them some other aural studies I have made, and which have only been partially in print. It will be seen that I have been impelled to such studies not only by a professional but by a personal curiosity, and have pursued them in some respects in a way peculiar to myself. The persistent want of coincidence among physiologists in their views as to the acoustic principles upon which the ear is constructed is disheartening, and I am far from imagining that it is in my power to determine the many moot points in a manner satisfactory to myself or to them. The most I hope is, that my investigations may help to convince some of my readers that certain views long regarded as established are untenable, and that I may be fortunate enough to contribute a few facts and suggestions which they may not regard as unworthy of their consideration.

But it is well I should announce here that I have a personal advantage in aural researches. This arises from the circumstance that the guttural orifices of my two Eustachian tubes

differ from each other in a very singular manner as to their degree of closure. In trying to explain how this happens, I will for the first time mention the heads of my *case*, because it will interest not only the aurist, but, as I believe, every medical practitioner.—Though not a robust I was a healthy boy, who roughed it in all weathers. Nothing used to ail me but occasional acute attacks of what passed at home by the name of stiff neck and sore throat, the latter meaning inflamed tonsils. It might once upon a time be troublesome enough to keep me from school or play for a few days, but never so much as for the family medical attendant to be consulted. These attacks may have indicated a tendency to relaxed throat, but at intervening periods I suffered no discomfort thereabout. My tonsils are not large and have never ulcerated. When, however, I was far advanced in my teens, as a consequence of a rash exposure to cold and wet, I was seized with some inflammatory affection of the chest. My medical attendant was no auscultator, and I can state no more about it than that in, say the first week of my illness, I had no expectoration, and for some weeks afterwards plentiful. After many weeks, in the spring of the year I got out of doors again, but for many months I remained very feeble, and was much tied upon my breath.

It was in rallying from this illness that I first became aware that I had an uvula; I was checked in eating a dinner by the sudden feeling of an impediment to swallowing. The pendulent end of the uvula had become expanded to above the size of a pigeon's egg by effusion of blood into it, as if a blood-vessel had been ruptured there. After some hours' use of a gargle which was prescribed for it, the blood escaped through a breach in the mucous membrane, and I was released from my difficulty. For years after, at times (say twice or thrice yearly), the same accident occurred whilst I was in the act of eating, and I got in the habit of seizing the uvula between my forefinger and thumb, and by means of the nails pinching or scratching through the mucous membrane at its end as the readiest escape from the annoyance. The bag of blood was emptied without any further bleeding.

During this time the uvula kept on elongating, and a relaxed condition of the fauces became chronic. I dare say that a laborious course of studies which I embarked in ere my strength was tolerably restored, tended to deteriorate my health and to aggravate the relaxation. It has been said that a long uvula may occasion cough by tickling the epiglottis. This effect did not occur in my case at any stage of the elongation, as I was never troubled with cough unless I chanced to have a catarrh, and I have never met with it in patients suffering

from long uvula. But a curious annoyance did finally befal me from great length of uvula. When I was about twenty-five years of age, and wearied and weak, though still at my work, it had stretched to such a length that it dipped into the pharynx so far that it often became caught by it in the act of swallowing, and pulled downwards, making me arrest the act spasmodically, under a feeling that it was dragging the whole of the soft palate from its bony attachment.

By the advice of a very eminent physician and by the hand of a distinguished surgeon a portion of my uvula was snipped off. Once or twice only after the healing of the wound did a small bag of blood appear at the truncated end. Only a little piece was amputated, but the uvula gradually contracted in cicatrizing, and subsequently, until at the present time it bears the aspect of a very stout, blunt uvula of full normal length. I now swallow without inconvenience, and enjoy excellent health.

I must add to this account, however, that the uvula does not hang straight from the palate, but curves to the right. It was either cut obliquely, or the cut has so healed that the right contour is concave and the left convex. Thus its weight, when it is pendent, is chiefly borne by the mucous membrane and subjacent textures of the right side; and generally there must be an undue tension of the textures on that side extending from the uvula to the bony attachments of the arches of the palate. Hence it is comprehensible that the tensor and levator palati muscles on this side may be rendered more tense than those on the left, or be made to act in directions deviating from the normal. And, assuming that Toynbee was correct in assigning to those muscles[1] the office of opening the Eustachian tube, it is comprehensible that this action may not be alike on both sides of my throat.

Whether or not it resulted from contraction of tissues in healing a singular difference in the behaviour of my Eustachian tubes, of which I had no experience previous to the operation, grew into existence so as first to attract my attention about five years afterwards. I noted phenomena which were to me an inexplicable puzzle, as long as I regarded the Eustachian tube as an open canal; but Toynbee's view, that the tube is only open in the act of swallowing, given in the 'Proceedings of the Royal Society' in 1853, just before the date of my first paper, made everything clear to me.

"In the act of swallowing," says this writer,[2] "the tensor and

[1] Von Troeltsch maintains that it is only the tensor that opens it, and that the levator rather closes it.

[2] 'On an Artificial Membrana Tympani,' p. 10. See also, by the same author, 'The Diseases of the Ear,' pp. 191-2, for a detailed explanation.

levator palati muscles open the guttural orifice of the tube, afford free egress to the mucus secreted by the lining membrane of the tympanum, and allow air to enter or leave the tympanic cavity. The closure of the tube, excepting during the act of deglutition, can be experimentally proved. To those accustomed to descend in a diving-bell, it is well known that the unpleasant sensation in the ears, amounting to positive pain, is capable of instant removal by the act of swallowing, during which the condensed air being allowed to enter the tympanum, and come in contact with the inside of the membrana tympani, the pressure on its outer surface is relieved by being counterbalanced." Thus far I concur in his physiology, though not so when he continues :—" Again, if an attempt is made to swallow while the nostrils are closed by the finger and thumb, a sensation of fulness and pressure is experienced in the tympanic cavity, in consequence of air having been forced, during the act of deglutition, through the open tube into the tympanum ; and this sensation continues until, by another act of swallowing, the tube is reopened, and the confined air escapes into the fauces."

Though in my paper I did not demur to his interpretation of the latter experiment, I afterwards did so as follows :[1]

" I assure myself that my Eustachian tubes are ordinarily shut, by the difficulty (greater for my left one) of forcing the breath into the drums when I stop my mouth and nose, and the hinderment to its escape till I swallow or eructate, showing that these acts open the tubes. If we mark the sinking-in of the lachrymal sac when we swallow with the mouth and nose stopped, we may see that the naso-guttural cavity *enlarges,* as the glottis is closed in that act, *producing a partial vacuum* in the drums, and therefore from the greater barometric pressure a feeling of tightness upon the membrana tympani, whilst from loss of usual pressure the Eustachian tube thereupon closes more firmly, and the faucial parts swell and stick together." In eructation, as the gastric bubble of gas passes the fauces, a muscular movement of the soft palate, enough like that in swallowing to occasion the Eustachian tube to open, may sometimes happen, especially in such a case as this, when there is a pressure tending to open it.

In his book, published in 1860, Toynbee adheres to his original inference from this experiment, but the following extract from a recent letter[2] of his will show that he finally abandoned his own in favour of mine. The person trying this experiment, he writes, " will at once feel a sense of pressure in each ear caused by the withdrawal of some air from each tym-

[1] ' Proceedings of the Royal Society,' vol. ix, p. 135, Feb. 1858.
[2] ' Lancet,' March 24th, 1866, p. 310.

panum, and that sense of pressure remains until the act of swallowing is again performed and air is thus readmitted."

In the second version of my paper (op. cit., p. 99), in speaking of the experiment of stopping nose and mouth, and blowing an excess of air into the tympanum, so as to force the membrana tympani outwards, I add: "The drum now is of unusual volume, and contains air of unusual elasticity, yet this is so plainly incarcerated, that if I refrain from swallowing, the excess is not lost for a very appreciable interval, the escape usually being accomplished in distinct jets, so palpably is there a struggle between the passive force of collapse of the tube, and the expansive force of the imprisoned air. If, on the contrary, I swallow under these circumstances, the membrana tympani either returns to its place at the very first act, or, at any rate, quite so after two or three acts of swallowing."

I will append, that the resistance to the excess is strikingly greater in my left than in my right tube. In Toynbee's book an argument of like kind, but with less particulars, is adduced.

Such observations, I take it, furnish conclusive evidence that the guttural orifice of the tube is usually closed, and that it is opened in deglutition.

It was suggested by Mr. Brook, that the acoustic purpose of the shut tube is to prevent the sonorous vibrations which have reached the air in the drum, through the external auditory canal and membrana tympani, escaping into the fauces, and spending themselves there; his idea being, that sonorous vibrations are intensified in perfectly closed chambers, and that we hear through the fenestra rotunda and tympanic air, and not through the fenestra ovalis and ossicula. Toynbee adopted these views, and applied them to explain the acoustic phenomena of a perforate membrana tympani, &c. In my paper I urged what I esteemed fatal objections against the theory on its own merits, besides from personal observations, but Toynbee continued to use it as the pivot of his physiological acoustics until half his book was in type. At about that point, however, he had become converted to the opinion, that we hear chiefly through the fenestra ovalis, and that it is not for an acoustic advantage that the tube is closed.[1]

The theory arose from a misconception, or from collating conditions which had no resemblance to each other. I must refrain from going into this subject here, since it would occupy too much space to do so. But were it true that sounds proceeding

[1] By comparing p. 192 with p. 294, it will be transparent that the physiological acoustics in the earlier portions of his 'Diseases of the Ear,' are quite at variance with those in the later.

from external air and entering a closed air-chamber through a portion of its wall, are intensified within it by reason of its being closed, we ought to hear sounds made outside the house better through a closed window than an open one, which every one knows is the reverse of what happens.

With respect to this provision of opening on occasion the Eustachian tube, I announced in the first version of my paper the following conclusions:

"*That it is not the egress of sonorous vibrations from the tympanum which is to be feared, that being a matter of indifference.*

"*But there must be a provision against the ingress of aerial undulations from the throat, which, if admitted, would threaten the membrana tympani with incessant oscillations, and endanger both its integrity and that of the complex and delicate apparatus in connection, and violate the peace of the labyrinth* viâ *this sudden route with all the sonorous impulses impressed upon the animal's breath.*

"*That, therefore, the moment seized for bringing the tympanum into communication with the fauces must be one in which there can be no respiratory current.*

"*That the only instant compelling a suspension of respiration is that in which the act of swallowing is performed, and must therefore be embraced for the service just named.*

"*Finally, the same rule secures the tympanum against the intrusion of gastric gases, &c., evolved through the fauces.*"

I have never seen any reason to modify these propositions, except in so far as to observe that our security against the intrusion of gastric bubbles is not absolute.

I have fallen upon these propositions by having had opportunities afforded me of making a series of observations with my right Eustachian tube in a patent state. But I am confident that any one who may provide himself with a tube open at both ends, just of a size to pass through a nostril into the throat, and having projecting from it another near one of its ends, suitable for being passed into the Eustachian tube, may, by thus introducing this into one of his tubes, so that the air may pass freely from it into the drums, verify all my main experiments.

In recurring, in order to relate the experimental use I have made of it, to the difference between my two Eustachian tubes in regard to force of closure, I may state that, as well as I can ascertain, the tightness of closure and the muscular action of the mouth of my left tube are normal. But I must repeat that in the case of my right one the closure is so gentle that I can at any time,

with stopped mouth and nose, blow air into the drum with the slightest effort; or, by lifting my ribs as if for inspiration, cause the membrana tympani to sink tensely inwards by the expulsion of air from the drum into the throat.

But the most curious fact is, that there are occasions in which my right Eustachian tube opens and remains open without any disposition to close. This event has happened almost exclusively in the summer or autumn; though an artificial atmosphere, like what prevails in those seasons, may produce it. I unfortunately never recorded the precise barometric, thermometric, and hygrometric conditions which favoured the occurrence, though for several years in the seasons mentioned I was very frequently, sometimes daily for weeks together, subject to the annoyance of an open Eustachian tube. In a hot dry air, when evaporation from the skin and lungs proceeds rapidly, the loss of water, it may be presumed, lessens the volume of all the soft tissues in the body—of the faucial tissues among the rest. In such a case my right tube, which is never more than weakly closed by a centripetal projection of its wall, may become an open one; because its parietal tissues may shrink to such a degree as to obviate its closure.

In proof of this explanation I may adduce that whenever I have been teased by the patency of the tube, I have always been able to close it for some instants by an inspiratory movement with nose and mouth stopped. This diminishes the aerial pressure in the throat, and allows its textures to expand, and those of my right tube sufficiently so to close it.

Again, as another criterion. When the patency of the tube has been troublesome, I have had the consolation of knowing that a hearty meal would be a remedy for several hours, if not for the remainder of the day. The faucial tissues, no doubt, imbibe moisture directly from fluids swallowed, but the remedy proves effectual so long that I presume the source of relief must lie in a renewed supply of blood or of its watery elements causing an increased turgescence of the soft tissues.

Finally, as further evidence in support of the explanation, I will add, that I have the gratification of being able to state that for several years past I have been growing free from attacks of an open Eustachian tube; insomuch that though the last summer has been a hot one, I have escaped them entirely, or at most have only once or twice suffered a transient premonitory symptom of an attack. This happy circumstance I ascribe to an increase of tissue about the mouth of the tube, in accordance with a gain in my bodily weight to the extent of one seventh of the whole taken at any time within the long period in which I was subject to the complaint.

As my notes were carefully made when I made my observations, I shall use the present tense in relating, which I am now going to do, the phenomena which ensue from an open tube.

When the closure of the tube becomes very weak, a jet of air in expiration, unawares, puffs into the drum, pushing the membrana tympani before it, whilst, perhaps, the orifice of the tube closes behind it. This leaves an unpleasant feeling of tension upon the membrane of the drum, and I am impelled to replace it by an inspiratory effort with closed mouth and nostrils. Things now being set to rights, the accident may not occur again; but if the force of closure becomes still weaker, the membrane flaps outward again at every expiration, until I am wearied with replacing it.

Or, the tube has become patent, and I abstain from eating and drinking to make observations. Then at each expiration, and, though in a notably less degree, at each inspiration, I *feel* the breath playing through the Eustachian tube and drum, and its membrane vibrating. At each expiration the membrana tympani is rudely thrust outwards and drags painfully at its marginal support of bone or periosteum. At each inspiration, when the faucial current of air is of less pressure than that in the external auditory canal, there is a tendency for the excess of pressure on the internal surface of the membrane first to disappear, and then for the excess to act on the outer surface, and restore the membrane to its place or push it inwards. However, I find that it is only occasionally restored in this way, though I feel its inclination to be so, or that it trembles from external pressure. The reason I take to be this, that the orifice of the Eustachian tube is lodged in a recess behind the posterior arch of the palate, where the soft palate and uvula contract the channel for the outlet of the breath, and thus in expiration a portion is squeezed into it with decided force. In tranquil inspiration, the outlet for air through the tube is too small for such a short act to suck off enough from the drum to bring the pressure therein so low as to reduce the membrane.

I will subjoin that in explosive expirations, as in speaking, coughing, hawking, sneezing, the pressure upon the membrana tympani is painful, and in the more violent, particularly involuntary kinds, threatens its integrity.

I will now recite the sonorous phenomena that result from an open Eustachian tube.

At each expiration the breath carries a *souffle* into the drum, clearly caused by the narrowness of the channel by which it enters; and if the membrana tympani had not previously become displaced, this springs outward with a flapping, or popping, or cracking sound, in accordance with the amount of pressure

or percussion that it encounters; also, I believe the sounds or *souffle* made by the splitting of the expiratory current of air at the posterior nares into three currents are heard, especially that of the nearer of the nares.

Every word spoken whilst the tube is open *strikes* the membrana tympani through it, and is thus heard many times louder upon this ear than upon the other. The Eustachian tube is minute compared with the external auditory canal, and can only admit a fraction of the volume of air that the latter holds. And, therefore, I do not mean to affirm that my experience of speaking into my own Eustachian tube is so stunning as what happens when another person speaks into the external one with his lips applied to the auricle; yet the illustration is so correct that, with a due allowance for the difference pointed out, it will convey a fair idea of the nature of the shock.

Inspiratory sounds are also heard through the tube, but then only in a very faint manner. There is a trifling *souffle* made in the tube; and a sound made by drawing air in forcibly through the narrowed fauces, travels into the tube, but is not loudly heard.

Whoever may meditate upon the foregoing sum of evils arising from an open Eustachian tube, will perceive what a happy correlation[1] it is that the glottis and tube should never be open together, so that when the latter is permeable we cannot speak, shout, sing, cough, hawk, sneeze, or whistle; not to say no *souffles* as above described annoy us from mere breathing. As movements are appointed, the most vehement expiratory explosions take place, and thin membranes and tiny bones, with delicate articulations, ligaments, and muscles, carry on their functions, with impunity, and we are not distracted by sounds impinging upon the labyrinth through the tube.

An open Eustachian tube is not attended with deafness. I believe that I enjoy as good hearing as any of my acquaintances, and my right ear is not a whit inferior to my left. Also, it hears equally well whether the tube be open or shut. In the midst of all these phenomena I have enlarged upon, if when the mouth of the tube remains dilated at its utmost I suspend breathing, my right ear performs its proper functions as perfectly as when the drum is shut off from the throat. And if I let breathing recommence, even amidst the consequent faucial *souffles* and tremblings of the membrana tympani, sounds falling upon the body from without, are distinctly heard upon this ear with no other bother than usually attends the disentanglement of sounds when two or more reach the sensorium at the same, or nearly

[1] Could not a careful anatomist detect by what muscular connection between the glottis and tube this correlation is preserved?

the same, instant of time. This fact is a practical refutation of Mr. Brook's theory of a closed tympanum.

As to the observations of Lucæ and Schwartze of oscillatory movements of the membrana tympani with the to-and-fro movements of respiration, they could not, if correct, as the particulars mentioned make manifest, have indicated an opening of the Eustachian tube. It is possible that the mouth of the Eustachian tube, whilst continuing closed, might *pout* under the diminished pressure of inspiration, and sink back in expiration so as to affect the volume of the tympanum. But I suspect that it will be hard for others to verify their observation.

It is now full fourteen years since I have understood the phenomena which an open Eustachian tube entails, and, as physician to two charities, and otherwise, have met with numerous patients who are affected with aural symptoms; yet I have never fallen in with one with a permanently patent Eustachian tube. At this I have been surprised, because the throat is so liable to relaxation, ulceration, and puckering of the lining membrane in healing, that I should have anticipated the not unfrequent occurrence of the accident. In one case, where there had been old syphilitic destruction of the faucial textures in a middle-aged man, there were aggravated troubles from an open tube, which lasted some time; but unhealthy secretion often, it seemed, blocked the tube and suspended the phenomena, and after a while renewed ulcerations in the throat, which he neglected to have prescribed for at once, brought on deafness in that ear. In a few cases, an unhealthy secretion within the drum by seeking vent through the tube, kept it so expanded that the expiratory current of air readily passed into it, and out through the external auditory canal when the membrana tympani has been perforate. But such cases are not allied to that we have been describing.

About a dozen years ago, a young woman, a member of a church choir, with only a very slight appearance of faucial relaxation, and a slightly elongated uvula, with no other sign of ill-health than a slightly anæmic look, narrated symptoms essentially like those that have been before us. I related her case in the second version of my paper. Her attacks were only occasional, though frequent, and not enough so to hinder her from continuing with the choir. Two years afterwards, her aural symptoms remained unchanged. She had herself discovered and practised the mode of reducing her bulging-outwards membrana tympani by trying to breathe with shut nose and mouth. Cases of a transient opening of the tube during breathing, according to my experience, are not very rare, but in anything like the degree of the last-mentioned one they are uncommonly so.

J. Müller[1] speaks of an abnormal state which he could beget in his own ears by "elevating the palate," when a "buzzing sound is produced in the ear, and the sound of one's own voice by this means acquires the intensity of the tones of an organ." From this description one would be led to imagine that he could voluntarily open his Eustachian tubes whilst breathing was proceeding: a feat which I cannot perform, though by some contraction of the palate I can help to keep open the tube when it is already dilated.

I have, too, some recollection of having perused a few printed accounts of ailments in hearing which impressed me with the suspicion that had they been less general or vague, the patient would have appeared to suffer from occasional untoward opening of the tube.

I repeat, then, in concluding the first part of this article, that constant patency of the tube is an almost unknown disease, and that occasional irregular patency is a very rare disease.

In my next part, I shall show that the foregoing observations have decisive import as to certain other views on the physiological acoustics of the tympanum, and connect them with another set of observations.

Art. II.

Ruptures or Lacerations of Internal Viscera. By Francis Ogston, M.D. Aberdeen University.

The cases which follow are offered in continuation of previous articles in this Journal, illustrative of some of the forms of sudden and violent deaths. The instances selected at this time will be limited to examples of the laceration or rupture of the more important viscera of the body, spontaneous or otherwise, commencing with—

1. *Rupture of the heart.*—Of this class of injuries it will be seen that five were of morbid origin, while three of them originated in violence.

(1) *Spontaneous Ruptures of the Heart.*

Case 1.—Mr. M—, æt. 55, an officer on the staff of a militia regiment, notorious as a gross feeder and hard drinker, and known to have suffered from dyspeptic symptoms, while in the water-closet, one morning at 7, was seized with sudden faintness, but was able to stagger back to his bedroom, imme-

[1] 'Physiology,' translated by Baly, p. 1264.

diately adjoining. When seen, shortly after, his pulse was almost imperceptible, his skin cold, his features pinched, and his face deadly pale. In rather more than an hour subsequently to this, reaction had taken place to some extent, and he continued to improve slowly and steadily, though still showing signs of considerable debility, till towards 6 in the evening of the same day, when he suddenly expired.

The inspection of the body, on the third day after death, showed an anæmic condition of the principal viscera, an enormous enlargement and proportionate thickening of the walls of the stomach, and the heart bulky, and loaded with fat. A continuous layer of blood, firmly clotted, a quarter of an inch in thickness, adhered to and covered the whole of the anterior surface of the heart, the removal of which disclosed an irregular vertical rent, half an inch in length, in the wall of the left auricle; a great quantity of dark fluid blood in the anterior mediastinum, and an aneurysm of moderate size in the ascending aorta, whieh had given way at its forepart; the heart's cavities empty.

Case 2.—Elizabeth C— or L—, æt. 48, found dead on the floor of her room, had been seen in life half an hour previously, and been known for some months to have been in delicate health.

At the post-mortem inspection, twenty-three hours after death, the general surface showed marked pallor, with cadaveric lividities on the right sides of the head and trunk. The cavity of the pericardium was filled to distension with blood, mostly in a clotted state, which had escaped through an irregular rent, three quarters of an inch in length, in an oblique direction, through the outer wall of the right auricle. The septum auriculorum was the seat of a similar rent, large enough to admit the passage of the forefinger. Eccentric hypertrophy of the left ventricle, and dilatation, with attenuation of the right cavities of the heart. Aorta and pulmonary artery enlarged, their walls thickened; those of the former vessel studded with patches of atheroma. The remaining viscera of the body healthy.

Case 3.—George T—, a house-carpenter, æt. 35, on the conclusion of a day's work with his employer, while engaged at home with a private job, and using an augur, became suddenly sick, and dropped down dead. He had made no previous complaint of illness at any time.

Post-mortem, thirty-six hours after death.—Surface generally pale and sallow, with moderate lividities of the dependent parts

of the body. Ten ounces, by measure, of loosely coagulated blood, and two ounces of bloody serum, in the cavity of the pericardium. An irregular rent in a vertical direction through the wall of the right cardiac auricle at the point of junction of the sinus auriculorum with the proper auricle, and a similar rent at the corresponding part of the left auricle. These rents admitted the passage through them of a round rod, five eighths of an inch in diameter. The heart's cavities on both sides almost empty of blood: neither the heart nor the remaining viscera of the body showed any traces of previous disease.

Case 4.—Alexander F—, æt. 35, a prisoner in solitary confinement, found dead in his bed one morning, on the sixth day of his imprisonment, previous to which he had been examined medically by the prison surgeon, and by him pronounced to be healthy.

Inspection, twenty-eight hours after death.—The right side of the chest, the outsides of the right thigh and leg, the ears, and the nails, slightly livid; the rest of the surface pale and bloodless. Six fluid ounces of reddish serum and a continuous clot of blood, weighing eleven ounces, in the cavity of the pericardium; an irregular oval opening partly in the wall of the left cardiac ventricle, partly in that of the intra-pericardial portion of the aorta. Heart pale, flabby, and empty, its tricuspid valve looped, but the organ otherwise natural. An aneurysmal tumour, three inches in exterior diameter in the ascending aorta, both above and below which the artery was dilated and its walls thickened: marked firmness of the brain: no other indication of disease on the body.

Case 5.—Isabella A—, æt. 83, a solitary pauper, found dead on the floor of her room; last seen in life two days previously.

Inspection on the day following the finding of the dead body.—Pallor of the lips and front of the body; points of the fingers and the dependent parts of the head and trunk livid. Ten measured ounces of blood, partly fluid, but mostly wholly clotted, in the pericardial cavity. An irregular rent, large enough to allow the passage of the forefinger, in the anterior wall of the right auricle. Fatty degeneration of the heart; its mitral and aortic valves ossified; aorta enlarged, with bony plates at its arch; osseous tumours in the uterine wall; kidneys waxy; brain and its coverings unusually pale; superabundance of cerebro-spinal fluid within the head.

(2) *Ruptures of the Heart from External Violence.*

Case 6.—Ann S—, æt. 4. Rode over by a cab, two of the wheels of which had passed over her body. After this, was seen in life by a medical gentleman in the vicinity, but expired in about twenty minutes.

Inspection twenty-four hours after death.—Dependent parts of the back and forearms bluish; the rest of the surface pale, with the exception of parchment-like patches of the skin on the left side of the forehead, the left cheek, and the front and outside of the left knee; minute abrasions of the left nates, right knee, and calf of the right leg. A linear fracture of the os frontis beginning at the discoloured patch on the forehead, and extending along the orbital plate of the bone for an inch and a quarter. The scalp and the contents of the cranium containing less blood than usual, a thick layer of clotted blood under the left lobe of the thymus gland and over the outer surface of the pericardium. A large quantity of partly clotted, partly fluid blood, in the cavity of the pericardium. An irregular rent, an inch and a half in length, and in an oblique direction, in the outer wall of the right auricle, extending into the root of the superior vena cava. Fifteen ounces of blood, partly clotted, but chiefly fluid, in the right cavity of the chest, compressing the right lung to a considerable extent. The interior of the pericardium communicating with the right cavity of the chest by a regularly circular opening half an inch in diameter, in the pericardial wall, at its right side, a little above its point of connection with the diaphragm. Pale and bloodless condition of the contents of the abdomen.

Case 7.—Elizabeth S—, æt. 64. Thrown down by a horse and cart coming rapidly down a steep incline. Died a few minutes after.

Post-mortem on the following day.—Pallor of the front of the body, inclusive of the lips and nails. Lividity of the dependent parts of the trunk. No trace of injury on the exterior of the body. The third, fourth, fifth, and sixth left ribs fractured, at points from half an inch to two and a half inches from their costal ends. Ten ounces of blood (six clotted, four fluid) within the pericardium. An irregular rent in the anterior wall of the right ventricle, near the apex, close to the septum of the heart, large enough to allow the ready escape of water poured into the cavity. Clotted blood infiltrated into the wall of the ventricle, in the immediate vicinity of the rupture; atheromatous patches in the mitral valve and ascending aorta. Right cavities of the

heart, and both venæ cavæ, full of dark fluid blood; left cavities of the heart empty. Liver cirrhosed; remaining viscera of the body natural.

Case 8.—J. C—, æt. 26. Thrown down and ridden over by a loaded railway truck. Instant death.

Post-mortem twenty-seven hours after.—Lividity of the dependent parts of the trunk. Blood at the lips and nostrils. Dry, brown, and horny state of patches of the skin on the outsides of the left arm and forearm, left shoulder, and both sides of the chest. A compound fracture of the right humerus at its upper third. Brain and its coverings natural. An irregular transverse fracture of the sternum, two inches below its upper border. The whole of the right ribs fractured, the first nine, both near their costal ends and in the vicinity of their angles. The fifth to the eighth left ribs also fractured near their anterior extremities. The œsophagus, at the lower part of the neck, irrregularly torn across. Both bronchi irregularly divided close to their separation from the trachea. Lungs, in twelve pieces, of various sizes, lying loose in the cavity of the chest. The aorta, pulmonary artery, and inferior vena cava, completely and very irregularly divided, and the heart only connected with the superior vena cava by a fragment of its wall. The heart itself has been torn across through the walls of both auricles, which were entirely separated from the ventricles. The pleura opposite the fractures of the ribs unbroken; sixteen ounces of fluid blood in the cavity of the chest. The fifth, sixth, seventh, and eighth dorsal vertebræ detached from the spinal column and pushed forwards into the chest; six ounces of fluid blood in the cavity of the abdomen; deep rents penetrating to its centre, both transversely and from before backwards, in the right lobe of the liver. A deep rent in the lower border of the spleen.

2. *Ruptures, or Lacerations of the Lungs.*

Of the extent and unusual character of injuries of this sort from extreme violence, the last case (Case 8) presents an instructive instance. The more usual circumstances under which these injuries are liable to occur are illustrated by the cases which follow.

Case 9.—James B—, æt. 59. Thrown from the top of a cart piled high with household furniture. The wheel of the cart had passed over the chest. Survived till next day. Front of the chest emphysematous, and crossed by a broad band of parchment-like integument. Coagulated blood effused into the subcutaneous areolar tissue over the left ribs. Double fractures of

the left ribs, from the fourth to the eighth inclusive, near their anterior third, the posterior fractured ends of the ribs protruding through the pleura. Ten fluid ounces of blood, mostly clotted, in the left pleural cavity. Left lung softened and infiltrated with blood, with a deep rent at the part corresponding with the projecting ribs. Right lung and abdominal viscera pale and bloodless.

Case 10.—Forbes J—, æt. 41; crushed betwixt the wheels of two carts. Death in a few minutes (eight or ten). Abrasions of the skin over the nose and sternum; emphysema of the chest and left side; layer of clotted blood under the skin over the lower and fore parts of the neck and upper and fore parts of the chest; sternal end of the right clavicle dislocated; surfaces of both lungs presenting a raw and bloody appearance, with a thin layer of blood under the pleura at the anterior margins of their lower lobes; irregular rent, two inches in length and about the same depth, across the upper lobe of the left lung; superficial rent on the back part of the lower lobe of the right lung; thirty-seven ounces of fluid blood in the left pleural cavity; fractures of the left ribs from the third to the seventh inclusive, at from one and a half to three inches from their cartilages, and of the third and fourth five inches from the same part, the broken ends of the latter protruding through the pleura; reddish froth in the larynx and trachea; bloodless state of the brain, liver, spleen, kidneys, alimentary canal, and left lung; right lung congested; heart's cavities empty.

Case 11.—Mary Ann D— or M—, æt. 38, thrown down by a stage coach, its wheels passing over the chest. Death in a few minutes. Front of body pale; its dependent parts reddish; parchment-like patches on the left cheek and chin; abrasions of the forehead, nose, and over the centre of the left clavicle; lacerated wound on the occiput; contusion of right mamma; emphysema of the left side of the neck; fractures of the left ribs, from the first to the eighth inclusive, at half an inch from their sternal ends; of the eighth left rib, two and a half inches from its dorsal ends; fractures of the right ribs, from the third to the tenth inclusive, at points from three to five inches from their cartilages, and of the ribs on the same side from the second to the tenth inclusive at their necks; rents in the pleura, opposite the fracture of the sixth left rib; two rents in the right lung at its root; dislocation of the vertebral extremity of the seventh right rib; oblique fractures of the laminæ of the sixth and seventh dorsal vertebræ; heart's cavities empty; raw and bruised appearance of the left lung, opposite the fractures

of the left ribs; eight ounces of fluid blood in the right, and six ounces of blood in the same state in the left pleural cavities; brain and lungs bloodless; abdominal viscera natural.

Case 12.—James G—, æt. 61. Fall from the roof of a house. Death immediate. Surface generally pale; lacerated wound on the left side of the head; contusion of the left shoulder; vertical linear fracture through the left parietal and temporal bones; fracture of the lower third of the left humerus; a thick continuous clot of blood over the front of the neck and the upper and fore part of the chest, extending into the left axilla; transverse fracture of the middle of the sternum; fracture at the outer third of the left clavicle; comminuted fractures of the sternal ends of the left ribs from the first to the fourth inclusive, and single fractures of the middle of the ribs on the same side, from the fifth to the tenth inclusive; third right rib fractured at two points, and second and fourth near their cartilages; irregular rent, three quarters of an inch in length, in the upper lobe of the left lung; heart empty; fluid blood in the upper part of the spinal canal; brain and abdominal viscera pale and bloodless.

Case 13.—William A—, æt. 10. Thrown down by a cart, the wheels of which had passed over the body. Death immediate. Blood at the mouth and nostrils; front of body pale; dependent parts livid; pupils dilated; a brownish patch on the left temple; mouth, throat, and trachea, full of fluid blood; fifth and seventh left ribs fractured, the former three inches from its spinal extremity, the latter close to its cartilage; six ounces of fluid blood in the right, and five ounces of partly clotted but chiefly fluid blood in the left pleural cavity; heart's cavities empty and perfectly clean; lower lobe of the right lung torn both vertically and transversely almost through its whole thickness; a rent an inch both in length and depth in the upper lobe of the left lung; brain, liver, spleen, kidneys, and alimentary unusually pale and bloodless.

Case 14.—Jean McK—, or F—, æt. 62. Crushed between two railway waggons. Death instantaneous. Front of the body pale; dependent parts livid; left eyelids blue; oblique fracture of the outer end of the right clavicle; right ribs from the first to the seventh inclusive fractured at from one and a half to two inches from their cartilages; the left ribs, from the first to the sixth inclusive, fractured near their middles, and also at their angles, the broken ends of these last projecting through the torn pleura; sixteen ounces of blood, partly clotted but chiefly fluid in the left, and eight ounces of like blood in the right

pleural cavities; upper lobe of the left lung extensively torn to a considerable depth; viscera of the head and belly generally bloodless.

Case 15.—William S—, æt. 64; thrown from a gig drawn by a runaway horse. Survived till the following day. Front of the body pale; dependent parts livid; left lower eyelid blue; two lacerated wounds of the right temple, with subcutaneous effusion of clotted blood in their vicinity; right ribs, from the second to the eighth inclusive, fractured at from two to two and a half inches from their cartilages, and the fifth and sixth again close to their angles; eight ounces of fluid blood in the cavity of the chest; right lung collapsed and torn, the seat of the rent not recorded; clotted blood in the right cavities of the heart, and in the left cardiac auricle.

3. *Ruptures of the liver.*—Of this, in addition to Case 8, already given in a different connection, the following instances may be adduced:

Case 16.—Christopher B—, æt. 36, while escaping from the pursuit of one police constable was met by another; when slipping his foot he fell on his back; on this the second constable, with his whole weight and momentum previously acquired by running, came down on B—'s belly with his bent knee. When seen by an assistant twelve hours after this he merely complained of pain in the belly, for which warm applications were prescribed, and this in ignorance of his previous history, which only came out after his death. On his visit to B—, an hour and a quarter after that of his assistant, the writer found him sullen and indisposed to speak, only stating, in answer to questions, that he had had some whisky, and was pained at the navel, but less so than before the warm applications. Pulse 84, full and soft. On returning in little more than an hour subsequently to this visit, it was found that an opiate draught which had been administered had sickened him. The pain of the belly had again increased; the pulse, which was irregular, varied in frequency, not rising above 90, while it was small and hard; the muscles of the belly were rigid; the face flushed and anxious, and he resisted all attempts to move him from his prone position. On this B— was removed to the Aberdeen Royal Infirmary, where he survived till the third day, sixty-two hours after the first professional visit; the treatment had recourse to while in the infirmary having been the application of leeches to the belly and repeated doses of castor oil.

At the inspection of the body, the face and general surface

was pale, the dependent parts of the trunk and extremities livid; ten pounds of thin, chiefly fluid blood, was found effused into the pelvic cavity and dependent parts of the abdomen; the gall-bladder, detached from the gall-duct at its root, lay loosely in front of the smaller intestines near the umbilicus, full of bile. A portion of the liver remained attached to the upper surface of the gall-bladder, to which it adhered with some tenacity. This portion of the liver, which corresponded with a deficiency of but slight depth in the cystic hollow, was of an irregular four-sided figure, one inch in breadth each way. The surface of the liver at the torn part was irregular and infiltrated with blood to some depth. The smaller intestines, generally, injected and somewhat softened. Blood, partly clotted, in about equal quantities, on both sides of the heart. Lungs bloodless, with abundant frothy mucus in their air-cells. Contents of the cranium natural.

Case 17.—Joan McA—, æt. 2½ years; found dead; had been previously beaten with sticks and jolted roughly on a donkey by two gipsy boys.

Inspection.—General surface pale; numerous contusions and abrasions on the face, trunk, and limbs; scalp bloodless; small clots of blood in the upper surfaces of the anterior lobes of both cerebral hemispheres. Heart's cavities full of clotted blood; clots of blood in the meshes of the omentum, and on the surface of the transverse colon; blood, partly clotted, in quantity, in the cavities of the abdomen and pelvis; clotted blood in the areolar tissue around the right kidney; one rent at the lower surface of the right lobe of the liver, and two other rents near the junction of its right and left lobes; all directed from right to left; the first an inch and a quarter in length, the others of smaller sizes; brain, lungs, and abdominal viscera bloodless.

Case 18.—Alexander W—, æt. 60. Ridden over by a cab. Survived for half an hour.

Inspection.—General surface pale; slight lividities of left side of forehead, left shoulder, and dependent parts of trunk; lacerated wound of right temple; oblique fracture of upper part of sternum; double fractures of the sternal ends of the left ribs, from the second to the fourth inclusive; fifth left rib also fractured close to its costæ; clotted blood around the fractured ends of the ribs and in some quantity in the anterior mediastinum. Heart pale, flaccid, and containing a little fluid blood, about equally distributed in its cavities on each side. Thirty-three and thirty ounces of fluid blood in the cavities respectively of the chest and abdomen; antero-posterior laceration

through almost the whole depth of the right lobe of the liver; less blood than usual within the head.

Case 19.—Ann T—, or S—, æt. 38. Instantaneous death in a railway collision.

Inspection.—General surface pale, abrasion of left shin; fracture of lower end of left fibula; lacerated wound of the abdominal wall, extending from a point a little above the middle of the crest of the right ilium to one a little above the symphysis pubis; the pyloric end of the stomach detached from the rest of the organ; the whole of the smaller and great part of the larger intestines, the right kidney, isolated portions of the pancreas, and about one third of the liver at its central part, with the gall-bladder attached, all protruded through the abdominal wound and outside the cavity of the belly; the first and second lumbar vertebræ detached from the spine and thrown forwards, and the spinal cord torn across at the part; little or no effusion of blood into the cavity of the belly; examination of the cavities of the head and chest not permitted.

Case 20.—Gordon M—, æt. 28. Instantaneous death from the fall of a railway arch. General pallor of the surface; lividity of the shoulders and upper and fore part of the chest; slight abrasions over the upper part of the sternum, the left wrist, and right shin; comminuted fracture of the bones of the right leg, close to the knee; compound fracture of the bones of the right forearm, near the wrist; lacerated wound with retraction of its edges to the extent of one and a half inches across the forehead from temple to temple; a similar wound with retracted edges, one inch apart from the point of the nose to the right side of the forehead immediately above the inner end of the right eyebrow; two small lacerated wounds on the crown of the head; marked absence of blood in and around the whole of these wounds; comminuted fractures of the nasal, lachrymal, ethmoïd, and upper and lower maxillary bones; upper part of the head unnaturally flattened from depression of the skull; a linear fracture encircling the head, except for two inches, at the middle of the right temple, nearly in the line of the usual serrature, in the removal of the skull-cap. From this fracture four nearly vertical linear fractures ran down to the base of the skull; one through each temporal bone, a third through the occipital bone towards its left side, and the fourth through the left side of the sphenoïd bone, comminuting the ala major of this bone. Beneath the circular fracture on both sides of the head the dura mater and cerebral hemispheres lacerated, the latter to some depth. Separation of the two lateral halves of the frontal portion of the

frontal bone in the line of the frontal suture and of the frontal and parietal bones at the coronal suture; marked absence of blood in the scalp, in the vicinity of the fractures of the bones of the head and face, and in the contents of the cranium; point of the tongue protruded and fixed between the teeth; a little bloody mucous froth in the trachea; a sparing quantity of thin fluid blood in each of the auricles of the heart. Ventricles of the heart empty of blood; the interior of the right ventricle so clean that it did not stain a towel applied to it. Both lungs adherent to the chest; left lung unusually bloodless; right lung much congested; the third to the sixth right ribs inclusive fractured at from four to six inches from their sternal extremities; a deep rent at the outer and back part of the right lobe of the liver; kidneys waxy; albumen in the urine; vertical fractures through the middle of the right iliac bone and the right horizontal ramus of the pubis; a transverse fracture through the last lumbar vertebra; marked absence of blood in the vicinity of these fractures.

Case 21.—Jessie W—, æt. 20, crushed betwixt the buffers of two railway waggons; death instantaneous.

Inspection.—Slight lividities of the dependent parts of the head and trunk; a blush of redness on the front and left side of the chest, rest of the surface pale; fifth and sixth right ribs fractured close to their centre; cartilage of the seventh right rib divided across close to the rib. Frothy mucus in the trachea; blood effused in patches under the pleura at the base of the right lung, of the breadth of half an inch; minute clots of effused blood on the surface of each lung; twenty-eight ounces of fluid blood and some loose clots in the abdominal cavity. Left lobe of the liver almost divided through from front to back; a second but superficial rent in the same direction, on its upper surface; right lobe of the liver near its middle torn nearly wholly across from before backwards; a second rent of some depth at the back part of the same lobe; a superficial rent along the inner part of the spleen for nearly its whole length; clotted blood effused in some quantity into the areolar tissue surrounding both kidneys; a rent on the inner border, and a numerous series of short and superficial rents transversely across the posterior surface of the left kidney; less blood than usual within the head; intestinal canal bloodless.

Case 22.—Arthur W—, æt. 50, ridden over by a cart; death in less than an hour. Small parchment-like patches on the nose, right side of the forehead, right cheek, and outer third of right clavicle; lividity of the dependent parts of the trunk; rest of the surface pale; oblique fracture at the outer third of

the right clavicle, the parts around slightly bloody; lungs bloodless; fluid blood in small quantities in the cavities of the heart; thirty-five ounces of fluid blood in the abdomen; several rents from the front to the back of both the large lobes of the liver, penetrating almost its entire depth; two longitudinal rents, each two inches in length, on the convex surface of the spleen; several rents, chiefly transverse, on the anterior and posterior surfaces of the right kidney; osseous plates on the ascending aorta; aortic valves thickened and cartilaginous.

Case 23.—John K—, æt. 3 months. Believed to have been subjected to bad usage; found dead in bed. Redness of the lips and dependent parts of the head and trunk, rest of the surface markedly pale; scalp bloodless; frothy fluid, in some quantity, in the lower part of the trachea; blood in the heart, partly clotted partly fluid, but in greater quantity in its right than in its left cavities. The fourth, fifth, and sixth right, and the second to the seventh left ribs inclusive, fractured obliquely at points from four to five eighths of an inch from their cartilages, their fractured ends surrounded with callus in the state of cartilage; three irregular rents in the lower surface of the liver, one dividing the lobulus caudatus, one dividing the pons hepatis, and the third passing through the lobulus quadratus; a cavity in the centre of the right lobe of the liver, of irregular shape and containing fluid blood; ten drachms of fluid blood in the abdominal cavity; stomach and intestines pale and bloodless.

4. *Ruptures of the spleen.*—In addition to the three instances previously adduced (Cases 8, 21, and 22), the three following presented themselves:

Case 24.—Christian R—, æt. 61, had either thrown herself or been precipitated by others, from an attic window in a house of two floors. Found dead on the ground beneath, some hours after.

Inspection.—Slight wound of right hand; eyelids discoloured (blue); lacerated wound on right temple; comminuted fracture of the squamous plate of the right temporal bone, from which fissures proceeded, on the same side of the cranium, through the petrous portion of the bone, through the frontal and orbital portions of the frontal bone, through the large wing of the sphenoïd bone as far as the sella turcica, and downwards and backwards through the occipital bone to the foramen magnum; emphysema of the right side of the chest; a thick layer of clotted blood over the entire surfaces of the left cerebral and cerebellar hemispheres; fluid blood on both faces of the falx cerebri;

fractures of the right ribs from the second to the twelfth inclusive, the second and third at two inches from their cartilages, the first, second, and third again at their heads, and the fourth to the twelfth inclusive from two to two and a half inches from their cartilages; ruptures of the pleura costalis opposite the fractures of the third, fourth, and sixth ribs; dislocation of the scapular end of the right clavicle; three ounces of fluid blood in the right pleural cavity; bloody, frothy mucus in the trachea and lower lobe of the right lung; an irregular rent nearly its entire length and depth in the spleen; an ounce of fluid blood in the abdominal cavity; heart pale, and its cavities empty; coagulated blood effused into the areolar tissue around the bladder; comminuted fractures of both ossa innominata; clotted blood effused amongst the muscles of the right side and back part of the chest and of the loins and pelvis.

Case 25.—Ann D— or S—, æt. 55, rode over by a carriage. Instant death.

Inspection.—General surface pale; moderate lividity of the dependent parts of the body; slight abrasions on both cheeks, the right elbow, the left ear, and the inside of the left leg. Contusion with clotted blood on the right temple. Thin layers of clotted blood over the greater part of the right and the lower surface of the left cerebellar hemispheres. Brain pale; skull uninjured. The second to the eighth ribs inclusive fractured about two inches from their cartilages; a small rent in the pleura, opposite the fracture of the fifth rib. Heart's cavities on both sides almost empty. Osseous plates in the pericardium and aortic arch; close adhesion of the pericardium to the surface of the heart. Twenty-six ounces of dark fluid blood in the abdominal cavity; spleen extensively and deeply lacerated, and a detached portion of it lying loose in the abdomen; marked bloodlessness of the liver.

Case 26.—Sophia W—, æt. 4 months, rode over by a cart. Death instantaneous.

Inspection.—Slight lividities on the back; with the exception of slight blueness of the inside of the right thigh, the rest of the interior surface pale. Comminuted fracture of the middle of the right humerus. A thick layer of clotted blood under the scalp over the right side of the head, and occipital fracture with depression of the right, and linear fracture of the left parietal bones, the brain under the former broken up into a pulp and infiltrated with blood; from the back part of this fracture a fissure extended through the occipital bone to the foramen magnum; a little fluid blood in the trachea; right clavicle fractured; as also the third cervical vertebra through its body, and

the first eight right ribs close to the spine. Soft parts in the neighbourhood of these fractures merely dyed with blood; a transverse superficial rent, half an inch in length, across the middle of the outer surface of the spleen, and a second rent of the entire length at its anterior margin. Fluid blood in about equal quantities in the cavities on both sides of the heart; raw and bruised appearance of the root of the right lung; fractured ends of the ribs protruding through the pleura.

5. *Of ruptures of the gall-bladder*, or its duct, we had illustrations in Cases 16 and 19.

6. *Of ruptures of the kidneys* in Cases, 19, 21, and 22; and of

7. *Rupture of the pancreas* in Case 19.

8. *Ruptures of the stomach.*—Of this we had an example in Case 19. A further instance, and this of spontaneous occurrence, presented itself in private practice several years ago. But as the notes taken at the time have, unfortunately been lost, and only the inspection of the abdominal cavity was permitted, its value on both these accounts is excessively lessened. Such as it is, however, it is here offered from memory.

Case 27.—Miss E—, æt. 18, one of a numerous family of strumous children, two of whom had previously died of consumption. While confined to bed by some obscure head symptoms, of several days' duration, was suddenly seized with excessive retchings and vomiting, when she fell back in a faint, and died in a few seconds. At the inspection, the wall of the stomach at the upper part of the great cul-de-sac was found to have given way in the form of a triangular flap, the sides of which did not exceed an inch and a quarter in length. Though the torn edges of the flap were ragged, the coats of the organ presented otherwise a healthy appearance.

9. *Of rupture of the intestines* in the healthy state of the tube, we have only encountered the following instance, and this a post-mortem production.

Case 28.—Joseph C—, æt. 36, a police constable, on duty at the Aberdeen harbour during a tempestuous night, was supposed to have been blown into and perished in the water. It was not till late on the following day that the body was found. In the interim, the harbour being then a tidal one, the water had risen and fallen, and a ship had left the berth near which the corpse was discovered, giving reason to believe that it had either been crushed betwixt the ship and the quay wall, or below the ship's keel. In addition to the usual indications of death by drowning, the inspection disclosed contusions on the occiput, the left side of the chest and the inside of the left leg, frac-

tures of the zygoma and of the bones of the right forearm, a dislocation of the scapula and of the right clavicle, and a lacerated wound of four inches in length, through the abdominal wall a little above the right groin. Through this wound the greater part of the jejunum and ileum had protruded, torn in several places, with escape of feculent matter in some quantity. There was fluid blood in the abdominal cavity, and clotted blood in the right ventricle of the heart.

10. *Rupture of the urinary bladder.*—The only case known to us of this occurrence, and that but partially, was that of an adult male, whose death led to a trial for homicide at one of our criminal courts about thirty years ago.

Case 29.—While engaged in a scuffle in a harvest field, this person received a kick on the hypogastrium with the toe of a woman's shoe, the sole of which was heavily loaded with nails. He survived till the third day.

The inspection disclosed, with entire absence of marks of external injury, the bladder ruptured; the rent concealed behind a thick coating of lymph spread over the lower part of the abdomen.

11. *Ruptures or Lacerations of the Brain and its Membranes.*

Injuries of this class are found to bear a close family resemblance, as in Cases 20 and 25, already adduced in another connection. The brain and its membranes, in their ordinary condition, are only known to give way on direct pressure exerted on them by fragments of the comminuted skull, or depressed portions of the edges of linear fractures or fissures, as shown by the limitation of such injuries to the parts of the soft organs which have suffered such compression. It would be useless, therefore, to multiply examples of this sort; two or three additional cases, however, may be subjoined, as being to some extent of a rare and exceptional character. Thus Case 30 exhibits the possibility of a rupture of the cerebral mass at one or more points away from the seat of the cranial fracture. In Case 31, again, the breach in the skull wall corresponded pretty closely with the size and shape of the instrument (an axe) which had produced it. The remaining instance (Case 32) shows the extent of injury which may occasionally follow a fall from a very moderate height.

Case 30.—Robert C—, æt. 25, found dead at the foot of the retaining wall of a railway viaduct. Seen shortly before in drink.

Inspection.—Surface generally pale; a slight lividity on the back part of the trunk; blood at the nostrils, mouth, and ears; second right incisor tooth fractured; a comminuted fracture of the squamous plate of the left temporal bone, with fissures running from it into the left parietal bone, left side of the frontal bone, and along its orbital plate, and downwards and forwards through the sphenoïd bone, as far as the sella turcica; a layer of clotted blood over the upper surface of the middle and posterior lobes of the left cerebral hemisphere; a slight rent at the lower surface of the middle lobe of the right cerebral hemisphere, and a superficial rent, an inch in length, on the lower surface of the left lobe of the cerebellum, covered with a layer of clotted blood; the anterior surface of the upper lobe of the right lung stained with purplish patches, giving it a raw and bruised appearance; lungs congested; cavities of the heart, on both sides, empty; brain, in its interior, and the abdominal viscera generally, markedly pale.

Case 31—that of Ann Milne, or Forbes, or Joss—has already been published at length, from our joint notes, in the number of the 'Edinburgh Medical Journal' for July, 1865, by Dr. Silver, late Assistant-Professor of Medical Jurisprudence, Aberdeen University, and is only here referred to for the reason stated above, viz., the unusual circumstance of the breach on the back part of the skull corresponding so closely with the back of the axe with which the murderous blow had been inflicted. For details of this instructive case, reference must be made to the article in the journal.

Case 32.—Alexander or Shaw S—, æt. 42, a strongly built, muscular man, was found on a stone floor in a barn close to the foot of a loft which he had been seen to climb up to by a ladder while in drink, an hour before. When discovered on the floor he was quite insensible, and blood was flowing from a wound on his head. He died a few minutes after. The height of the loft was eight feet above the floor of the barn.

Inspection.—Pupils broadly dilated; front of the body, including the lips and nails, pale; dependent parts of the trunk and limbs livid; scalp wound, four inches in length, at the upper and back part of the right temple; a continuous clot of blood, a quarter of an inch in thickness, under the scalp, over the upper part of the forehead and the right side of the head; a linear fracture of the cranium, extending from a point in front of the right temporal ridge of the frontal bone to the middle of the vertex, then taking a bend, and passing downwards to the right parietal protuberance, and sending off two branches, one

to the occipital protuberance, the other to the base of the skull, ending in the foramen lacerum anterius, in baso cranii; at its commencement the anterior edge of the fracture depressed, and at the forking at the parietal protuberance the skull was comminuted and displaced; a second and separate fissure, or linear fracture, commenced at the back part of the vertex, and passed forwards to the base of the skull on the same side; a continuous clot of blood, covering the whole of the left cerebral hemisphere, thickest at the base of the brain; a similar clot over the right hemisphere, except at its upper and fore-part; coagulated blood, in some quantity, betwixt the hemispheres in both faces of the falx major; a rent of the brain, two and a half inches in length, and a quarter of an inch in depth at the commencement of the first fracture; brain unusually pale and bloodless. Its lateral sinuses full of effused blood, as was the upper third of the spinal canal; a layer of clotted blood over the cartilages of the second and third left ribs and upper half of the sternum; irregular transverse fracture of the sternum, a little above its middle, with clotted blood betwixt its broken ends; fracture of the second right rib, two and a half inches from its sternal extremity; cavities on both sides of the heart and the large vessels connected with these entirely empty of blood; hypertrophy of the left ventricle of the heart; patency of the aortic and pulmonic arterial openings; partial fatty degeneration of the walls of the heart; both lungs highly emphysematous, and attached to the chest by lengthened adhesions; deficiency of blood in the lungs, liver, and kidneys; spleen atrophied; kidneys granular; intestinal tract unusually pale and bloodless.

Remarks.—A few of the more striking points in the cases adduced claim our attention.

1. As was to be anticipated, disease of the heart or its vessels was met with in five of the eight cases of ruptures of the organ, viz., in four of the instances of *spontaneous* rupture, and in one of those *from violence*. Thus in Cases 1, 2, 4, 5, and 7, single examples of the following morbid conditions were encountered: atrophy (general), general attenuation of the right heart, hypertrophy (general), hypertrophy of the left ventricle, fatty degeneration, aneurysm of the aorta, dilatation of the aorta, atheroma of the aorta, ossification of the valves of the aorta, dilatation of the pulmonary artery, ossification of the mitral valve, atheroma of the mitral valve, adhesion of the tricuspid valve.

In Cases 3, 6, and 8, the heart and its vessels were in a healthy state, the first of these an instance of spontaneous rupture. In this last the first impression certainly was, that the heart was of morbid size; but when it was compared with that

of the man's body (of very bulky frame) this idea had to be abandoned.

In the cases of *spontaneous* ruptures the walls of the right, as may have been looked for, had suffered in the proportion, as compared with the left heart, of four to two; and the auricles on either side as compared with the ventricles in the proportion of four to one. The rents took a vertical direction, whether in the auricles or ventricles, in two of the cases; an oblique direction in two; and an irregular course in two. Besides involving both sides of the heart in one case, in another instance the laceration had extended from the parietes of the heart to the septum ventriculorum, and in a third, from the auricle to the root of the aorta.

In one of the instances of ruptures of the heart *from violence*, the rent was in the right ventricle. In another, it was in the right auricle, passing into the superior vena cava. In Case 8, the rent it will be seen was so entire, as to have torn away the auricles from the ventricles of the heart.

If we assume from the firm coagulation of the blood in the pericardium, and its fluidity in the anterior mediastinum, that the rent of the heart in Case 1 had preceded the rupture of the aortic aneurysm, we have in this instance the striking peculiarity of the survival of the patient for eleven hours after the injury of the heart.

2. Experience has shown that *the lungs*, from their looseness of texture and natural elasticity, are found to escape laceration from almost any amount of mere concussion, though Case 8 shows a remarkable exception to this. Such an injury is, however, by no means rare, as the result of such compression of the chest as is sufficient to fracture the ribs and force their broken ends against these viscera, whether accompanied by laceration of the costal pleura, as in Cases 9, 10, 12, 14, and 15, or without such complication, as in Cases 8, 11, and 13.

Whether from the character of the violence, or what is more likely, from their lesser amount of strength, the fractures of the left ribs, in this class of cases, predominated over those of right ribs, in the ratio of 56 to 49. In 105 separate fractures, the relative numbers of these respectively, at their sternal extremities, near their angles, and at or near their middles, were as 74, 24, and 7.

3. In Case 16, it is not easy to understand why it was that the shock should have spent itself almost entirely on the gall-duct, as neither the duct itself, the gall-bladder, or the liver presented any obvious trace of morbid structure, unless we consider as such the somewhat tenacious adhesion of the apposed surfaces of the liver and gall-bladder. The occurrence led to

the trial of the assailant for homicide, at the Circuit Court of Justiciary at Aberdeen, in the spring of 1839.

The seats of the several ruptures of the liver, 26 in all, in the nine cases of this injury, were as under, viz.:—in the right lobe in 11; in the left lobe in 6; in the central parts in 7; in the lobulus caudatus in 1; in the lobulus quadratus in 1. Of these, nine took the antero-posterior direction, 2 the transverse, the others being irregular.

4. In the ordinary run of cases of fatal syncope, as in copious hæmorrhages, for instance, the examination of the heart usually shows the distribution of the blood about equally betwixt the cavities on both sides of the heart. In not a few of the above cases, however, though the immediate cause of death must be referred to the same category, a different state of matters was found to present itself. Thus, leaving out of view, for obvious reasons, the instances of ruptures of the heart, we have the cardiac cavities entirely empty in Cases 10, 11, 13, 25, and 31; and almost empty in Cases 18, 20, and 22. The first of these sets of cases were examples of immediate deaths, the other of either immediate or speedy deaths. All of them were of course the results of severe shocks to the system, the only circumstances under which we have encountered this rare form of death by syncope. That the shock played the most important part as the cause of death in these cases, is evident from the comparatively trifling losses of blood in all.

Besides this emptiness of the cavities in both sides of the heart, a still rarer phenomenon was observed in two of the cases. In Case 20, it is narrated that the interior of the right ventricle was "so clean that it did not stain a towel" used to wipe it. The same circumstance was observed in Case 13, as regards the whole of the cavities of the heart.

5. The effect of severe shocks to the system in dividing, detaching, and displacing some of the more important viscera and closely and firmly connected bones, was remarkably exhibited in several of the cases recorded. Thus we have division of the œsophagus, the bronchi, aorta, pulmonary artery, venæ cavæ, and of the spinal cord, in Case 8; and in the same case, the detachment and division of the lungs, and displacement of four of the lumbar vertebræ. Again, in Cases 16 and 19, we encounter detachment of the gall-bladder; and in the latter of these, detachment of a kidney, parts of the pancreas, liver, and stomach, with displacement of two of the lumbar vertebræ and division of the spinal cord.

Art. III.

On Lithotomy. By W. F. Teevan, F.R.C.S., B.A., Surgeon to the West London Hospital, &c.

It would be difficult to select from the entire domain of surgery any operation that has attracted more attention than lithotomy; volumes have been written about it and still more said regarding it. A few years ago there was no operation whose rules were so dogmatically laid down or so universally accepted as those of lithotomy. The competition, however, to which lithotomy has been subjected by its rival operation lithotrity, has been the means of awakening increased attention to lithotomy, to see if that procedure could not be in some way made more successful than it has hitherto been.

At the present time the various modes of performing lithotomy are subjects of controversy, and it is my object to bring forward certain facts which I hope may be the means of narrowing the questions at issue.

I would, firstly, make some remarks regarding certain points in connection with lateral lithotomy.

1. Regarding the particular kind of sound to be used. There are two kinds of sounds in use—those which are similar in shape to a catheter, and those which resemble a lithotrite. I have no hesitation in saying that the latter ought always to be employed. On several occasions I made lateral sections of the adult male pelvis, and compared the merits of the different shaped sounds in order to ascertain which were best adapted for the particular end in view. I found when I used the catheter-shaped sound that a stone would often elude discovery, even after a prolonged examination, and I ascertained the cause of failure to arise from the beak being too long and curved—its point becoming entangled in the folds or fasciculi of the bladder, and hence, when the sound was moved to the right or left it did not roll to either side of the bladder, but simply carried the bladder to the right or left of the pelvis. It is for this reason that it is often so difficult to detect a stone with a staff. Surgeons often experience considerable delay when trying to hit the stone with a staff, but I have generally found that by introducing the finger into the rectum and tilting up the bladder quickly the stone will be jerked against the staff and the wished-for click elicited. If a short-beaked sound be used, its end is so very free that it insinuates itself into the vacuities of the bladder, and a stone rarely eludes discovery. I would here remark that a stone may be struck and yet no metallic click

heard. In Guy's Hospital Museum I have seen two calculi (2091[56] and 2091[57]), which were composed of alternate layers of fleshy and and earthy matter, each stone being about the size of a walnut with a fleshy covering. They were removed from a boy about two years old, and when first extracted were thought to be hydatids.

How is it that surgeons have occasionally cut into the bladder and found no stone? They struck something which seemed of tolerable dimensions and which gave forth a dull sound. What was the body? It was the promontory of the sacrum. It is well to bear this in mind, as a young operator might be puzzled.

2. A very important point in the operation is that the staff be firmly held. If the eye be kept on the handle of the staff, as usually held, it will be observed, at the moment the surgeon is entering the groove with his knife, the handle of the staff gives a succession of small jerks as the knife is running along; and it will be seen that the surgeon's knife does not run smoothly into the bladder, but has a tendency to stop at points. This results from the impossibility of keeping the staff fixed in the position in which it is generally held. The fact is, the slightest pressure of the surgeon's knife is sufficient to make the staff jerk. The steadier the staff, the easier and speedier will the surgeon's knife glide into the bladder. The greater the force the surgeon uses in driving his knife along the groove of the staff, the less the liability for the knife to slip. However, as a rule, the surgeon cannot use much force, as he would either slip out of the staff or else drive the latter before him. If the staff be very firmly fixed, there is much more comfort and safety for the operator. Now, the staff as usually held possesses but little fixity. The hand of the assistant is kept unsupported in the air, and the little fixity the staff may have is derived from pressing the upper curve of the instrument against the symphysis pubis—a process not devoid of danger, for the instrument may recede an inch or so from the bladder. I believe in many of those cases where the operator has not succeeded in reaching the bladder, or only reaching it after much difficulty, the fault has often rested with the assistant, who has allowed the staff to recede from the bladder.

When required to hold a staff I always use both hands, one being placed over the other, the lower one resting on the crest of the pubis. In this way the staff can be held with a fixity which is unattainable by any other method. The penis can be compressed into a very small space against the crest of the pubis, and another assistant can keep the scrotum out of the way.

Most staffs are too long: the shorter the staff, the steadier it will be. If a long staff be used, the point nearly always presses against that part of the bladder near the sacrum. Now, such a procedure is very dangerous in boys; because, if the surgeon's knife should slip as he is driving it home, the point will most likely perforate, or wound the opposite side of the bladder. Specimen No. 2104[75] in Guy's Hospital Museum illustrates this accident:—the point of the knife penetrated the right side of the bladder opposite to the place of incision, and the boy quickly died of peritonitis.

I consider the stone ought always to be struck with the staff; for it does not at all follow that because it has just been struck with the sound, only a few minutes previously perhaps, that therefore the staff is in the bladder. I know it is often difficult to strike a stone with a staff, but by introducing the finger into the rectum and suddenly tilting up the bladder, the stone will be made to strike the staff.

Too much gentleness cannot be used in passing the staff. The point of the instrument is generally made too sharp, and hence disastrous consequences have ensued. In Liston's case, No. 803, R, University College Museum, the staff was introduced into the bladder *without passing through the prostatic urethra.*

3. Regarding the size of knife to be used.—When the surgeon is about to open the prostate, I consider that he ought not only to know the size of the opening he intends to make, but that he ought also to be aware of the opening he really makes. Any sized opening may be made with a narrow-bladed knife, but it is far better to make a definite opening with a broad-bladed knife than an indefinite opening with a narrow knife. In one case he knows exactly what he is doing, whereas in the other he does not.

Most surgeons use a different sized knife when operating on children to what they use for adults. Not only is the boy's knife much shorter—which is perfectly right, taking into consideration the relative depths of the perinei—but it is also much narrower in the blade. Now, if a surgeon when making his deep incision withdraw his knife through the same parts by which it entered—and this is the safest plan—he will have considerable difficulty in getting his finger fairly into the bladder, and he will either have to use considerable force to enlarge the opening by laceration, or will have to reapply the knife to widen the slit. The aperture made by the boy's knife is only sufficiently large to admit the tip of the little finger. Now, one of the most unpleasant things which can happen to a young surgeon, when operating on boys, is to fail to reach the bladder, and find his finger wandering in the loose cellular

tissue between the bladder and rectum. This mistake arises entirely from the smallness of the opening; the point of the finger pressing against an aperture in the prostate which is not sufficiently large for its admission, and thus pushing the bladder backwards in the pelvis till it leaves the staff. This misfortune rarely occurs except in operating on children, and its cause is to be sought for in the fact that the narrow-bladed knife is used.

It is stated by some surgeons, that the cause of the above accident arises from the connections in the pelvis and perineum being more lax than in the adult. I look upon this, however, as incorrect as a cause. In each case we cut against the staff, which is equally fixed in either instance. The only difference in the method of performing lithotomy in the adult and in the child is that in the latter most surgeons use a smaller knife to what they do in the former, although they desire to introduce the same sized instrument—the left forefinger. I consider that whether we are about to operate on a boy or on an adult, we ought in either case to use a knife of the same breadth of blade, and I found my opinion on the simple fact, that as in either case we desire to introduce the same sized instrument, the left forefinger, so ought we also to use the same sized knife. The knife ordinarily used for lithotomy on the adult is too narrow in the blade, for the forefinger can scarcely be introduced through the aperture made by it into the bladder without rupturing the tissues in its path. In fact, the moment the surgeon gets the point of his finger into the bladder, he uses his digit as a lever, and breaks down tissues in order to work in the rest of the finger. But surgeons say they do not lacerate—they only dilate, and as proof they adduce the fact, that when the point of the finger enters the bladder a firm ring is felt tightly encircling the finger, and that it yields like a sphincter to pressure. It is generally supposed that this constriction results from the muscular fibres encircling the neck of the bladder being firmly contracted on the finger. I have, however, ascertained by experiment on the dead body that this is not correct, for the feeling of constriction is also experienced when operating on the dead body, and must therefore be due to a physical and not a vital cause. The constriction is produced by the submucous fibrous coat of the bladder, and can be imitated by trying to thrust the entire forefinger through the button-hole of a shirt which is only sufficiently large to admit the tip of the forefinger.

If the breadth of the ordinary lithotomy knife were the same as that of the gorget, the surgeon would never experience any difficulty in reaching the bladder, and the parts would escape that bruising and laceration which give rise to suppuration,

phlebitis, and pyæmia. I therefore always use the adult-sized knife when operating on boys.

4. Why does the forceps so often let the stone slip? The usual forceps has always a tendency to eject a stone from its grasp, because its ends are not sufficiently bent inwards. Forceps will not let a stone slip if the blades are well bellied. Professor Liston loved a large powerful forceps, because the peculiarity in his method of operating was that he tore the stone out of the bladder.[1] A young surgeon ought to use a smaller and lighter forceps than what Liston would have recommended, for he will not then be able to use much force, and will extract slowly and gently.

5. Regarding the distance above the anus at which the external incision ought to commence.—Some surgeons begin as high as one inch and three quarters above the anus, others as low down as one inch above the anus. Which incision is the better? I am decidedly in favour of commencing the cut low down, for when we want to extract a calculus we do it in a direction downwards and outwards. The desired object is to open the membranous urethra as near to the prostate as possible. If the operation be commenced low down, a surgeon can thrust in his knife more boldly than when the high incision is resorted to, and hence will generally, at the very first incision, hit or get close to the staff. Not so, however, when the high incision is adopted; for it does not do to drive the knife in boldly, as the bulb will very likely be wounded, and a good deal of troublesome but not dangerous hæmorrhage ensue. I have observed that those surgeons who perform lithotomy with the low incision make only two cuts; that is to say, having made the external incision, they have not to introduce the knife again to divide any structures preparatory to entering the groove in the staff. Thus, therefore, the low incision simplifies the operation.

6. Regarding the use of the gum-elastic tube after lithotomy. —When the operator has a good house-surgeon to rely on, the introduction of the tube is useless. It occasionally happens that a few hours after the operation, when the external edges of the wound have become glued together, bleeding goes on internally. When the patient becomes blanched, and there is no external appearance of bleeding, the finger ought at once to be introduced into the wound, to see that the opening is quite free. If we can be sure that the house-surgeon will be on the look-out for the above accident, and know what measures to take to arrest

[1] No doubt, the spectators wondered at the ease with which he extracted a stone. Little resistance, indeed, could any prostate offer to a Liston's forceps in a Liston's hand.

the hæmorrhage, then we need not trouble about the introduction of a tube.

I will now proceed to discuss the special point I have laid down: *The limited incision of the prostate, in lateral lithotomy, is wrong in principle and mischievous in practice.*

Half a century ago, there were as many great names to be found in favour of making a free incision into the prostate as there were against that procedure. As time, however, wore on, it would appear that the supporters of the limited incision had it all their own way, and it was only now and then that a solitary voice was raised against their method. Professor Syme has always been opposed to the surgery taught by British surgeons on this particular point. The very thing which up to a short time ago all British surgeons were unanimous in saying ought not to be done, Professor Syme said ought to be done. In the fifth edition of his 'Surgery,' p. 413, when enumerating the parts to be cut, he states—"the prostate gland through the whole extent of its left lateral lobe."

Professor Ellis has for a great many years past pointed out to his class, that the so-called dilatation of the prostate was rupture, that surgeons exceeded the limits of the prostate in lithotomy, and that infiltration of urine did not follow division of the capsule of the prostate, inasmuch as the most successful cases of lithotomy were those in early life, when, on account of the very rudimentary condition of the gland, the incision must always exceed the limits of that organ. Mr. Henry Thompson, when defining the extent of the incisions in lateral lithotomy, states—"They involve one side of the prostate gland, it may be nearly to its full extent,[1] while in children and in exceptional cases they go beyond it."

Professor Erichsen has lately given it as his opinion that a stone must either be cut out or torn out.

On looking over most surgical works, it will be observed that the writers very decidedly state that one of the great dangers connected with lithotomy is infiltration of urine following free division of the prostate, and that by merely notching the prostate and afterwards dilating it, the aperture will be large enough to allow any ordinary sized stone to pass without exceeding the limits of the capsule, and thus the dreaded evil will be avoided.

Science profits as much by the elimination of error as by the addition of new truths, and much harm has accrued to surgery by the traditional acceptance of dogmas having no better basis than assertion on which to rest.

When we are told that a stone four or six inches in circumference, together with a pair of forceps and the prostate gland,

[1] 'Lithotomy and Lithotrity,' p. 4.

can all be contained in a fibrous ring only one and three quarter inches in its greatest diameter, and utterly incapable of dilatation, I think we are not wanting in respect even if we refuse to accept, without proof, the assertion of the most eminent surgical authorities.

Remarkable as has hitherto been the almost complete unanimity of surgeons as to the vital importance of not exceeding the limits of the prostate, the absolute absence of all facts in proof of such an assertion is still more remarkable. Take, for instance, the statement of the late Mr. Liston that he could extract a stone four or six inches in diameter through the prostate by dilatation. He provided no facts whatever in proof of his statement. How, indeed, could any surgeon say that he had extracted a stone by dilatation only from the bladder of a man who recovered after the operation? Not having seen the prostate of the patient upon whom he operated, he could not tell whether he had or had not exceeded its boundaries.

Those who advocate the limited incision of the prostate, do so on the principle that inasmuch as the great danger connected with lithotomy is infiltration of urine caused by the deep fasciæ being opened up, it follows that if the operation can be performed without opening the deep fasciæ, the chief danger will be avoided—that the operation can be so performed inasmuch as the prostate can be sufficiently dilated to allow an ordinary sized stone to pass through without destruction of the capsule.

It will be seen that everything hinges on the capability of the prostate to dilate; for if it can be shown that no ordinary sized stone can pass through the prostate by means of a limited incision and subsequent dilatation, then it is useless to attempt to do that which is a physical impossibility. Even were it true that infiltration of urine were one of the consequences most to be dreaded after lithotomy, it would matter nothing, for if it were impossible to prevent it from taking place, it would be useless to attempt by any means to avert it.

I will now proceed to inquire whether any ordinary sized prostate can, after being partially incised, be sufficiently dilated to allow an ordinary sized calculus to be extracted without exceeding the limits of the gland.

It is well, in the first place, to recall some anatomical facts connected with the prostate. It is situated between two very dense fasciæ which are absolutely incapable of being stretched: I mean the pelvic fascia and the recto-vesical fascia. It has two fibrous jackets—a plexus of veins separating one from the other, and the inner one being the tight-fitting one.

It will thus be seen that the prostate is not only hemmed in on all sides, but has a close-fitting coat which can only be rup-

tured, not dilated. The greatest diameter of the fibrous ring which surrounds the prostate is only one and three quarter inches, whilst the average diameter is little more than an inch. Judge, therefore, of the impossibility of extracting a stone four or six inches in circumference, together with the forceps, through a fibrous ring of an average diameter of one inch and lined with the substance of the prostate gland.

I have on a great many occasions performed lithotomy on the dead body, and dissected the bladder and prostate to see the results. Several years ago I informed my friend Professor Erichsen of the results at which I had arrived; and since then I have often repeated my experiments, and see no reason to alter the conclusions I had come to. On May 15th of last year I exhibited to the Pathological Society the bladders and prostates removed from five different subjects on which I had recently performed lithotomy, post-mortem.

I have experimentally ascertained the following facts :—

1. The prostatic urethra will only admit the terminal joint of the forefinger without laceration.

2. If the introduction of the forefinger be continued, the mucous membrane splits longitudinally as the second joint is passing through. The urethra splits in the roof because the convexity of the joint is pressed against that part. In lateral lithotomy the incision into the prostate prevents laceration of the roof of the prostatic urethra.

3. If a stone half an inch in diameter be extracted through a prostate in which no incision has been made, the mucous membrane of the floor of the urethra is lacerated and the prostate slightly torn; the capsule remains perfect, but the orifices of the ejaculatory ducts can seldom be distinguished.

4. If a calculus half an inch in diameter be extracted through a prostate which has been partially incised, as in lateral lithotomy, the capsule and orifices of the ejaculatory ducts remain perfect.

5. Stones upwards of half an inch in diameter, when extracted by the median operation, lacerate more or less the prostate and its capsule, and obliterate the orifices of the ejaculatory ducts.

6. Calculi of one inch in diameter and upwards, when extracted through a prostate which has only been partially incised, in the lateral operation, lacerate the gland and its capsule completely in a direction downwards and outwards, and obliterate the orifices ofthe ejaculatory ducts.

7. If a calculus be extracted through an aperture which was made by cutting and not by laceration, then the orifices of the ejaculatory ducts can always be distinguished.

8. The so-called dilatation of the prostate is complete rupture.

9. When a stone is extracted from the bladder by means of a limited incision and subsequent so-called dilatation, either in median or lateral lithotomy, there is always more or less eversion of the gland; that is, in such cases, the stone has a tendency to enucleate the gland from its capsule in a direction forwards. Thus, therefore, only a very small stone can be extracted through a partially incised prostate without completely lacerating the gland and its capsule. In fact, if an ordinary sized stone be extracted, either in median or lateral lithotomy, by a limited incision, the prostate is found split into two, the halves being held together by a remnant of the capsule about half an inch broad. Some persons would object to deductions drawn from experiments made on the dead body, and say that results obtained after death must be very different to what happens after lithotomy on the living. It must, however, be remembered that the mechanical properties of the fasciæ are not altered for some time after death, and therefore experiments made a few hours post mortem afford similar results to those which would have ensued on the living.

From a personal examination of all the pathological museums in London, I have ascertained the following facts:

1. That out of the very many specimens of bladders and prostates after lithotomy, there is no unequivocal specimen which shows that an ordinary sized calculus can be extracted from an ordinary sized prostate, by means of a limited incision and the subsequent so-called dilatation, without complete rupture of the prostate and its capsule.

2. That in extracting ordinary sized calculi through the prostate, not only is the gland and its capsule completely ruptured, but the rent extends into the bladder as far usually as the orifice of the left ureter.

3. That when there has been much laceration and bruising of parts, the orifices of the ejaculatory ducts can no longer be distinguished.

4. That there are several bladders and prostates of persons who have lived upwards of ten years after lateral lithotomy, and in such specimens the cicatrix can be seen extending into the bladder nearly to the orifice of the left ureter.

5. That a fistula in the bladder communicating with the rectum is not an uncommon occurrence after lithotomy—that such fistula is the result of laceration, and would not appear to be of much consequence.

6. That in ordinary lithotomy, the prostate is completely split

into two, the halves being generally held together by a remnant of the capsule about half an inch broad.

7. That the most frequent cause of death after lithotomy would seem to be extensive suppuration, followed by phlebitis and pyæmia, set up by the laceration and bruising of parts.

8. That infiltration of urine after lithotomy must be regarded as a surgical curiosity.

It will thus be seen, whether cases of lithotomy on the living or dead be examined, the same conclusion will be arrived at —that an ordinary sized stone cannot be extracted through an ordinary sized prostate, by means of the limited incision and subsequent so-called dilatation, without complete rupture of the gland and its capsule.

A subject of very great importance which has not yet received the attention of the profession, is the occurrence of impotence after lithotomy. When it takes place, it is from laceration of the mucous membrane around the orifices of the ducts, and their subsequent plugging. My objection to the median operation is that the mucous membrane generally gets lacerated in the floor of the urethra, and the prostate occasionally split into halves in the paths of the ducts.

The following are the conclusions at which I have arrived:

1. When lateral lithotomy is performed, the stone ought always to be cut out, and not torn out.

2. That the median operation is not justifiable for the extraction of calculi which are upwards of half an inch in diameter; for if such sized stones be removed by that process, obliteration of the orifices of the ejaculatory ducts and permanent impotence will result.

Lateral lithotomy in the boy is such a very successful operation, and if performed without laceration of the parts causes no injury to the seminal ducts, that no possible advantage can be gained by substituting the median operation for it, inasmuch as that procedure cannot be accomplished in the child without the laceration of parts, and such laceration will occur in the very spot we ought to avoid—namely, in the floor of the urethra. The only cases in which I consider the median operation eligible, is in those rather rare instances in the adults in which we have a small stone and an enlarged and diseased prostate, and in which lithotrity may not be considered advisable.

Thus, therefore, as I have shown that no ordinary sized stone can be extracted through an ordinary sized prostate by the process of limited incision and so-called dilatation without complete rupture of the prostate and its capsule, and occasional injury to the ducts and other important parts, it follows that the

surgeon must choose whether he will extract a calculus by cutting it out or by tearing it out.

I will show that the latter method is the preferable one.

When a calculus is extracted by a combination of cutting and dilatation (laceration), the surgeon does not know what mischief he may have caused; for when once he begins to tear, he cannot possibly tell where his rent will terminate. The laceration may extend into the rectum, far into the bladder, or destroy the seminal ducts. Now, although these injuries are serious, they are not positively dangerous, and therefore the fatal consequences resulting from the effects of laceration are to be found in the extensive smashing of the plexus of veins round the prostate and neck of bladder. At all times a clean-cut wound is preferable to a lacerated one; for not only is the latter attended with considerable local destruction of tissue, but it is more often followed by phlebitis and pyæmia than the former. Now, the plexus of veins round the prostate cannot be bruised with impunity. If such be done, extensive suppuration will follow, and we are exposing the patient to all the chances of pyæmia and phlebitis. In the adult and old these veins often attain an enormous size, and any destruction of tissue must be at the expense of running a serious risk. An examination of the specimens in the museums will convince the observer, that above all things force ought to be avoided in extracting stone: gentleness implies cutting, force laceration.

What is meant by a free incision of the prostate as opposed to a limited one? I look upon the term "free incision" as entirely relative. I consider that whether the stone be large or small, it ought to be extracted through an aperture which has been made entirely by cutting; that is to say, that the stone ought to be extracted without the employment of force. It may perhaps be asked, how can it be ascertained beforehand the the exact size which the incision ought to be? It cannot be so ascertained. Firstly, make an opening into the bladder with a broad-bladed knife, taking care not to enlarge the opening as the knife is withdrawn. The finger will pass into the bladder with the greatest ease, and no tearing will be requisite to insinuate the forceps. If, during the extraction of the stone, it is found that the parts offer the slightest resistance to its exit, a probe-pointed knife ought to be introduced along the outer blade of the forceps, and a very slight cut made downwards and outwards. When parts are on the stretch, a very slight cut gives a great deal of room.

Two objections will be raised against cutting in preference to dilatation (rupture). It will be said that serious and fatal hæmorrhage may arise, or that the deep fasciæ may be opened

up and infiltration of urine take place. Professor Ellis very truly remarks, that it is just in those very cases where we know we must exceed the limits of the prostate—in children—where, on account of the very rudimentary condition of the gland, a stone cannot be extracted without dividing the deep fasciæ, that the greatest success ensues. So long as urine has a free exit it rarely occasions harm. The fear of infiltration of urine is imaginary; the fear of hæmorrhage must also be considered as very slight. How rarely do we see a death from hæmorrhage!—unless, indeed, the pudic or some abnormal branch of it be wounded. In the adult or old, there will of course often be free bleeding from the skin surrounding the prostate if freely divided; but such hæmorrhage can nearly always be speedily stopped. I always myself cut a stone out in preference to tearing it out, and I only once had any troublesome bleeding, which was soon stopped by applying ice above the pubes.

To show that free division of the prostate is a harmless procedure, I need only refer to the experience of Mr. Brett, formerly surgeon to the Governor-General of Bengal. Mr. Brett had very great success as a lithotomist. He cut for stone, by the lateral operation, no less than 108 times; and although two thirds of his cases were under puberty, yet when we consider that the rate of mortality was only 1 in 15, we must consider him as having been a wonderfully successful operator. His last sixty-eight cases all recovered; many of them were Europeans. Now on what principles did he operate? At page 206 of his work 'On some of the Surgical Diseases of India,' he stated—"The prostate is completely divided on withdrawing the knife, cutting downwards and outwards with a slight pressure in the same line as the former incision." Further on, when discussing the cause of death after lithotomy, he states, at page 224—"It has certainly appeared to me, that the very result so much apprehended from a free incision of the neck of the bladder seems to have followed in most of my unsuccessful cases from a want of a sufficiently free incision; whereas my unhesitatingly cutting all opposing textures has, especially in my last sixty-eight operations, been followed by the happiest results. Indeed, I have almost felt conscious whenever a case terminated unfavorably, or the recovery had been slow, that my internal incision had not been sufficiently bold, and that the operation had been protracted thereby." Better surgical principles than these no book contains.

Mr. John Wood has written a paper which is published in the third volume of the 'Medico-Chirurgical Transactions,' entitled, "On an Operation for extracting a Stone from the

Bladder by Urethrotomy and Dilatation of the Prostatic Urethra by means of an expanding Staff."

Dr. Willis has brought forward a method which he calls Lithectasy, by which the stone is extracted by opening the membranous urethra and dilating the prostatic urethra very gradually. I have the same objection to the above two proposed operations as I have to all operations conducted on the median principle, *i.e.*, that we expose the patient to the risk of being emasculated, inasmuch as I have shown that in the median operation laceration nearly always occurs, and that such laceration takes place in the course of the orifices of the seminal ducts.

In concluding, therefore, I would repeat that—1. The median operation is not generally admissible. 2. In lateral lithotomy the stone ought to be cut out, and not torn out.

ART. IV.

On the Complications of the different Forms of Bright's Disease. By T. GRAINGER STEWART, M.D., F.R.S.E., Pathologist and Extra Physician to the Royal Infirmary, Edinburgh.

IN my former papers I have explained what I believe to be the true morbid anatomy and clinical history of the affections usually classed together under the title "Bright's Disease." The general results may be seen in the subjoined tabular statement:

TABLE I.—*Showing the leading Symptoms of the different forms of Bright's Disease.*

		URINE.			DROPSY.	FEVER	COEXISTING GENERAL DISEASES.	AFFECTIONS OF ABDOMINAL VISCERA.	URÆMIA.
		Quantity.	Quality.	Deposits.					
Inflam. Form	Stage 1st.	Diminished	Albuminous or bloody	Bloody; finely granular, or hyaline casts	Early and severe	Generally severe	Scarlatina and other exanthemata	None	Somewhat rare.
	„ 2nd.	Diminished or not increased	Albuminous; often pale	Fatty and coarsely granular casts	Severe or diminished	Less severe	—	None	More common.
	„ 3rd.	Diminished	Albuminous	Fatty and hyaline casts	Severe or slight, but occasionally recurring	Slight	—	None	Common.
Waxy or Amyloid Form	Stage 1st.	Increased	Pale, with little albumen	Few hyaline casts	None	None	Syphilis, tuberculosis, chronic suppuration, &c.	Liver and spleen often enlarged	None.
	„ 2nd.	Increased	Pale, with more albumen	Few hyaline; finely granular	None, or very slight	None	Ditto	Ditto	None.
	„ 3rd.	Increased or not much diminished	Pale; albuminous	Ditto	None, or slight	Slight	Ditto	Ditto	Occurs occasionally.
Contracting Form	Stage 1st.	Natural, or slightly increased	Natural	Few hyaline casts	None	None	Gout	None	None.
	„ 2nd.	Natural, or diminished	Albuminous	Hyaline and fatty casts	None, or slight	Slight	Ditto	None	Occurs occasionally.

It is, of course, impossible to do justice in such a table to all the points which weigh with us in diagnosis, or even to the modifications of the pathological conditions with which we meet; but the leading elements are indicated, and I am satisfied, from all that I have seen, that they may suffice for correct diagnosis, provided we keep carefully in view two considerations, viz., that combinations of the forms frequently occur, and that it is difficult to ascertain the early history of many cases which come under our notice at an advanced stage.

The present paper is intended as a contribution to our knowledge of the complications of these affections. Our first step must, therefore, be clearly to define our conception of what a complication is. In the widest sense, we may say that any disease coexisting with another is entitled to be so called. But experience has shown that certain diseases frequently coexist, and that such are related, as cause and effect, or as results of a common cause; these affections are, in a special sense, complications. But, further, it may be held that in strict accuracy the name should be confined to coexisting diseases, which, though frequently met with together, are essentially independent of each other, or are results of a common cause, and should not be applied to those which are consequences of others, such being regarded as part of the original disease—mere symptoms, and not truly complications. But, in accordance with the example of previous writers and for practical convenience, I shall adhere to the second definition of the term, and shall so include some conditions which are undeniably mere symptoms, and others which might reasonably be held to be such. Of complications thus understood we recognise several varieties:—1. Some are results of a primary disease—as dropsy of cardiac, renal, or hepatic affections. These are consequent complications. 2. Some are causes of the disease with which they coexist—as bronchitis of emphysema, arterial degeneration of apoplexy. These are causal complications. 3. Some are results of a cause which they have in common with the disease which they complicate—as sore-throat and periosteal nodes of syphilis. These are complications from a common cause. We must further bear in mind, that while in many cases the relations between different complications are obvious and direct, in others they are obscure and indirect, and that it not infrequently happens that diseases coexist without any special relationship existing between them at all.

In seeking to determine the frequency and importance of the complications of any disease, we must insist upon attention to the following points:

1. That the two diseases coexist in such a proportion of cases as cannot be explained on the theory of mere accidental concurrence.

2. That all cases be put out of consideration in which the complications may be referred to other causes.

3. That the supposed consequent complication be more frequent

as the disease which is regarded as the cause advances or increases in severity.

To these principles I shall strictly adhere in the following paper.

This paper is, as I have said, intended as a contribution to our knowledge of the complications of Bright's disease. It consists of an analysis of the cases examined post-mortem in the Royal Infirmary during a period of forty months. They are 131 in number, and the different forms occur in the following proportions:—Inflammatory form 51 cases, waxy 50, contracting 13, combined waxy and inflammatory 17. In order to make my results as reliable as possible, I have struck off in my calculations 23 examples of the inflammatory form in which death took place in the very earliest stage of the renal affection. It is apparent that the number of cases at my disposal is much smaller than might have been wished; but they have all been examined and recorded by myself, and are capable of affording at least a contribution towards a more accurate knowledge of the subject. I have given the results in percentages—the only form in which we can conveniently grasp their meaning; and for purposes of comparison I have reduced some tables by other authorities to the same standard. The fractions have been expressed only when they amount to one fourth, one half, or three fourths of unity, for this I consider sufficiently accurate for all practical purposes.

The number and importance of the complications of Bright's disease have been long recognised. Dr. Christison, in his excellent work on this subject published nearly thirty years ago, says—"The primary disease seldom continues long without other important diseases being superadded to it, and giving occasion to a great variety of additional symptoms;" and he enumerates among them secondary affections of most frequent occurrence:—Dropsy, diarrhœa, pleurisy, peritonitis, pneumonia, catarrh, dyspepsia and chronic vomiting, coma with other affections of the head, chronic rheumatism, chronic diseases of the heart, and organic diseases of the liver.[1] Frerichs, writing in 1851, enumerates affections of the circulation, particularly cardiac hypertrophy; of the lungs and air-passages, particularly œdema, pneumonia, vascular emphysema, and tubercle; of the liver, fatty degeneration and cirrhosis; of the spleen, hypertrophy; of the stomach and intestines, chronic catarrh and occasionally ulceration; of the central organs of the nervous system, apoplexy and serous effusions; of the serous membranes, dropsies and inflammations; and, lastly, occasional affections of the bones and skin.[2] He carefully analysed 292 cases recorded by Bright, Christison, Gregory, Martin Solon, Rayer, Becquerel, Bright and Barlow, Malmston and himself; and from his tabulated results I have calculated the following percentages.

[1] Christison, 'On Granular Degeneration of the Kidneys,' p. 76.

[2] Frerichs, 'Der Brightische Nierenkrankheit und deren Behandlung,' p. 44.

TABLE II.—*Showing the per-centage of Affections accompanying Bright's Disease (Frerichs).*

HEART.		LUNGS.			LIVER.		SPLEEN
Hypertrophy, without other known cause.	Congestion and Œdema.	Pneumonia.	Emphysema.	Tubercle.	Cirrhosis.	Fatty Degeneration.	Enlargement.
33·75	25·5	9·5	7·5	12·5	8·5	6·5	8·5

STOMACH.	INTESTINE.	NERVE CENTRES.		INFLAMMATION OF SEROUS MEMBRANES.		
Catarrh.	Catarrh.	Sanguineous Apoplexy.	Serous Effusion.	Pericardium.	Pleura.	Peritoneum.
8·5	11·5	3·5	13·6	4·5	12·5	11·5

These results are valuable as showing the complications of all the forms of Bright's disease; but they throw no light on the different forms or their stages. M. Paul Lorain[1] mentions, in addition to the above-named complications, amaurosis and cephalalgia, which he has found of frequent occurrence. In a valuable paper in the 'Medico-Chirurgical Transactions,' Dr. Dickinson gives the results of 369 cases of Bright's disease examined post-mortem in St. George's Hospital.[2] He distinguishes two forms of disease—nephritis and granular degeneration. I subjoin a copy of his table.

TABLE III.—*Showing the per-centage of Affections accompanying each Renal Disorder (Dickinson).*

	NEPHRITIS (119 Cases).	GRANULAR DEGENERATION (250 Cases).
Bronchitis . . .	18·5	22·0
Diarrhœa . . .	20·1	11·6
Vomiting . . .	26·0	15·6
Pleurisy . . .	64·3	60·8
Pericarditis . . .	27·7	32·8
Peritonitis . . .	25·2	13·6
Pneumonia . . .	20·1	12·4
Œdema . . .	66·3	48·0
Ascites . . .	33·6	26·0
Hydrothorax . .	28·5	25·2
Hydropericardium . .	24·3	14·0
Epistaxis . . .	0·8	0·8
Erysipelas . . .	15·9	6·8
Cirrhosis of Liver . .	10·9	15·2
Atheroma . . .	21·8	52·4
Hypertrophy of Heart .	24·3	48·0
Valvular Disease . .	17·4	43·2
Convulsions . .	10·9	5·6
Simple Coma . .	10·0	8·8
Sanguineous Apoplexy .	2·5	6·8.

[1] 'De l'Albuminurie,' par Paul Lorain, M.D., Paris, 1860.
[2] 'Medico-Chirurgical Transactions,' vol. iv, 1861.

The large number of facts here tabulated renders this very valuable; but as the cases have not been subdivided according to the classification of Virchow, now almost universally acknowledged to be correct, further information must be sought. Rosenstein[1] distinguishes between the different forms, and enumerates among the results of what he terms the diffuse nephritis (which very much corresponds to Virchow's parenchymatous nephritis and my inflammatory form), dropsy, hypertrophy of the left ventricle of the heart, gastric disorder, nervous symptoms, uræmia, retinitis, bronchial catarrh, pericarditis, pleuritis, pneumonia, and peritonitis. He gives the following results of 114 cases collected by himself:

Pneumonia was present in . . .	17 per cent.
Pleurisy	16 „
Peritonitis	8 „
Pericarditis	7 „
Mediastinitis	2 „

In regard to the waxy or amyloid form, he states that dropsy is *common*, occurring *in* 61 *out of* 72 *cases*, but that its amount varies—a statement which the sequel will disprove; that among the symptoms connected with digestion, frequent diarrhœa is to be mentioned; that secondary inflammations of serous membranes and parenchymatous organs occur, but less frequently, and among them all that peritonitis is the most prominent; that in advanced conditions, hypertrophy of left ventricle of heart, uræmia, and retinitis occur. Among the causes he reckons chronic tubercular disease of the lungs, long-continued suppurations, syphilis, and obstinate intermittent fevers.

Of the gouty or contracting form he says nothing.

From these statements it is evident that the complications of Bright's disease generally, and the frequency with which they occur, are pretty well ascertained, but that there is room for more exact information as to the complications of the individual forms and of their stages. The present paper is intended as a contribution towards the supply of this want. Being drawn from pathological observations, it is defective in respect of some points of clinical interest; but it is, on the other hand, I think, peculiarly trustworthy as to the internal lesions. I would gladly have added these points of clinical importance, but find that my notes are so defective as not to enable me to do so.

The following table shows the proportionate frequency of the different complications in each of the forms of the disease.

[1] 'Die Pathologie und Therapie der Brightischen Nierenkrankheiten,' von Dr. Rosenstein, Berlin, 1863.

TABLE IV.—*Showing the Chief Complications of the Different Forms of Bright's Disease.*

	Number of Cases.	Dropsy.	Hypertrophy of Heart.			Lungs and Bronchi.			Inflammations of Serous Membranes.						Liver.				Spleen.		Intestine.		Brain.
									Pericard.		Pleura.		Peritoneum.										
			Total.	Otherwise explained.	Kidney sole Cause.	Congestion and Œdema.	Pneumonia.	Tubercle.	Total.	Independ. of M.B.	Total.	Independ. of M.B.	Total.	Independ. of M.B.	Fatty.	Waxy.	Waxy and Fatty Degeneration.	Cirrhosis.	Waxy Degeneration.	Capsule Thickened.	Waxy Degeneration.	Tubercle.	Sanguineous Apoplexy.
Inflam. form.	28	67·75	57·0	17·75	39·25	64·0	21·25	7·0	7·0	0	17·75	3·5	3·5	3·5	25·0	0	0	14·0	0	0	0	3·5	7·0
Waxy or Amyloid . . .	50	6·0	12·0	8·0	4·0	20·0	4·0	48·0	8·0	0	6·0	4·0	6·0	6·0	6·0	32·0	46·0	0	74·0	0	58·0	18·0	2·0
Contracting .	13	23·0	53·75	7·5	46·25	53·75	7·5	23·0	7·5	0	15·25	0	0	0	15·25	0	0	15·25	0	38·25	0	15·25	15·25
Inflam. & Waxy combined .	17	47·0	0	0	0	29·25	0	52·75	...	...	17·5	0	0	0	5·75	23·5	52·75	5·75	88·0	0	52·75	17·5	0
Peculiar Cases fatal in early stage . .	23	0	4·25	4·25	0	73·75	21·5	4·25	8·5	8·5	30·25	30·25	4·25	4·25	13·0	0	0	0	0	0	0	0	0
Total Inflam. .	51	37·0	33·0	11·0	0	68·5	21·5	5·75	3·75	3·75	23·25	15·5	3·75	3·75	19·5	0	0	7·75	0	0	0	1·75	3·75

I propose to take up the different complications in succession, and to explain the facts given in the tables with regard to them.

I. *Dropsy.*—This is well known to be very specially associated with the inflammatory form, and these tables confirm the accuracy of the opinion, for they show that it was present at the time of death in 37 per cent. of all my inflammatory cases; or if we subtract the peculiar cases which died in the earliest stage, and leave only those which would be recognised as examples of Bright's disease by all pathologists, in 67 per cent. of them; while in the waxy it was present in only 6 per cent., and in the contracting in 23. Moreover, of the cases in which inflammation of the tubules was superadded to waxy degeneration, it was present in 47 per cent.; and thus a relationship between this symptom and disease of the tubules is clearly demonstrated. But if it were possible to follow out the clinical history of my cases, I am satisfied that this relationship would be found even more striking, for a considerable proportion of people who have been affected with inflammatory Bright's disease die of diseases unconnected with the kidneys; while the renal malady, being in a quiescent state, is not attended by its characteristic symptoms, and so on post-mortem examination they are wanting. In contrasting the different forms, I have merely referred to the frequency, not to the severity, of the accompanying dropsy; but it may be well to state that inflammatory cases are not only the most frequently dropsical, but the most severely so. Now, in regard to the waxy form, my results differ very strikingly from the statements of Rosenstein, for he found dropsy in 61 out of 72 of his waxy cases; and the only explanation of the discrepancy which occurs to me is that he has included all cases in which any waxy degeneration existed, thus bringing in those of combined waxy and inflammatory, which of course have dropsy. This explanation is borne out by many other considerations.

It is further interesting to note the relationship of dropsy to the different *stages* of the inflammatory form, as shown in the following table.

TABLE V.—*Showing the Relations of Dropsy to the Stages of the Inflammatory Form.*

Stage 1st	. . .	25·0 per cent.
„ 2nd	. . .	84·5 „
„ 3rd	. . .	85·5 „

Thus it appears that as the case advances to a fatal termination the frequency of dropsy increases; for while it was present in but 25 per cent. of the fatal in the first stage, it was in 84 of those in the second, and 85 of those in the third. But even the first stage

cannot long exist without dropsy being developed, and its comparative rarity in my cases is explained by the fact that some of the patients died of erysipelas, scarlatina, or similar diseases, before the renal affection had advanced sufficiently to induce dropsy.

II. *Hypertrophy of Heart.*—This is a frequent complication, and occurs with all the forms, though chiefly with the inflammatory and contracting. In not a few cases, however, it depends, in part, at least, upon causes other than renal, *e. g.* valvular disease, atheroma of vessels, but in a considerable proportion Bright's disease is the only apparent cause, and doubtless in all it exerts some influence. I have met with it in 57 per cent. of the inflammatory form, 12 per cent. of the waxy, and 53 per cent. of the contracting; but of these, 17 per cent. of the inflammatory, 8 per cent. of the waxy, and 7 per cent. of the contracting, had other causes. There remain still, however, a considerable proportion of cases in which the sole cause was situated in the kidneys, viz., 39 per cent. of the inflammatory, 4 per cent. of the waxy, and 46 per cent. of the contracting. Frerichs had found this complication in 33 per cent. of all his cases; Dickinson in 24 per cent. of his cases of nephritis and in 48 per cent. of his cases of granular degeneration; and Rosenstein states that it is common in the diffuse nephritis and occurs in advanced conditions of the waxy.

As the disease advances, the tendency to this complication increases, as is shown by the following table.

TABLE VI.—*Showing the relation of Cardiac Hypertrophy to the Stages of the Forms.*

		Per cent.			Per cent.
Inflamm. form	Stage 1st	12·5	Waxy form	Stage 1st	0
	„ 2nd	38·5		„ 2nd	0
	„ 3rd	100		„ 3rd	5·9

Here it appears that in the first stage 12 per cent. had cardiac hypertrophy without other known cause, in the second stage 38, and in the third 100 per cent.

This last fact is certainly very curious. The only explanation which suggests itself to me is that, if important disease of the valves or vessels coexist with Bright's dissase, the patient seldom lives long enough to reach the third stage of that malady. My observations amply confirms Rosentein's statement, both with regard to the inflammatory and waxy forms. And it is interesting to notice how exactly this complication possesses the qualities which I have insisted as characteristic of the true consequent complications.

III. *Affections of Lungs and Bronchi.*—Three pulmonary diseases

occur not infrequently in combination with Bright's disease, viz., congestion and œdema of the lungs and bronchi, pneumonia, and tubercle. We shall consider each of them in succession.

1. *Congestion and œdema of lungs and bronchi.*—This is a common complication of all the forms, and constitutes, indeed, a frequent fatal termination. It may be acute, developing with great rapidity; or it may be chronic, slowly and insidiously increasing. It is most common in association with the inflammatory and contracting forms, less so with the waxy, and the addition of the inflammatory to the waxy increases its frequency. It existed in 64 per cent. of my inflammatory cases, 53 per cent. of the contracting, and only 20 per cent. of the waxy, while in the combined waxy and inflammatory it was present in 29 per cent. According to my tables, the frequency of this complication is much greater than the observations of Frerichs would make it, for he found it in only 25 per cent. of his cases. On the other hand, bronchitis is regarded as a frequent complication by some writers, while I have found a truly inflammatory condition of the bronchi exceedingly rare. That mucous or muco-serous fluid is frequently present in quantity is certainly true, and the existence of this fluid, of course, produces the symptoms and physical signs of bronchitis; but this is connected with œdema rather than inflammation, for I have found that in almost every instance a healthy or simply congested condition of the mucous membrane was found when the mucus had been washed off by a stream of water. We find that this complication is most frequent in the first and third stages of the inflammatory, and the more advanced conditions of the waxy and contracting, as is shown in the following table.

TABLE VII.—*Showing the relations of Pulmonary Congestion and Œdema to the Stages of the Inflammatory Form.*

		Per cent.			Per cent.			Per cent.
Inflam. form	Stage 1st,	75·0	Waxy form	Stage 1st,	0	Contracting form	Early stage,	0
	„ 2nd,	46·0		„ 2nd,	20		Advanced „	53·75
	„ 3rd,	85·5		„ 3rd,	20			

2. *Pneumonia.*—Inflammation of the lungs coexists with Bright's disease in a considerable number of cases. It was present in 21 per cent. of the inflammatory, 4 per cent. of the waxy, and 7 per cent. of the contracting cases. But it appears to me very doubtful whether any really important relationship exists between them. Frerichs found it in only 9 per cent. of his cases, and Dickinson in 20 per cent. of his tubular and 12 per cent. of his intertubular.

Moreover, when we refer to the stages at which it occurred, we find that it is as frequent, or even more so, in the early as in the later, as is shown in the following table.

TABLE VIII.—*Showing the relation of Pneumonia to the Stages of the Forms.*

Inflam. form	Stage 1st,	12·5 per cent.	Waxy form	Stage 1st,	22 per cent.
	„ 2nd,	30·0 „		„ 2nd,	0 „
	„ 3rd,	14·0 „		„ 3rd,	0 „

This table shows that with the waxy it was found only in cases in the first stage. With the inflammatory it was most frequent in the second, and about equally common in the first and third. Now, if it were an affection really resulting from Bright's disease, we should expect that as that malady advances it would increase in frequency, and this is not the case.

3. *Tubercle.*—This affection of the lungs is shown to be peculiarly related to the waxy form, for while it was present in only 7 per cent. of the inflammatory and 23 per cent. of the contracting, it existed in 48 per cent. of the waxy and in 52 per cent. of the combined waxy and inflammatory. There can be little doubt that these affections often depend upon a common cause, such as constitutional syphilis; but it is certain that while tubercle rarely becomes superadded to Bright's disease, that disease is very frequently superadded to it. When this occurs I have generally found the renal affection to be combined waxy and inflammatory.

The following table shows the relation of tubercle to the different stages.

TABLE IX.—*Showing the relation of Tubercle of Lungs to the Stages of the Forms.*

Inflam. form	Stage 1st,	12·5 per cent.	Waxy form	Stage 1st,	66·5 per cent.
	„ 2nd,	15·5 „		„ 2nd,	60·0 „
	„ 3rd,	0 „		„ 3rd,	35·25 „

In this case it is very manifest how important the relationship is in the earlier stages, and how in all the forms the frequency diminishes as the case advances.

IV. *Inflammation of Serous Membranes.*—These affections have been commonly regarded as among the most serious and fatal of the complications of Bright's disease. Dr. Watson says—"Intercurrent acute inflammation is not an uncommon cause of the patient's death. The pleura appears to be much more often affected in this manner than either the peritoneum or the pericardium."[1] Frerichs' cases give the following results:—Pleurisy was present in 12 per cent., peritonitis in 11 per cent., and pericarditis in 4 per cent. Dr. Dickinson found the affection much more frequent, pleurisy

[1] Watson's 'Practice of Physic,' 4th edit., vol. ii, p. 682.

occurring in 64 per cent. of cases of nephritis, in 60 per cent. of cases of granular degeneration; peritonitis in 25 per cent. of the former, 13 per cent. of the latter; pericarditis in 27 per cent. of the former, and 32 per cent. of the latter. Rosenstein again found pleurisy in 16 per cent., peritonitis in 8 per cent., and pericarditis in 7 per cent. of his cases of diffuse nephritis; and he states generally that with the waxy degeneration the same affections occur, but less frequently.

Thus, a remarkable discrepancy exists between the results of Dr. Dickinson's London experience and that of the German experiences of Rosenstein, and the collated German, British, and French results of Frerichs. My observations accord more with those of Frerichs and Rosenstein than with those of Dickinson, for I find that with the inflammatory form I had 17 per cent. of pleurisy, 7 per cent. of pericarditis, and 3 per cent. of peritonitis; but that in a considerable number of the cases of pleurisy, and in all those of peritonitis, the inflammation manifestly resulted from other causes. With the waxy I had 6 per cent. of pleurisy, 8 per cent. of pericarditis, and 6 per cent. of peritonitis; and here, again, many of the cases were distinctly referable to other causes. With the contracting I had 15 per cent. of pleurisy, 7 per cent. of pericarditis, and no case of peritonitis.

Thus, we had of inflammatory cases not proved to depend upon causes other than the renal disease, 14 per cent. of pleurisy, 7 per cent. of pericarditis, and no peritonitis. But when we further inquire into the stages of the renal disease at which the complication appeared, we find that the inflammations are connected with the earlier stages rather than the later. It is shown in the following table.

TABLE X.—*Showing the relation of Serous Inflammations to the Stages of the Inflammatory Form.*

	PLEURISY.	PERICARDITIS.	PERITONITIS.
	Per cent.	Per cent.	Per cent.
Stage 1st . .	7·0	7·0	0
„ 2nd . .	3·0	0	0
„ 3rd . .	3·0	0	0

Now, it is singular that so large a proportion of the cases of pleurisy, and that all the cases of pericarditis, should have occurred in the first stage. If this inflammatory affection of the kidneys was the cause, we would rather have expected that as the renal affection advanced the frequency of such inflammations should have increased.

But, again, when we turn to the waxy cases, we find that only 2 per cent. of the cases had pleurisy, 8 per cent. had pericarditis, and 2 per cent. peritonitis, which were not proved to depend upon causes other than Bright's disease. On referring to the stages, we find that all the cases of pleurisy coexisted with the first stage; that in one case of pericarditis the stage was not recorded, but that the other cases, amounting to 6 per cent., occurred in the last stage, as is shown in the following table.

TABLE XI.—*Showing the relation of Serous Inflammations to the Stages of the Waxy Form.*

	PLEURISY.	PERICARDITIS.	PERITONITIS.
	Per cent.	Per cent.	Per cent.
Stage 1st . .	2	0	0
„ 2nd . .	0	0	0
„ 3rd . .	0	6	0

It is evident that these results give no countenance to the opinion that waxy degeneration is a cause of pleurisy or peritonitis; and with regard to pericarditis, while the facts recorded are reconcilable with the view commonly held, it cannot be asserted that they strongly support it.

Turning now to the contracting form, we find that pleurisy occurred in 15 per cent., pericarditis in 7, peritonitis in none, and all the cases were in the later stages of the malady. But it must be borne in mind that the number of cases of this affection was small, and that, when the inflammation existed, though it could not be proved to depend upon other causes, it did not appear closely connected with this, a remark which applies also to the other forms.

With the view of testing the question whether these serous inflammations may not be accidental coincidences, rather than true complications, I have examined the reports of my last twenty-five cases of aneurism of the aorta and cancer of the stomach (diseases which are not supposed specially connected with serous inflammation), and calculated the per-centage of serous inflammations accompanying them. The results are shown in the following table, along with the exact results in my three forms. I have, of course, here as elsewhere, carefully separated such cases as owed their serous inflammation to some definite cause.

TABLE XII.—*Showing the frequency of Serous Inflammations in cases of Aortic Aneurysm, &c.*

	PLEURISY.	PERICARDITIS.	PERITONITIS.
	Per cent.	Per cent.	Per cent.
Aortic Aneurysm . .	8·0	0	0
Cancer of Stomach .	12·0	0	8
Inflam. Bright's Disease	14·0	7·0	0
Waxy „ .	2·0	6·0	0
Contracting „ .	15·0	7·0	0

From this table it is evident that inflammation of the pleura is somewhat more frequent in nephritis and cirrhosis of the kidney than it is in cancer of the stomach or aortic aneurysm, but in a proportion by no means important; that, on the other hand, pericarditis is more common in all the forms of Bright's disease than it is in either of the other diseases investigated; while peritonitis is absent in all the cases except cancer of the stomach, in which it occurs occasionally independently of perforation or other direct and obvious cause.

From these facts I conclude that the frequency of intercurrent fatal attacks of acute inflammation of serous membranes has been greatly exaggerated; that they are certainly not to be regarded as consequent complications, and that it is very doubtful whether any relationship exists between Bright's disease and this, unless it be that they may sometimes depend upon a common cause. It may be that intercurrent attacks of such inflammations of a slighter kind do occur, but if so, the evidence must be afforded by the clinical physician rather than by the pathologist, and such evidences I have myself hitherto sought in vain.

V. *Diseases of the Liver.*—I have tabulated the results in regard to four affections of this origin, viz., *fatty degeneration, waxy degeneration, fatty and waxy degeneration combined,* and *cirrhosis.* The *fatty degeneration* is most common in the inflammatory form (25 per cent.), next in the contracting (15 per cent.), and least in the waxy (6 per cent.). The *waxy degeneration* occurs only along with the waxy and combined waxy and inflammatory, and that in 32 per cent. of the former and 23 per cent. of the latter. The *combined waxy and fatty degenerations* occurred also only with the waxy and combined waxy and inflammatory, and was met with in 46 per cent. of the former and 52 per cent. of the latter. *Cirrhosis* coexisted most frequently with the contracting (15 per cent.), somewhat less frequently with the inflammatory (14 per cent.), and in no case with the waxy.

These results strikingly bear out the opinions generally held, that

the liver and the kidneys are frequently the seats of the same lesions, each form of disease in the kidneys being specially associated with a similar affection of the liver. But though such association is frequent, it does not appear that one is the result of the other, rather that both result from a common cause.

VI. *Diseases of the Spleen.*—Two morbid conditions of this organ commonly coexist with Bright's disease, viz., waxy degeneration and thickening of the capsule. The *waxy* degeneration occurred only with the waxy and the combined waxy and inflammatory forms, in 74 per cent. of the former and 88 per cent. of the latter. *Thickening of the capsule* existed only with the contracting cases, and then in 38 per cent.

Of these affections, too, it is evident that they depend upon a common cause, and are not otherwise connected with one another.

VII. *Diseases of the Alimentary Tract.*—Of the affections of this tract I have tabulated only two as being frequent complications of Bright's disease, viz., *waxy degeneration* and *tuberculosis*. The *waxy degeneration*, which is most common in the villi of the small intestine and in the stomach, but occurs by no means infrequently in the large intestine, I have found to accompany only the waxy and combined waxy and inflammatory forms, and that in 58 per cent. of the former and 52 per cent. of the latter. *Tubercular deposit and ulceration*, occurring most commonly in the lower part of the small intestine, but also sometimes in the large, coexists with all the forms, but especially with the waxy (18 per cent.), the waxy and inflammatory combined (17 per cent.). It is somewhat less common with the contracting (15 per cent.), and is most rare with the inflammatory (3 per cent.).

With regard to these, also, it is plain that no special causal relation can exist, unless, perhaps, in the case of the waxy degeneration resulting from tubercle of the intestine. But this I think very doubtful.

VIII. *Diseases of the Brain.*—The affections of this organ which have been most commonly recognised as complications are sanguineous and the so-called serous apoplexies. But I confine my attention to the former class, having found, like most other recent pathologists, that the anatomical conditions which used to be regarded as characteristic of serous apoplexy are commonly met with in diseases accompanied with no apoplectic symptoms, and are not always to be found in cases whose clinical history might lead us to expect them.

Sanguineous apoplexy occurs as a fatal termination in a certain proportion of cases of all the forms. It is most frequent in the

contracting (15 per cent.), next in the inflammatory (7 per cent.), and least in the waxy (2 per cent.).

In relation to the stages, we find that this lesion presents the characteristics of a consequent complication, as is shown in the following table.

TABLE XIII.—*Showing the relations of Apoplexy to the Stages of the different Forms.*

Inflam. form	Stage 1st,	0 per cent.	Waxy form	Stage 1st,	0 per cent.
	„ 2nd,	7·5 „		„ 2nd,	0 „
	„ 3rd,	14·0 „		„ 3rd,	5·0 „

These facts show clearly that as the disease advances the complication becomes more frequent.

From the facts which we have adduced it is evident that among the consequent complications we may reckon with certainty dropsy, which, of course, is more correctly regarded as a symptom than a complication, congestion and œdema of the lung, hypertrophy of the heart, and sanguineous apoplexy. Among causal complications of the waxy and combined waxy and inflammatory forms we may reckon tubercle of the lungs. The others are regarded as results of a cause common to them and Bright's disease, or as slightly connected or unconnected with the renal diseases. I hope one day to add further pathological and clinical information on the question we have here considered.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

HALF-YEARLY CHRONICLE OF PHYSIOLOGY.

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I. BLOOD.

1. PH. OFSIANNIKOF.—*On the Histology of the Blood-corpuscles.* (In the 'Bulletin de l'Academie Impériale de St. Petersburg,' t. viii, p. 561.)

2. MAX SCHULTZE.—*On the Construction of a Microscopic Slide capable of being Heated, and on the Examination of the Blood by its means.* 'Archiv. f. Microscop. Analonin,' Band I.

1. PROFESSOR OFSIANNIKOF believes himself to be justified in concluding from his experiments that the blood-corpuscles of most if not all animals possess an investing membrane or proper cell-wall, which reacts differently from the cell contents when exposed to the action of water, serum and other fluids, and under certain circumstances, possesses a remarkable degree of firmness. The behaviour, however, of different corpuscles taken from the blood of the same animal with these fluids often differs considerably. He has observed that the process of crystallization occurs in the cell contents whilst yet enclosed within the membrane, though sometimes this takes place with more, sometimes with less, facility. He alludes to the variety of forms that have been depicted as the result of the action of certain reagents upon the corpuscles, but declares that he has observed nearly all these forms in the corpuscles whilst swimming in blood-serum. He has found the entire substance of the blood-corpuscles, cell-membrane, contents and nucleus, when this last is present, dissolve after a time completely in serum, water, solution of sugar, and in other fluids. Beautiful crystals may be obtained from (frogs' blood but not from the blood of man), treated with solution of sugar mingled with alcohol.

2. For the particulars of the construction of Max Schultze's slide we must refer to the original, only observing that it consists essentially of two metal arms, extending laterally, to which the flame of a spirit lamp can be applied. The temperature is ascertained by a small thermometer, and is regulated by the part of the arms to which the lamp is applied. One of V. Recklinghausen's moist chambers is an important adjunct.

In examining the blood, the temperature was maintained at about 100° Fah. As regards the *white corpuscles*, Schultze found constantly in his own blood and in that of some of his friends some white corpuscles of smaller size than the red corpuscles, some of equal size, and, lastly, the well-known typical form; in these last the most lively and vigorous movements were perceptible, processes being thrown out which were followed up by the whole corpuscle, exactly as in the movements observed in the Amœbæ. In no instance was he able to determine the existence of a membrane on the surface of these colourless corpuscles, and its absence was also rendered highly probable by the taking up into their interior of certain colouring matters, as carmine and indigo, and of milk-granules. They thus appear to be membraneless cells, and they therefore belong to the same series of cells as the blood-cells of the Invertebrata.

The movements of the colourless cells cease in man after two or three hours at 100° Fahr. If the temperature is much elevated beyond this point they quickly die, but if it be lowered to near 32° Fahr. the movements may continue even for as long as twenty hours.

The *red corpuscles* never exhibited any movements, but when raised to 125° Fahr., at which temperature the white corpuscles were killed, the red underwent a change of form hitherto unnoticed; at first they were unchanged, then indentations occurred at the margins; then roundish fragments, at first with pedicels, detached themselves. The end of all was that in the blood-fluid only small spherical bodies of darker colour remained, the largest being always smaller than an unaltered blood-disc, and the smallest quite a minute granule. Schultze arrives, from his experiments—not only on man, but on the dog, rabbit, calf, and guinea-pig—at the conclusion that the coloured corpuscles of the blood are also destitute of any investing membrane, and he is herein in accord with Rollett and Brücke, but opposed to Ofsianniokof, whose statement has just been given. Schultze found constantly, according to the period of the day, a varying proportion of small spherical red blood-corpuscles, the significance of which is not yet satisfactorily determined. In violent fevers there was a marked tendency in the red discs to become globular.

II. Lymphatics.

1. Leopold Auerbach.—*Researches on Lymph- and Blood-vessels.* ('Virchow's Archiv,' vol. xxxiii, 1865, p. 350.)

2. W. His.—*On the Existence of a Perivascular Canal System in the Central Organs of the Nervous System, and upon its relations to the Lymphatic System.* ('Siebold und Kölliker's Zeitschrift f. Wiss. Zool.,' xv, 1865, p. 127.)

1. Auerbach remarks that, according to the representations of all modern writers, besides the chyle spaces of the intestinal mucous and submucous tissue, there is a superficial subserous network of lymphatics; these communicate by some straight vessels, which traverse the longitudinal muscular coat obliquely. Whilst these statements hold for the frog and many cold-blooded animals, they are incorrect representations of what is really visible in warm-blooded animals.

There is, no doubt, a superficial subserous network of lymphatics; but this is only visible at the point of attachment of the mesentery, and over a space varying from half a millimètre to six millimètres on either side of it. On the remaining and by far the larger portion of the intestine there is no subperitoneal network. At and close to the attachment of the mesentery the vessels dip through the longitudinal muscular layer to communicate with the deeper stratum of lymphatics.

The elongated network of larger trunks, described by authors as subserous, is in reality situated between the longitudinal and circular layers of muscular fibres, and constitutes only the largest collecting trunks of a very much closer network of lymphatics situated at this level, which, again, is only an offset of a still larger system, that everywhere interpenetrates the muscular tissue, and is in connection with the chyle passages. These lymphatics of the muscular layers he has examined in the pig, calf, rabbit, guinea-pig, cat, and dog. He terms them *interfascicular* capillaries of the lymphatics. In the longitudinal muscular tissue there is usually only one layer of these capillaries, but in the circular layer of muscular tissue there are many layers succeeding to one another. They present the usual characters of lymphatics.

The lymphatics from both layer of the muscular coat pour their contents into a network of larger canals, which is extended between the muscular coats in the same fissure or space as that in which the plexus myentericus nervosus is found, and, indeed, always on the inner side of the latter, crossing its trunks and ganglia. This he terms the *interlaminar* network, and states that it may be very easily shown. The tubes vary greatly in size, from 1·7 to 1·6 mm. in diameter, and the larger ones have valves, and, beginning small at the free border of the intestine, course more or less obliquely towards the attached border, with many dilatations and constrictions.

Chyle is often found in the *interlaminar* network which constitutes a medium of communication between the chyle passages of the submucous tissue on the one hand and with the canals of the mesentery which carry off the chyle on the other. Yet, however large and spacious the vessels of this network may be, they only carry off half the entire amount of chyle, the remainder being transmitted through the submucous vessels.

As regards the form and arrangement of the latter network of vessels, he has little to add to the investigations of Teichmann, His, and Frey. In the mucous membrane, as well as in the submucous tissue, he notes, however, that he has usually found the lymphatic vessels clearly *tubular*, with the exception of those which surround the base of the follicles.

In regard to the larger vessels which are formed from the *submucous* networks, they *do not*, as is frequently stated, perforate at certain points the muscular layers. They divide into two sets, of which one is his interlaminar network; the other continues in the submucous tissue from the free to the attached border of the intestine, where it perforates the muscular tissues to reach the serous coat and blends with the interlaminar canals. The trunks so formed at once pass to the mesentery. *It thus appears that there are two layers of lymphatic or rather chyle vessels, differently situated as regards the muscular tissues* (which must have a certain physiological significance), one of them passing between the two muscular tissues and one in the submucous tissue.

As regards the finer structure of the lymphatics, he gives a good historical *résumé* of the different opinions that have been held on the question of the presence of a distinct membranous wall to the finer lymphatic capillaries, some admitting, some denying it, and maintaining that they are only splits or fissures in the tissue.

In his own observations on the finest capillaries of the lymphatics he could only see for a long time certain cell-nuclei present in great numbers, with a double contour and clear contents, a few fine granules, and one or two nucleoli. He then observed that these were not scattered irregularly, but followed certain lines, and by-and-by that they were adherent to a tubular membrane of perfect transparency, the borders only of which (when seen in profile) were visible. By carefully moving the slide cover he sometimes saw folds in this membrane. These appearances alike proved the great flexibility and independency of the membrane and showed also that it was not invested by any other membrane. These vessels presented a most striking analogy to the capillary blood-vessels, but their great and varying diameter and the simple structure of their walls showed conclusively that they were not blood-vessels.

Now, whilst Recklinghausen had described in certain lymph-vessels an epithelial coat, the cells of which were destitute of nuclei, it suddenly struck Auerbach that the scattered nuclei above mentioned might be the nuclei of flat epithelial cells lying in close apposition to one another. Of such a layer, however, no trace was perceptible on simply examining fresh specimens; but he now proceeded to try Recklinghausen's plan of injecting a weak solution of nitrate of silver, and he undertook a fresh series of researches with this object, and on various accounts found the guinea-pig most satisfactory. He then discovered that there were, indeed, epithelial cells lining the tubes; in some the flat cells remained quite clear or presented only a few fine granules, whilst the *nuclei* presented a dark double contour line and distinct nucleoli. In other instances the *cells*

remained clear close to their dark outline, but at a little distance a finely granular brown precipitate formed, becoming denser near centre of cell, and in this dark granular material the nuclei appeared only as a clear, sometimes very sharply limited, spot. Or, lastly, according to the mode of preparation, he observed only a dark granular mass near the centre, apparently consisting of a kind of precipitate. The two last appearances were chiefly seen in the widest lymph spaces, and he considers that they prove that not the whole of these cells are cornified plates, but that in part, at least, they contain in their interior a softer material, more liable to chemical change, and which is chiefly collected around the nuclei. Sometimes two nuclei are seen in a cell. The sinuosity of the walls of the cells is a very remarkable character, and pretty constant, though it varies in degree; smooth and even borders occur only in the widest canals.

Thus, he believes the finest lymphatics are bounded by a layer of nucleated cells, adhering together and in close apposition. He thinks the lines indicating the outlines of the cells may be possibly due to the presence, as Recklinghausen maintains, of an intervening softer material, which takes the silver impregnation better than the cells, but that they more probably indicate the presence of small furrows between neighbouring cells in which the silver deposit accumulates. He alludes to two forms of stomata that have been noticed in the walls of the vessels and denies both.

"In the intestinal walls the cells which have been described unite by their edges to form quite a firm membrane, which *alone* constitutes the wall of the lymphatics, and is not in any way composed or strengthened by the surrounding tissues or by any modification of them, and, in point of fact, is for the most part only loosely adherent to them."

The only exception to this is the tissue forming the substance of the villus which adheres to the walls of the enclosed chyle vessel.

2. He observes that on examining fine sections of the spinal cord from chromic-acid preparations, &c., it will be constantly found to present fissures which every one must have seen, and which have always been referred to breaking down of the tissue during section. But on attentive examination the fissures will be found to present smooth edges, and to be bordered by a layer of condensed tissue. Their course and division are regular, and the same for all sections made from the same cord. In the white substance they pursue for the most part a radial direction from the grey substance outwards, whilst others commencing at the periphery pass inwards; these are connected by smaller transverse fissures. In the grey substance they are less regularly arranged, are shorter and more angular and closer together, especially at the cervix cornu posterior. They recall the lymph fissures of the testicles and of the intestines. On dipping an injection-needle into a fresh calf's cord, and throwing in some injection, he was immediately successful in filling a canal system, whose arrangement on section exactly corresponded with that of the above-mentioned fissures. The grey substance appeared like a

sponge, penetrated by a close network of tubes $\frac{1}{100}'''$ to $\frac{4}{1000}'''$ diam., and the same appearances were shown in the human cord. The radial direction was very well shown. The calibre of the chief branches diminishes quickly in passing outwards from the grey substance, but opens into a close and narrow network of canals. The whole closely resembles the root network of the lymphatic system; but we will first consider its relation to the blood-vessels.

Close examination shows that every fissure is traversed by one blood-vessel, sometimes lying close to the wall, sometimes free on both sides, and so, on transverse section, each vessel is surrounded by a clear space.[1] Frommann, who has seen them, attributes these perivascular spaces to retraction of the tissues. The best mode of displaying them is to inject the blood-vessels in the usual way, and then to inject the spaces by sticking the needle in at random into the texture. The arrangement is also well shown by injecting a nitrate of silver solution either into the vessels or perivascular spaces. All modes of preparation show that the perivascular spaces are constant, and not produced by extravasation nor by collapse of the blood-vessels. The calibre of the spaces is two, three, or even four times greater than that of the contained vessel, and there is no bond of connection between the walls of the vessel and those of the circumscribing space.

Appearances similar to those described above are also to be seen in the brain. The spaces are proportionately larger round the larger vessels than around the smaller. His inquires, what relation does this canal system bear to the lymphatics?

Those accustomed to injections will readily perceive a similarity existing between the perivascular spaces and the lymphatics, and in their size and mode of branching, as well as in the absence of an investing membrane or wall separable from the adjoining tissues, and in the difficulty of exhibiting an epithelial lining. Fohmann and Arnold showed indisputably the presence of lymph-vessels in the pia mater, but no one has succeeded in tracing lymphatics into the substance of the brain. Are the perivascular spaces, then, continuous with the lymphatics of the pia mater?

In answer to this question His shows that the lymphatics of the pia mater can be injected *from* the perivascular spaces of the brain, and the union is effected by a wide lacunar system separating the brain from the pia mater. He believes there is no question that the perivascular spaces are the lymphatics of these parts of the nervous system. He thinks that, on the one hand, these perivascular spaces act as reservoirs for the nutritious fluids, and, on the other, serve as organs protecting these important parts from pressure, serving the same purpose for each individual segment of the nervous system that the liquor cerebro-spinalis does for the whole collectively.

[1] See also John Dean Smithsonian, 'Contrib. to Knowl.,' 1864, pl. v, vi, and viii.

Nervous System Muscle.

1. Dr. Theodor Korner.—*Anatomical and Physiological Inquiries respecting the Motor Nerves of the Uterus.* ('Heidenhain's Studien des Physiolog. Instituts zu Breslau,' 1865, p. 1.)

2. R. Heidenhain.—*On the influence of the Nervus Accessorius on the Movements of the Heart.* ('Heidenhain's Studien des Physiolog. Instituts zu Breslau,' 1865, p. 109.)

3. Dr. E. F. M. Pflüger—*Critical and Experimental Investigations on the Theory of the Inhibitory Nerves.* ('Untersuchungen aus dem Physiolog. Institut zu Bonn,' herausgegeben von Dr. Pflüger.)

1. Dr. Körner observes, that the uterus in the human subject is supplied by two sets of nerves; one, proceeding from the inferior mesenteric ganglion of the sympathetic, the other proceeding from the middle sacral nerves. These two sets unite in a plexus, the branches of which are distributed over the rectum, bladder, upper half of the vagina, and the portio vaginalis uteri. Numerous ganglia, the presence of which has been alternately admitted and rejected, are clearly visible in various parts of the uterine structure, when this has been treated with vinegar, and a weak solution of bichromate of potash.

Dr. Körner's physiological experiments upon the above nerves were chiefly undertaken in rabbits, in which animals he found the distribution of the nerves to agree in all essential respects with that of the human subject. The chief results at which he appears to have arrived, after a course of inquiry extended over a considerable period of time, are:

1. That electrical excitation of the branches of the sacral nerves distributed to the uterus are constantly followed by contractions of that organ. 2. That electrical excitation of the branches of the sympathetic nerve distributed upon and embracing the aorta, is also constantly followed by contraction of the uterus. On the other hand, section of these two sets of nerves renders it impossible to excite uterine contractions by electrical excitation of the spinal cord. These two sets of branches must therefore be regarded as the only motor nerves of the uterus. 3. Irritation by electricity of any portion of the spinal cord is followed by uterine contractions, providing only that the motor nerves distributed to the uterus are uninjured; and contractions are induced with greater facility in proportion as the central part of the lumbar region of the spinal cord is approximated, which is to be considered as the point best adapted for the induction of uterine contractions by electrical excitation. 4. Each of the two above-mentioned sets of nerves—the sacral spinal and the sympathetic—possess motor branches for the muscular tissue of the uterus proper to themselves, for if either of them be divided uterine contractions can still be occasioned by applying electrical excitation to the other, though in the case of the sympathetic branches the

agency exerted seems to be an inhibitory or regulatory one, bringing movements already commenced to a stand-still. 5. The sacral spinal branches for the supply of the uterus leave the spinal cord in the region intermediate to the third and fourth lumbar vertebræ. The uterine branches of the sympathetic, on the other hand, leave the cord at about the level of the last dorsal vertebra, as is shown by the fact, that on section of the spinal cord above this point electrical irritation of the upper segment is no longer followed by uterine contractions.

As regards the presence of a central cerebral nucleus for the excitation of uterine contractions, Körner's experiments do not lead to the supposition that any such exists, but he found that contractions of the uterus could be induced by electrical stimulation of the medulla oblongata, cerebellum, pons varolii, corpora quadrigemina, crura cerebri, corpus callosum, optic thalamus, and corpus striatum, but that the contractions were more easily called forth in proportion as the point irritated approximated the medulla oblongata.

2. Heidenhain commences his paper by observing that without entering into a discussion of the whole subject of the existence or non-existence of inhibitory or regulatory nerves, he is desirous of recording a few observations which, whilst they appear to him to militate against the exhaustion theory promulgated by Schiff, yet seems to favour the view that the pneumogastric nerves are not inhibitory but motor nerves, so far as regards the heart. He describes the rough anatomy of the roots of the pneumogastric and spinal accessory nerves in the rabbit, and the relations of their trunks to one another. He states that the accessories in rabbits can be torn out by the roots with the greatest facility, whilst this is more difficult to accomplish in dogs, in consequence of the fibrous nature of the passage by which it leaves the skull in those animals. The accessory nerves are doubtless the special motor nerves of the larynx. The effect of their evulsion was long ago stated by Bernard to be paralysis of the larynx, in reference to its phonetic attributes only, but Heidenhain agrees with Schiff in thinking that the paralysing influence is exerted upon the respiratory functions of the larynx as well as upon those connected with the voice. Thus one of the earlier consequences of paralysis of the larynx is, not unfrequently, death by the entrance of food into the air-passages. If the animal survives the operation for some time, it often dies from pulmonary inflammation, also occasioned by the entrance of particles of the food into the bronchi, whilst in other instances pleurisy or pericarditis supervenes.

The frequency and rhythm of the respiratory acts is unaltered after evulsion of the accessory nerves, nor do the movements of the stomach and alimentary canal seem to be in any way interfered with, provided that a branch to the œsophagus given off from the pneumogastric very high up, is not damaged.

Heidenhain holds that the inhibitory nerves of the heart contained in the trunk of the pneumogastrics primarily originate in the accessory nerves, since he has found that a few days after evulsion of the

accessory nerves from the jugular foramen, the excitation of the pneumogastric nerve is not followed by its wonted inhibitory effect. He has observed also, that evulsion of the accessory nerves is constantly followed by increased frequency in the beats of the heart, unless only in those cases where the rapidity of the heart's action before the operation was very considerable. The deep origin of the inhibitory nerves existing in the trunk of the spinal accessory nerve appears to be from the medulla oblongata, and not from the spinal cord. In opposition to the results obtained by Schiff, Heidenhain finds that section of the pneumogastric nerves, after previous evulsion of the spinal accessory nerves, is constantly followed by a diminution in the frequency of the beats of the heart, which he attributes to the derangement of the respiratory mechanism consequent on the former lesion. On the whole, therefore, he concludes that the inhibitory nerves of the heart contained in the vagus, originally proceed from the nervus accessorius Willisii, for, on the one hand, the stoppage of the heart's action, induced by excitation of the pneumogastric is dependent on the integrity of the accessory fibres, and, on the other hand, evulsion of the accessorius is followed by increased frequency of the pulse, and if the pneumogastrics are afterwards divided, the frequency of the heart's beats may be diminished in consequence of embarrassment of the respiratory mechanism. If, under normal circumstances, the vagi of an animal are divided, this lesion operates in two opposite directions upon the rapidity of the heart's action ; for the division of the accessory fibres contained in the pneumogastric trunk occasions an acceleration, whilst the division of the proper vagal fibres induces a retardation of the cardiac beats; and usually, as is well known, the former influence preponderates—the pulse rising in frequency.

3. Pflüger commences his paper by observing, that all the facts which Schiff and Moleschott believe they have established against Pflüger's doctrine of the inhibitory influence of the vagus upon the heart are illusory. Schiff, it is well known, considers that by extremely feeble stimulation of the vagus the frequency of the beats of the heart may be increased, but that if the exciting electrical force be increased, even to a very moderate amount, the vagus becomes exhausted, and is no longer capable of transmitting the normal motor energy to the heart. Schiff acknowledges the variation of frequency under gentle stimulation to be very slight, indeed scarcely more than occurs under ordinary circumstances, without stimulation. Now, in reply to these statements, Pflüger first adduces a series of experiments, in which he shows that with extremely feeble currents applied to the vagus of the frog either no variation takes place in the heart's frequency or the number of beats is absolutely diminished.

Again, Schiff maintains that —1. It is the accessorius whose stimulation really induces the stoppage of the heart's action when a current of moderate force is applied to the vagus. 2. That paralysis of the accessorius causes no increase in the number of the beats of the heart. But that—3. The division of the vagus in the neck occasions

the increased frequency of the pulse, as has been so often observed. Pflüger remarks on this, that the very recent experiments by Heidenhain (above recorded) have shown that the tearing out of the accessorius occasions an increase in the frequency of the pulse, whilst, according to Schiff, paralysis of the accessorius has no influence on the heart. Heidenhain has also shown that after tearing out the accessorius, at which time irritation of the trunk of the vagus occasions no increase of the heart's beats, section of the vagi does not, as Schiff contends, produce an acceleration but a *diminution* in the number of the beats. Heidenhain explains this, by supposing that an indirect influence is exerted upon the heart through the deficient play of the lungs, but Pflüger thinks it reasonable to suppose that when the accessorius was torn out the vagus also suffered. He then enters into a critical examination of Moleschott's experiments, in which electrical, chemical, and mechanical stimuli were applied, and points out various sources of error.

Moleschott's conclusions from his experiments were—

"1. That weak excitation of the sympathetic causes increased frequency of the pulse. 2. That this effect is not of a reflex character. 3. That strong electrical stimulation applied to the sympathetic can bring the heart to rest."

Of these propositions Pflüger considers the first two to be insufficiently proved, and the last to be absolutely erroneous. The conclusions at which Pflüger has arrived after repeating and variously modifying his former experiments, are, that the very first effect which weak currents of electricity transmitted through the vagus nerves effect, is a prolongation of the *diastole* of the heart, and that, under no circumstances, is the rapidity of the heart's action primarily increased by such stimulation, whether the electrical stimulus be strong or weak. He has also investigated again the action of the splanchnic nerves, when irritated, on the peristaltic movements of the intestines, and does not hesitate to pronounce as erroneous the statement of Schiff that it increases their rapidity and energy. He regards them as being essentially the motor nerves of the arteries distributed to the intestines.

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

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Tobacco as an Antidote to Strychnia.—Dr. Norman Chevers, senior physician to the Calcutta Medical College Hospital, reports a case of strychnine-poisoning in which tobacco was used with effect as an antidote.

The patient, a Mahomedan girl, æt. 11, was admitted into the First Physician's Ward in the evening of the 15th of March, 1865, at 5 p.m., with the symptoms of poisoning by strychnia.

From the statement of the patient, it appeared that her husband, who was a dispenser in the hospital, had kept in his house about three grains of strychnia, and had warned her to be particularly careful of the powder. On the day of her admission, the man had a quarrel with her, and struck her, on which she determined to put an end to her life. At 1 o'clock she took her dinner as usual, and about 2 she took the drug and put it into her mouth, chewing the crystals. She found the drug to be anything but agreeable to the taste, and spat out, as she believed, the whole of it. Immediately afterwards, instead of washing her mouth, she swallowed some water to get rid of the intensely bitter taste which had been left behind. Within half an hour the girl began to suffer much from sickness, and a burning sensation about her throat and stomach. This was soon followed by marked convulsive fits. She had about five strong tetanic convulsions before admission into the hospital. Immediately the husband ascertained the cause of the attacks, he administered sulphate of zinc and mustard emetics. She was brought to the hospital about three hours after the accident. The report on admission was—"Countenance anxious; skin warm; pulse quick; pupils dilated; respiration easy; heart's action considerably accelerated, and fluttering against the wall of the thorax. She complains of a burning and suffocating sensation about the throat and chest; the extremities are stretched out, the hands grasping firmly the sides of the bed, legs apart, feet everted; no frequent spasms."

Shortly after her admission she had a severe tetanic convulsion, involving nearly all the muscles of the body and extremities. The chest was fixed; respiration became difficult; the body was bent backwards (opisthotonos). This was followed by an interval of rest and relaxation of all the muscles, but the patient seemed to be much prostrated. Slight convulsive starts and twitchings of the hands and feet followed at various intervals. After this attack the patient became so sensitive, that the slightest touch or movement of the bed, or a sudden noise, would cause a spasmodic jerking of the whole body. A free mustard emetic and a cathartic enema were given

upon admission. The symptoms continued much the same as before.

Dr. Chevers saw the patient soon after the emetic had been taken. Pure animal charcoal mixed with melted lard was given in large quantities. Immediately after this she began to take, after each tetanic fit, small doses of an infusion of tobacco, one drachm to the pint. The first dose was twenty minims; the second, twenty-five; the third, at 6 p.m., thirty minims; the fourth, at 6.52 p.m., thirty minims; at 6.54, the same; at 6.57, the same; and at 6.58, the same.

At 7 p.m. the patient had so severe a tetanic convulsion, as to leave it for a time doubtful whether she would live through it: it lasted for six minutes, and a full dose of the infusion was administered. At 7.7, there being some twitching of the hands and feet, a dose of fifteen minims of the infusion was given; the same at 7.10 and 7.16; at 7.25, twenty-five minims; at 7.32, twenty; at 7.45, twelve; and at 7.50, thirteen. At 8 p.m. the patient vomited, which fact Dr. Chevers took as evidence the tobacco was acting. The vomited matter consisted of dholl and rice mixed up with some animal charcoal. The breathing was easy; the pulse 144.

From this time the patient began to recover, but the stomach continued to be very irritable. On the 18th of March, after taking an emetic, she vomited a lump of carbon and lard of the size of an olive; and afterwards, about an ounce more of the same matter in small particles, after the administration of the emetic. On the 24th of March she was discharged well.

In commenting on this case, Dr. Chevers states that the poison took full possession of the nervous system; that the contents of the stomach were never thoroughly evacuated as long as tetanic symptoms lasted; and that recovery was wholly due to the antidotal action of tobacco. He employed the antidote on the suggestion afforded by a case related by Dr. O'Reilly, in which a man who had swallowed six grains of strychnia took upwards of an ounce of infusion of tobacco-leaves in small doses at intervals, and recovered. He had previously taken an emetic, which had caused copious vomiting.

The quantity of infusion of tobacco swallowed by this child, viz.:—at least 355 minims or nearly five fluid drachms of infusion of a drachm to a pint of boiling water,—could not have been safely taken (Dr. Chevers thinks) within three hours, and the repeated doses could not so long have failed to produce vomiting or other evidence of nicotism, had the power of this agent not been met and for a long time resisted by that diametrically opposite condition of the nervous system which strychnia gives rise to. The case appears to confirm the view that strychnia does not act as a direct excitant of the nervous system, but that it exaggerates excessively the reflex functions of the spinal cord, so that the slightest irritation produces tetanic convulsion. It appears to produce augmentation of the vitality of the spinal cord in two ways:—1st, by increasing the amount of blood in the spinal cord, by paralysing the muscular

coats of the vessels which supply it; and, 2nd, by acting in a special manner on the tissues of the cord. Nicotine acts as an antidote, Dr. Chevers thinks, by exerting a reverse action upon the vessels through the great sympathetic system.—*The Indian Annals of Medical Science*, No. XX, August, 1866.

Poisoning by Strychnine.—A case of poisoning by strychnine has occurred recently in New Zealand, and has led to a double trial. At the second trial Mr. J. Kirkland gave evidence as an expert. The prisoner, Captain Jarvis, was accused of administering strychnine to his wife on the 26th of September, 1864. The most important feature in the trial, in a medico-legal point of view, had reference to the cross-examination of Mr. Kirkland. He gave evidence that crystals of quinine could be distinguished from crystals of strychnine by their shape: "the crystals of quinine are needle-shaped; the crystals of strychnine are rhombic prisms, needle-shaped or circular, but not square, like quinine." If there were aniline in the contents of the stomach, he believed it would be eliminated by Stas' process for the detection of strychnine. He was asked the question whether the decomposition of the human body might re-arrange the elements into a substance that would produce the same play of colours as is presented when strychnine is exposed to oxidizing agents. To this question he replied, that he had examined the tissues of a dog that had been buried three months, and that no such play of colours was observed. Mr. Kirkland also stated his belief that the 20,000th part of a grain of strychnine could be discovered in the contents of the human stomach by Rodgers and Girdwood's process. At the same time, he admitted that the colour-tests were not to be trusted alone, as "hundreds of other substances will produce them." He relied on three tests—colour-tests, taste, and physiological action. In respect to the difference of bitterness between quinine and strychnine, Mr. Kirkland stated he could distinguish the two substances in solution by their taste:—"The bitter of quinine is a pure bitter; that of strychnine is acid, and produces a peculiar sensation in the back of the palate, which quinine certainly does not." The Judge, in summing up, defended the scientific evidence from the attacks of the prisoner's counsel. The Judge observed—"I say to you that scientific facts cannot be judged of by common-sense without special knowledge and preparation; and that when Mr. Smith (the counsel for the prisoner) appeals to the common-sense of uninstructed men, he is appealing to an incompetent tribunal so far as those facts are concerned." The prisoner was found guilty.—*Australian Medical Journal*, October, 1865.

The Action of various Compounds of Amyl.—We have ourselves been engaged in the study of the amyl compounds on behalf of the British Association for the Advancement of Science. We have investigated the action of the hydruret of amyl; the acetate, the iodide, the amylic alcohol, and the nitrite of amyl. Of all these compounds the nitrite is the most potent. The following is a summary of the facts as yet discovered in regard to it:

"1. It is absorbed by the bodies of animals, however introduced into the organism—by the skin, by the stomach, by the lungs, by the cellular tissue.

"2. After its absorption, its effects are seen immediately on the heart and circulation: there is in the first instance violent action of the heart, with dilatation of the capillaries, followed by diminished but not extinguished power of the heart and contraction of the extreme vessels. As an excitant of vascular action, the nitrite of amyl may be considered the most powerful agent as yet physiologically discovered.

"3. On animals, such as frogs, whose bodies admit of its removal spontaneously, and whose circulatory and respiratory systems are simple, the nitrite suspends animation, and when the animals are placed under favorable conditions for the process of recovery, they may recover after considerable periods of time. There is no other known substance that suspends animation in these animals for so long a period. On warm-blooded animals, which are clothed in thick and less penetrable skin, and in whose bodies the circulatory and respiratory systems are more complicated, the nitrite cannot actually stop the movements of respiration and circulation without destroying life. But even in these animals it can, without destroying life, reduce the forces of respiration and circulation so extremely, that a condition precisely analogous to what is known as *trance* or *catalepsy* in the human subject can be brought on and sustained for many hours.

"4. The nitrite of amyl is not an anæsthetic. By it consciousness is never destroyed, unless a condition approaching to death be produced; and when this occurs there is rarely any recovery. The animal passes into actual death.

"5. The effects of the nitrite on the organism are directed to the motive force, which it first wildly excites and then subdues.

"6. Physically, the nitrite holds a place between the volatile bodies, such as chloroform, and the solid bodies, such as opium and woorali. Hence its effects are less evanescent than those of the very volatile substances, and less certainly destructive than the solid substances. In this lies the secret of its prolonged action.

"7. In cases where the nitrite of amyl is carried to its extremest effects, the appearances of the internal organs present some modifications. The appearances are not the same in every instance, but vary according to the mode in which the substance is administered. If it be administered very quickly, the lungs and all the other organs are found blanched and free of blood, the right side of the heart is engorged with blood, and the left side is empty, the brain being free of congestion. If the substance be administered slowly, the lungs are congested, the brain is congested, and blood is found both on the right and left sides of the heart. The organs of the body are also of a dirty reddish-brown colour, and the blood is similarly discoloured, no distinction in colour existing between the arterial and venous bloods. Notwithstanding the violent action of the heart, I have never seen rupture of any vessel nor extravasation of blood. The inner lining of the blood-vessels is unchanged, and the valvular

mechanism of the heart maintains its integrity. It is to be remembered that these observations have all been made on healthy animals."

We have further, on the part of the Association, studied the effects of other amyl compounds, and have arrived at some conclusions respecting them.

Of the substance known as *Amylene*, we have found that when it is administered in an extreme dose it produces insensibility, but that, during this period of its action, there is in progress a kind of consciousness, leading the individual to perform certain acts and deeds which, after he has awakened from his sleep, are quite forgotten: —are not even remembered as a dream, but yet were performed with precision, and, it might be, in obedience to a preconceived or preconcerted plan.

Of *Amylic Alcohol*, we have shown that when animals or men inhale it, it sets up a peculiar class of symptoms which last for many hours, and from which it would seem the animal could not possibly recover, but from which it does always recover. The symptoms are those of muscular paralysis, with paroxysms of tremulous convulsion; which paroxysms can be excited at any moment by merely touching the animal, breathing upon it, or otherwise subjecting it to trifling excitation.

Of the *Iodide of Amyl*, we have shown that animals placed under its influence, whilst they present all the symptoms similar to those produced when they are treated with amylic alcohol, have one symptom superadded, *i. e.* the animal, when it is stimulated to move, or when in the course of its recovery it commences to move by its own volition, moves for a considerable period of time in a circle, and gains perfect control of its own actions by a gradual divergence, and, as it were, a spreading out from the centre of the circle. The iodide of amyl does not produce insensibility to pain.

Of the *Acetate of Amyl*, we have proved that it exerts an influence similar, but not so marked in all its stages as the nitrite and iodide.

Of the whole series, we have shown that they preserve in a fresh state animal and vegetable matters; that blood, which of all animal substances is most ready to decompose, can be kept free from organic destruction by them; and that while they change the colours and destroy the beauty of flowers, they prevent the indications of putrefaction.

From the phenomena observed, we have ventured to draw certain physiological inferences which may be very briefly recapitulated.

1. We infer that, excepting amylene, no body of the series can be considered as an anæsthetic: in other words, the amyls do not, like the compounds of formyle (chloroform and its allies), produce general insensibility to pain.

2. From the action of these substances on the organism, and from the fact that they are products derived and derivable from the oxidation of starchy matter, we think it possible they may be the organic blood-poisons on which some human maladies depend, viz., cata-

lepsy, hysteria, somnambulism, and convulsive affections resembling epilepsy.

3. Considered as poisons, we look on the amyl series of bodies as analogous in action to strychnine, but owing to their readier escape from the body, they are less fatal, except when the dose first given is so large as to be immediately fatal.—*Transactions of the British Association for the Advancement of Science*—1864, 1865, 1866.

Tests for Curare.—The essays of the past year yield excellent and instructive lessons on curare, a substance which in England has been used in the treatment of tetanus, but which of late years has received but little attention either by experimentalists or practitioners. The first essay to which we would call attention is by MM. Auguste Voisin and Henri Liouville, who have studied the substance with the special object of determining the tests for its detection.

From various chemical researches and experiments, they decided first that the elimination of the poison was by the urine. Their experiments were made upon animals with the urine of subjects which had previously been treated by curare, and although the results were satisfactory, they were still variable, according to the period of the urinary secretion and the dose given of the poison, which might have been either too small to admit of detection (and in that case could not have been mortal), or so large that the subject would have succumbed before the poison had had time to penetrate into the kidneys and bladder. It was thus thought necessary to obtain more reliable proofs, and with this view experiments were made on the viscera by MM. Voisin and Liouville. The specimen of the poison used by them was that known as "*Curare E. Carrey*," from the name of the traveller and distinguished writer who first introduced it. This curare is brought from the shores of the Amazon in little earthenware pots; when triturated, it gives a very powerful odour, similar to that of the poisonous solandra. The dose for a rabbit is three milligrammes; but although this would prove fatal to the rabbit, it would not have any effect on an adult man, for whom the therapeutic dose of this curare would be fifteen centigrammes.

To arrive at any just conclusion it was deemed necessary to administer to an animal a dose which would bear some equality with that from which a man would feel any serious results, and for this purpose two rabbits were killed. One was destroyed by a sub-cutaneous injection of thirteen centigrammes, and the other was killed, more slowly, by ten centigrammes, ligatures with roller bands being used to retard the absorption of the poison. The visceral organs of the rabbits were separately heated in capsules over a hot-water bath; and when these organs had become desiccated, the residue was dissolved in alcohol at 95°, and dried over the bath; again it was washed with alcohol at 95°, and filtered. The residue weighed from five to seven grammes, and was perfectly transparent and of the colour of mahogany.

MM. Voisin and Liouville then treated some frogs and rabbits

by the above-mentioned process, and the result of the experiments on the viscera of the animals (the liver, kidneys, spleen, lungs, and heart) was the production of a clear residue.

1. *Physiological reagents.*—For curare alone recourse must be had to the physiological re-agent. Experiments should be made upon various animals—dogs, rabbits, frogs—and the method of sub-cutaneous injection should be employed. If the animal under experiment (having received by this process a part or the whole of the residue) offers phenomena analogous to those of an animal *poisoned* by curare, it will present immovability and inability of movement, or at least great difficulty, showing a paralysis which arises almost immediately in the hinder limbs; then shiverings and slight general clonic convulsions; diffused undulatory tremblings successively traversing every part of the body; afterwards a still greater immobility, shown by determined continuance in the position in which the body was placed and a general quietness; great acceleration of the inspirations and pulsations of the heart, which become irregular; finally, if the animal must die, these phenomena are exaggerated, and double exophthalmia comes on, injection of the conjunctiva, hypersecretion of tears, sudden and extreme variations of the pupils, increase of central heat (rectum and vagina), and peripheric heat (ears and face), and death by cessation of the respiration. The autopsy in this case will show that the heart continues to beat; while electricity will disclose in the greater number of small animals and especially in frogs, that the motor nerves are no longer capable of being excited by the electric current, the muscles of this department having maintained, however, their contractile force.

If the animal, although infected by a residue, does not die (and that will depend upon the quantity of curare found in the residue, and by the resistance used by the animal under experiment), then the phenomena described above will not increase in intensity, and will last longer; the animal will recover by degrees, the use of his limbs will be restored, more frequently, at the end of two or three hours, and no return of symptoms will be exhibited. MM. Vulpian and Pelikan remarked in the larger animals upon which *they* experimented, that the sensitive nerves in every case were always found intact.

2. *Chemical reagents.*—Little can be said respecting the use of chemical agents for discovering the presence of curare. Experiments have been made with a view to the detection of sugar, one of the salient points of curaric intoxication, and which ought always to be inquired into. Sugar has been found in the urine of invalids who have been treated by curare, either by sub-cutaneous injection or the enema. The presence of sugar is only discoverable for about two hours after the introduction of the curare. Amongst the chemical re-agents already known, it is asserted that curarine, the active principle of curare, ought to take the following colours:

1. A blue colour with pure and concentrated sulphuric acid. This valuable characteristic, when it exists, clearly distinguishes curarine from strychnine.

2. A purple colour with nitric acid.
3. A violet colour with bichromate of potash with a little sulphuric acid. But these two last characteristics are common to curarine and strychnine.

If chemistry shows, in a residue, reactions common to curarine and to strychnine, the injection of the residue into an animal gives symptoms of a scientific certainty sufficient to carry conviction of the nature of the poison.

Thus, at the present, the living physiological reagent is still, in the search after curare, the best means a medico-legal observer can have in his hands for the purposes of justice.—*Annales d'Hygiène Publique*, Tome xxvi, 1866.

Elimination of Curarine.—Professor Lussana, of Parma, in speaking of the effect of curare, discusses the question, why this substance should act so much more energetically when it is introduced by the method of subcutaneous injection, compared with the mode of introducing it by the alimentary system. He attributes the difference to the circumstance, that when the curare is taken by the mouth, it passes after going through the stomach into the liver and escapes with the bile without ever fully intermingling with the blood-current. When, however, the substance is directly injected into the tissues, it pervades the whole of the blood, and directly destroys life.

This might be held to be merely an ingenious theory ; but Lussana has an experiment which renders the suggestion nearly demonstrative. He was able, in the bile of a hound which had taken curare, to discover curarine, showing that the substance passes into the bile. The injection of a minute dose of curarine into a mesenteric vein of an animal was followed by no effect; the injection of a similar small quantity into the veins of the thigh of an animal produced instant death. The author further opines that the different effects of curarine on different animals depends in some degree on the more or less direct connection of the vena porta and vena cava by anastomostic communication. In all cases, however, the separation of curarine through the liver is effected slowly and in minute proportions; so that, if a large dose be injected even by a mesenteric vein, death immediately ensues.

Lussana does not think it justifiable to introduce curarine into the list of therapeutical substances until its action and properties are more fully understood. At the same time, he admits that the success of the agent in the treatment of tetanus is encouraging.—*Schmidt's Jahrbücher*, Band cxxxi, 1866.

Part II.—Hygiene.

On the Causes of Mortality in Maternity Hospitals. — Dr. G. Lauth, of Strasbourg, has an excellent article founded on the work of Dr. Léon Le Fort. The positions taken by Dr. Le Fort are the following:

1. Women confined in hospitals and *maternités* not only die in them in greater numbers, but also in a proportion unusually large as compared with the number of those who are confined in their own dwellings.

2. The cause of this frightful mortality is to be attributed to puerperal fever, and it is by contagion that this destructive scourge exercises its ravages.

3. It is absolutely necessary to adopt important hygienic measures; and if it be impossible to prevent the disease from breaking out, it is possible to place barriers in its way, and to say, "Thou shalt go no further."

Dr. Lauth, in commenting on these various points, notes specially the causes of the mortality in maternity hospitals. He admits a special hospital influence; he annuls the effect of social position; but he thinks the psychical condition of the women may play some part. He dwells on the fact, that lying-in hospitals receive the most serious cases, and that there are more operations performed in the hospitals than in the towns; and these are facts to be remembered. He also contends that although obstetric operations increase the mortality, such increase is slight. On the whole, he comes to the following conclusions:

As it has been proved that in hospitals and *maternités* the mortality is beyond all proportion as compared with the mortality among women confined in their homes; as this difference is not *en rapport* with the diverse influences which have been passed in review, and as this shows itself everywhere in hospital establishments, it is in them that the principal cause of the mortality must be sought: this cause is *puerperal fever*.

On the subject of puerperal fever, Dr. Lauth's analysis of Dr. Le Fort's work abounds in interest. Allowing the direct agency of contagion as the prime cause of the malady, certain remarkable influences facilitating the development of the disease are stated, of which we must be content to notice one or two points.

In the first place, Dr. Lauth questions the influence of extreme cold as a disposing cause. He admits that the disease, as shown by Dr. Le Fort, is most prevalent and fatal in winter, but attributes this not so much to the cold itself as to deficient aëration incident to the closing of rooms and wards; and he shows that the fever is not influenced by concomitant epidemics. But the points on which he dwells chiefly are these: to ascertain the degree of influence which a long or short residence in hospital previous to *accouchement* exercises on the health of the patient, and how far this residence procures for the patients an immunity as regards puerperal fever. With a view of determining these points, Dr. Lauth gives the results of the researches of MM. Charrier, Lasserre, and Späth on this subject. M. Lasserre had long defended the cause of acclimatisation. His tables show that after a stay of more than eight days in hospital previous to confinement, there were 18 deaths out of 791 women; after a stay of less than eight days, 17 deaths out of 528

women; and of those who entered while in labour, there were 52 deaths out of 1020 women.

Dr. Charrier obtained an analogous result. Out of 1868 women who were confined during the ten days following their admission, 120 of them died, or 1 in 15; out of 351 who had resided in hospital more than ten days previous to their confinement, 9 only died, or 1 in 38.

M. Späth's researches in this matter among the midwives of Vienna produce results nearly similar, viz.:

Duration of stay before confinement.	Patients per cent.
From 2 to 7 days	25·4
„ 8 to 14 „	28·5
„ 15 to 21 „	20·3
„ 22 to 28 „	17·2
More than 28 „	19·1

Thus, says M. Le Fort, "if we except those women who arrive after confinement and those received during labour, a great diminution of mortality is seen from acclimatisation; and it is remarkable, that out of 90 women confined at home, but brought to hospital more or less directly after *accouchement*, one only was attacked by the malady. We are, therefore, led on to the inquiry, whether contamination does not specially and almost solely take place at the time of accouchement.

Two other circumstances exercising a marked influence on the spreading of this scourge in lying-in hospitals are in, M. Le Fort's opinion, agglomeration and overcrowding. He repudiates the idea of wishing to apply to lying-in hospitals what he had said relative to the fatal influence of large hospitals generally on the health of the patients, but considers the development of the disease facilitated by the reunion of large numbers of women in the same hospitals. Hence his preference for small establishments for this class of patients. As regards overcrowding, M. Le Fort considers that too many precautions cannot be taken for guarding against it, and he quotes the opinion of M. Delore on the subject. He (Delore) says—Overcrowding, it is well known, does not always manifest its fatal influence at the moment it is being exercised in the greatest degree; sometimes a long period elapses. It will be said that germs, after being somewhere deposited, require incubation to be hatched and for their presence to be revealed. In a surgical ward, moreover, he has frequently remarked that it was not at the time when it contained the largest number of extra beds that erysipelas epidemics broke out, but some time afterwards. The remembrance of this insufficiently considered observation may prevent the surprise so frequently felt when an observer is unable to explain the cause of the occurrence of fatal accidents at a time when there seemed nothing to fear.—*Annales d'Hygiène Publique*, October, 1866.

CHRONICLE OF MICROLOGY.

By J. F. Streatfeild, F.R.C.S.,

Assistant-Surgeon to the Royal London Ophthalmic Hospital, and to the Eye Infirmary of University College Hospital, &c.

Part I.—Physiological Micrology.

Salivary Secretion.—Schlüter, in the secretion of the submaxillary gland of the dog, obtained by nervous irritation, found, besides numerous faintly reflecting, acetic acid resisting, droplets of 0·003—0·046 mm. diameter, two different forms of saliva-corpuscles, one pale, and moving in the manner of the amœba, processes pushed out and drawn in, the other globular and granular with molecular motion within. The author could change the corpuscles of the first kind by addition of his own spittle or distilled water into those of the second kind, the familiar saliva-corpuscles out of the mingled saliva, and considered these changes as following a coagulation of the living protoplasma. Then the saliva discharged at the commencement of the investigation contained but few corpuscles, but, during the irritation, the number of the latter continually increased, so he derives the origin of the corpuscles from the nerve excitation; this shall first bring about increase of the nuclei (by division), and then division of the protoplasma (by contraction). A granular shapeless mass, which is found in the first place in the discharged secretion, he considers as a detritus proceeding from the accumulated destroyed saliva-corpuscles.—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxvii, part 1, p. 16.

On the Movements of the Spermatozoa.—Grohe, in investigating this subject, made observations, in the first place, on human spermatozoa which were contained in the fluid of a hydrocele evacuated by puncture; he was able, later on, to corroborate them in the spermatozoa of different mammals, especially in those of the guinea-pig; only the vesicular elevations at the upper end of the tail during the contraction were not again discoverable in the spermatozoa of the animals. In all cases the movement first began in the head and in the upper part of the tail, and thence extended to the free extremity. Locomotion followed if the first excitation was strong enough—only an oscillation or contraction in a weaker degree—whereas the free end often remained quite passive. The spermatozoa after drying up revived again with sugar or salt solutions; thus, a contracted movement in the head and the upper part of the tail occurs in the first place, as of a lever, whilst the lower part of the tail yet holds fast to the glass and is altogether immovable.

The author finds a confirmation of his view in the fact that, before maturity, the seminal elements of the *Rana temporaria*—the staff-like and cylindrical heads without tail or with imperfectly

developed tail, which may be stripped off from the section surface of the testicle—show by addition of distilled water the most heterogeneous conditions of contraction; they bend themselves in different degrees into a horse-shoe, sausage, or hook shape, and roll thereat, ever according to the intensity of contraction, at one time more quickly, at another more slowly, according to the different directions. The spiral windings of the head of the spermatozoa of the bird the author reduces to similar contractions, and the manifold nuclei of the so-called cysts, out of which, according to Wagner's and Kölliker's statements, the spermatozoa are said to proceed, are to him also only particles of contractile substance, which he thinks he must separate from ordinary cell-nuclei, "on account of their uniform fatty lustre and their superfluous nature." The cross streaks in the head of many spermatozoa he considers to be the expression of an unequal distribution of the contractile substance. As to the chemical analogies which the relation of the substance of the spermatozoa shows to the muscular substance, and which certainly among albuminous bodies never is wanting, the author himself seems to lay no great stress. He has taken trouble to find some way to make plainer the hardly perceptible division of the envelope and the contents of the spermatozoa, and for this purpose he recommends oblique illumination and colouring with aniline. Then, not only the head, but the tail also appears, surrounded by a bright colourless zone, after the aniline colouring, whilst in the axis of the tail the contractile substance passes off as a dark red streak.—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxvii, part 1, pp. 18, 19.

The Movements, called Amœboïd, observed particularly in the Blood.—MM. Hayem and Henocque contribute several papers on this subject, which are very much too extensive for a summary notice. They remark that "Blood and lymph do not only exhibit elements endowed with the amœboïd movements, which may be observed in different secretions, such as mucus, normal or pathological serosities, but the elements in which it exists have almost always the character of *leucocytes*; yet in some cases we see that other elements appear to be endowed with the same properties." The authors add particular observations of the movements and changes of forms in mucus of the nose, and of the neck of the uterus, in ovules, male and female, and in the elements of different tissues.—*Archives Générales de Médecine*, July, 1866, p. 61.

A new Kind of Amœboïd Cells.—La Vallette St. George obtained from the seminal canaliculi of full-grown mammals and embryos of birds and amphibia, and also from the testicle of a water palmer, contractile cells, which were round, oval, or irregular in the state of rest, contained one or more nuclei, and a number of granules in the circumference of the latter, and changed their form in many ways. They extended short and broad or thin knob-shaped processes, or little button-like projections, provided with threads of various

lengths, which moved themselves to and fro, as if feeling, drawn out and in again. No considerable changes of place would thus be brought about. In suitable fluid the movements continued for hours, in the frog even for twenty-six hours; one preparation showed them to be still lively thirty-two hours after the death of the animal. Water abolishes them instantaneously. Experiments to induce the cells to imbibe particles of colouring matter remained without result.—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxvii, part 1, p. 6.

Development of "Connective" and Fibrous Tissues.— Dr. Ordonez has thus abridged the results of a prolonged study of this subject:

1. The stellate corpuscles, called equally plasma-cells and connective-tissue-corpuscles, are not permanent elements properly belonging to fibrillary tissue—connective—but really transitory elements proper to elastic tissue, and in which the existence of a cavity can by no means be demonstrated.

2. The primitive fibrils of fibrillary tissue, called connective, do not possess a central canal, of which there is no means of demonstration.

3. Nor are the elastic fibres channeled.—*Robin's Journal de l'Anatomie*, &c., No. 5, 1866, p. 503.

Muscular Fibre.—Mr. Macnamara, of the Calcutta Ophthalmic Hospital, maintains that the crystalline lens is "a muscle, and probably the most complex arrangement of the contractile tissue to be met with." On the general structure of muscular fibre he compares the contents of the sarcolemma to "a ladder of contractile tissue; the steps or horizontal bars of the ladder being, however, spiral bands, whereas its side pieces or perpendicular supports are flat bands, running continuously from one end of the muscle to the other end; the horizontal bars connect the perpendicular ones, but, as above stated, are curled upon themselves like a spiral spring." The author believes the contractile tissue to be an homogeneous substance, and that the unstriped muscle is one of the least complicated examples of this contractile tissue.—*The Indian Medical Gazette*, Sept. 1st, 1866, p. 237.

Lymphatics of the Heart.—MM. Eberth and Belajeff have investigated the hearts of man, of the calf, horse, pig, dog, rabbit and cat. In investigating the human heart the results have been generally unsatisfactory, because the specimens were seldom sufficiently fresh and free from pathological changes. Injections were made either by puncture or directly by the lymphatic trunks. Prussian blue suspended in water, the solution of Beale or a solution of nitrate of silver, were made use of. With the latter solution, by imbibition, experiments did not give satisfactory results. In some specimens not only the lymphatics but also the blood-vessels were injected.

The authors say they have verified the existence of a very close network of lymphatics, large and small, not only in the parietal and

visceral pericardium, but just as much in the endocardium, which, in this matter, is not in any degree inferior to other organs, such as the mucous membranes.

The lymphatics are especially abundant in the endocardium of the auricles, and they are few in number in that of the auriculo-ventricular valves. The inner membranes of the great vascular trunks do not contain lymphatics.

From the endocardium and pericardium the fine lymphatics, when passing into the thickness of the muscular tissue, are given off; they can often be traced to a centimètre in depth. These lymphatics are, moreover, few in number, which is contrary to the opinion expressed by Luschka.

The distribution of the lymphatic capillaries is very variable. Beside somewhat varicose twigs, measuring 0·01 mm. and forming, by their anastomoses, very large meshes, are found others, which, having a greater diameter, are compacted together, and which, at the points at which they communicate, present ordinarily considerable enlargements.

Their walls are made up of simple pavement-cells, furnished with few lateral prolongations.

It happens frequently that when the branches given off the lymphatics are injected, the mass enters the cellular sheaths of the muscular trabeculæ situated in the thickness of the endocardium. A network of many amastomoses is then seen, and may easily be confounded with the lymphatic networks. These are distinguished by the almost uniform diameter of its passages and by the polygonal form of its meshes.

In most human hearts injection of the lymphatics of the endocardium has failed, or rather it has only entered some isolated vessels. It is probable that the lymphatics were obliterated, either by age or by effects of an endocarditis.—(*Centralblatt für die medicinischen Wissenschaften*, No. 19.) *Archives Générales de Médecine*, July, 1866, p. 104.'

Lymphatics of the Glans Penis.—Dr. Belajeff investigates this subject as one of particular interest to syphilologists. He says, "The lymphatics of the penis, in man, present a true network of separate closed tubes, the walls of which have an epithelial lining. The chief of these capillary networks extend beneath the *mucous body of Malpighi*, where it forms a layer of numerous ramifications and anastomoses. More deeply, the lymphatic branches become larger and rarer; vertical sections show their very large gaping openings, with an adherent internal epithelial wall; nearer the surface the tubes are, as I have said, thinner, smaller, and forming sometimes meshes in the furrows of the skin, at other times little closed prolongations amongst the papillæ.

The diameter of the largest lymphatics is 1 to 2 mm.; that of the smallest, 0·08 mm. Although this variation of size is considerable, it is yet always less than that of the blood-vessels. As a proper

characteristic of the lymphatics, I would mention their dilatations to be observed, either about the middle of the channels or towards their confluence; this dilatation is larger in man than in the rabbit. It may be circular or total, or it may be unilateral. In these lymphatic capillaries it does not indicate the presence of valves, as in the lymphatic trunks; it only bears reference to a simple dilatation.

The single epithelial layer of the capillary tubes consists of oval, polygonal, fusiform, or indented cells. The longitudinal axis of the cells corresponds to that of the vessels. The nearer a capillary tube is to a trunk, the more are its cells compacted and their form is elongated. The terminal meshes, on the contrary, have tolerably large and rounded cells. The flattening of the walls of a transparent tube gives to the cells, under the microscope, a multiform appearance; for the dark lines of the borders of the cells cross reciprocally, and those which belong to one of the walls modify the normal appearance of the cells of the wall beneath it. The average length of the cells is 0·6 to 0·4 mm., and their width is from 0·008 to 0·020 mm. There is greater abundance of lymphatic capillaries throughout the glands and the balano preputial furrow than in the penis and the prepuce; but the terminal tubes of the prepuce are larger than those of the glans. The epidermis-layer has no lymphatics; they are beneath the mucous body of Malpighi, whilst below them the network of blood-capillaries is very abundant, even to the summit of the papilla.

The lymphatic trunks of the penis have two important characters, without reckoning their considerable size, viz,, the frequency of varicosities which correspond to their valves, and the composition of their walls, which are formed of two or three layers. Above the internal epithelial layer, cells and annular fibres, accompanied by few cells said to be of the connective tissue, present themselves. The elastic and, perhaps, muscular fibres are in the elongated spaces which exist between the rows of transverse or annular cells. Their direction is sometimes rectilinear, sometimes zigzag.—*Robin's Journal de l'Anatomie*, &c., No. 5, 1866, pp. 469-70.

Vessels and Nerves of Fibro-cartilage.—M. Sappey has presented to the French Academy of Science some researches on this subject which are thus summed up by him:—"All fibrous and fibro-cartilaginous structures have vessels and nerves. In all of them they spread out in great abundance, but, at the same time, in unequal number. In this point of view the ligaments and the peri-articular fibro-cartilages hold the first rank, the tendons and aponeuroses the second, and the inter-articular fibro-cartilages the third. In all both the vessels and nerves are remarkable for the extreme multiplicity of their anastomoses."—*Archives Générales de Médecine*, July, 1866, p. 115.

Termination of the Nerves.—M. Rouget, at the French Academy of Medicine, read a paper on this subject, in which his results are

thus given :—Concerning the corpuscles of Pacchini, the nerve surrounded by the envelope of Schwan penetrates a central substance of the corpuscle described by Enghelman.—1. The corpuscle is made up of connective tissue, disposed as layers, which appear in succession to the envelope of the nerve-fibre or envelope of Schwan. 2. The central substance of the corpuscle is not an amorphous substance, as Enghelman and Leydig thought, but a mass of connective tissue, disposed as laminæ and mixed up with nuclei. 3. The axis cylinder may be distinguished in the central substance. As to the termination of the nerves in electric apparatus, M. Rouget found that the nerves end as a fine interlacement, and he adds that this arrangement can only be well observed in living animals. Finally, in the nerves of the skin of the tail of the tadpole the author has found the same arrangement of nerve endings as in the apparatus of electric fish.—*Archives Générales de Médecine*, July, 1866, p. 115; and August, 1866, p. 243.

Peripheral Termination of a Motor Nerve.—Dr. Moxon directs attention to the retractor antennæ of the larva of a gnat—a fibre about one thousandth of an inch wide, provided with sarcolemma, which itself has nuclei upon it. "The transverse striation of the fibre is complete and regular. From the antennal lobe of the insect's cephalic ganglion comes the antennal nerve, a nerve of some size, which has a neurilemma sheath provided with nuclei; at some distance from the base of the chitinous antenna the nerve expands to form a long spindle-shaped ganglion full of ganglion-cells, and then in this ganglionic condition enters the antenna, the cells still discernible through the chitin. About two thirds of the distance from the encephalon to the ganglion, the nerve gives at right angles to its own course a branch smaller than itself; this proceeds at once to the outer edge of the antennal muscle and joins the outer edge; the motor nerve is just so long as to allow the play of the muscle in its frequent contractions. At the point where the motor antennæ nerve leaves the sensory antennal nerve there is a corpuscle, whether neurilemmar or no I cannot say; also there are two small nuclear corpuscles close to the end of the nerve on the muscle. The union of the neurilemma and sarcolemma is a direct continuity.

"The muscle, in contracting, preserves a straight border, beautifully distinct from the sinuous folds into which the sarcolemma is thrown. During extreme contraction the sarcolemma is gathered up into wrinkle-like folds, and this to a very different extent on the side to which the nerve is attached. On the opposite side to this attachment the sarcolemma fits at all times closely to the sarcous tissue, and it requires careful observation to see the wrinkles of the membrane during contraction, but on the side to which the nerve is attached this membrane is then raised in the most obvious way into bulging folds. The inequality of the folding of the membrane on the two sides produces a puckered appearance of the sarcolemma, very striking during extreme contraction. What it is that occupies the space which is thus shown to exist between the sarcous tissue

and the sarcolemma on the side whereto the nerve is attached I could not be certain. Nuclei appeared to exist at the spots where the folds became most prominent, and these nuclei (or this appearance of nuclei) are visible at the same spots in the uncontracted or slightly contracted state. It should be said that these nuclei are very distinct from the nuclei of the sarcolemma, both in its position and in appearance. But it is not doubtful that the sarcolemma and the neurilemma are simply continuous with each other, and that their respective contents become continuous at the point where their union takes place.

"The nervous contents of the neurilemma are, then, continuous with a pellucid material disposed along the same side of the fibre between the sarcous substance and the sarcolemma.

In conclusion, the author says, "I would remark, the proof of a direct ending of nerve upon striated muscle-fibre in a single case must hold good for all cases alike, for I submit that no one can suppose that sometimes the nerve does go into the muscle and sometimes it does not."—*Quarterly Journal of Microscopical Science*, October, 1866, p. 237, &c.

On the Termination of the Nerves in the Organs of Generation.—W. Krause writes, "It has been known for some time past that end-knobs occur in the clitoris, as in the penis, of the human and mammalian species (in the hedgehog, in the pig, and in cattle).

"Even then it surprised me, that in comparison to the richness in nerves of the mucous membrane of the human clitoris, one found so few end-knobs, and that 'so large end-knobs apparently came into view.' However, at that time, sufficiently fresh material and time to investigate the circumstances exactly were wanting.

"By more recent original investigations, undertaken afresh, in company with Dr. Polle, I found, first of all, that in the vaginal mucous membrane of the rabbit the nerve-fibres terminated with cylindrical end-knobs, whilst in the deeper parts were situated Vater's corpuscles.

"In the clitoris, as in the penis, of the human subject, I have anew seen globular end-knobs in no very great number. They were of about 0·06 mm. diameter (according to my more recent estimate, 0·04 to 0·07 mm.). But, moreover, I found peculiar terminal corpuscles, which, in reference to their indubitable function, may be called the *corpuscles of the genital nerves*. In them by far the most sensitive nerve-fibres of the clitoris terminate, and certainly with pale terminal fibres.

"The genital nerve-corpuscles lie in the tissue of the mucous membrane, beneath the basis of the papillæ, and removed from the latter itself, 0·15 mm. on perpendicular section.

"The form of the corpuscles is very various; but it is characteristic that they indicate the surface puckerings, whereby they get a mulberry-like character. The number of the puckerings amounts to 1—5; the primitive form of the corpuscles thus separating into divisions, is hereby globular, ellipsoidal, bean-shaped. For the most part only 1—2, seldom 3—4, double-contoured nerve-fibres enter

into the genital nerve-corpuscles. The size of the latter varies; some are hardly larger than the before-mentioned end-knobs, from which they are distinguished by their puckerings, others have up to 0·15—0·2 mm. thickness and length. They consist of a connective-tissue envelope, very firm, and full of nuclei and soft, fine, granular contents. In this relation they are, in the first place, joined to the end-knobs and the touch-corpuscles. The larger genital nerve-corpuscles appear, however, in so complicated and multifarious figures that it would be hard to distinguish the simple primitive form. The colossal proportionate size of the genital nerve-corpuscles is, no doubt, the chief reason why they have not been sooner recognised. In the human penis quite similar forms appear, but the abundance of nerves is considerably less. It is known that L. Fick had previously described the same Vater's corpuscles, whilst Tomsa saw the nerve-fibres in the penis, as in the corpuscles of touch, terminate in ganglion-cells, and partly in nerve-coils. The latter are not to be mistaken for those of the conjunctiva bulbi described by me. Tomsa left his presumed ganglion-cells for twenty-four or forty-eight hours' long-continued digestion of the skin of the penis, in a mixture of hydrochloric acid and strong alcohol, and a prolonged washing with distilled water. By such a procedure even the resisting elastic tissue becomes destroyed, as does also, as a matter of course, the nerve-substance. It is not quite easily to be ascertained exactly what Tomsa may have seen. Probably they were nerve-twigs torn off, and remains of nerve-substance projecting forwards, which as 'ganglion-granules' have been described, as, at least, they appear in the representations of digested touch-corpuscles.

"In the rabbit the nerve-fibres end in the clitoris in quite similar genital nerve-corpuscles, which measure up to 0·15 mm. The mucous membrane of the whole vaginal opening of the labia only presents end-knobs. Thus, it is known, in these animals, how the sensitive nerves in all parts of the female genital apparatus terminate, and from the anatomical arrangements we may certainly argue that the female sexual feeling is altogether confined to the erectile clitoris.

"From the proof of special microscopic sexual corpuscles, which certainly are not reproduced when once they have been lost, it follows that it must be barbarous to extirpate the clitoris of healthy young women on any ground whatever.

"For the investigation of the genital nerve-corpuscles one must, with much care, make use of, in the first place, quite fresh (yet warm) preparations, without anything added. However, injection preparations well filled with size and Prussian blue, soaking the fresh or injected glans in 3 per cent. of acetic acid, over saturation with about 3 per cent. soda lye are also to be recommended.

"In the rabbit the microscopical terminal nerve-end organs are very easily to be seen in horizontal sections of the mucous membrane of the clitoris. From the inner surface the latter may be best observed; there the corpuscles, as brought into view on perpendicular sections, l e beneath the papillæ.

"*Supplementary Note.*—The nymphæ of the human female also contain end-knobs; no genital nerve-corpuscles."—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxviii, part 1, pp. 86—8.

Nerves of the Intestine.—Dr. Auerbach says the number of nerve-fibres which pass from the mesentery to the intestine seems very inconsiderable, when considered in reference to those which make up the mesenteric plexus. Most of them arise in the plexus itself. The primitive cylinders which compose it are of extremely fine fibres, measuring from 0·0006 to 0·0010 millimètres, of pale outline, and each separately arising from a ganglion-cell. No outer coat can be here discovered, and they are not furnished with proper nuclei; but in man and many animals the cylinders are grouped in bundles, numbering two to eight, and the bundles are surrounded by a very delicate sheath, which contains a great number of nuclei. The primitive fibres are, besides, extremely delicate, so that they become completely dissolved in distilled water.

The ganglion-cells may be considered as of two types—one, in which they are rather large, the other in which they are very small. There always exist between the two extremes many transition forms. In the ganglia, which are principally formed of little cells, a mass of considerable cells exists at the level of the origin of the nerves. Each cell is enveloped in a sheath, furnished with sufficiently numerous nuclei.

A certain number of the cells are evidently uni-polar, and they are frequently grouped in pairs. Others of the cells are bi-polar, or give off three prolongations in different directions.

The grouping of these different elements generally obeys the following rules:—The network first in order is chiefly made up of longitudinal branches connected transversely by ganglia, which, independently of these anastomoses, form transverse rows in which they are often joined together; but sometimes these series occupy the arc of a circle pretty long, parallel to the transverse muscular fibres, and, rejoining each other, surround sometimes all the circumference of the intestine as in a ring. The muscular layer is thus divided into a series of zones circularly, of which each is traversed by a mass of ganglion elements. The primitive cylinders given off from the ganglion-cells go to the nearest longitudinal branch, which then sometimes traverses many (four, or more) transverse zones before forming peripheral divisions.

The secondary plexuses, which have but few ganglia, are not all at the same level as the principal plexus; they are elongated, superposed at different heights. In all, the fibres are transversely divided, that is to say, in a direction parallel to the circular muscular fibres.

The primitive cylinders, having penetrated the layer of transverse muscular fibres, pursue a rectilinear course in the same direction to some extent. In the first half of this course they are enveloped in a delicate sheath, furnished with nuclei, which disappear later. Here and there they may be seen to bifurcate at acute angles. M. Auer-

bach has not been able to follow in a sufficiently detailed manner the nerve-cylinders destined to the longitudinal muscular fibres. As to the mode of termination of the nerve-fibres, he has not been able to determine it.—(*Berliner Klinische Wochenschrift*, No. 5, 1865.) *Archives Générales de Médecine*, February, 1866.

Structure of the Lens.—Henle discovers a contradiction in the descriptions that have been given of the lens-fibres, of which it is said that they may be seen to grasp one another, whilst yet together, with the denticulation of their borders, as of the bones of the skull; that every fibre with its sharp edge is very evident in the interstice of every two neighbouring fibres. The appearance as if the fibres held one another with the denticulations can, in fact, only be the result of an optical delusion. The denticulations rather advance from both sides into the space which borders the broad surfaces of the fibres; many seem to be long enough to reach to the middle of this space, and the author thinks that the fine cross streaking which may often be made out on the superficial aspect of the fibres may be the expression of those together mutually advancing denticulations.—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxvii, part 1, p. 27.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

On the Injurious Effects of Dressings after Amputations.—Dr. Burow brought this subject before the profession in 1859, with the object of showing that much of the mortality that follows operations is really due to the employment of dressings. He adduced the slight mortality that had attended his own cases in which these were abstained from as compared with that exhibited by Pauli's statistics. He is now enabled to refer to an additional number of his own cases, making ninety-four altogether, in which the mortality has proved quite trivial. And yet these include numerous cases of amputation of the thigh, leg, arm, &c. many of the patients upon whom the operations were performed being very poor, and treated under disadvantageous sanitary conditions. The stumps in all, however, were left freely exposed to the air, unincumbered with dressings.

As to the execution of the operations, Dr. Burrow, when possible, always prefers the tourniquet to manual compression, and performs flap operations whenever the condition of the soft parts admit of this. He disapproves of shaving off the periosteum before dividing the bone, and is very particular in tying every bleeding vessel, believing it far better to tie some of these superfluously than to have

the stump disturbed by subsequent bleeding. The surface of the stump is to be left quite exposed until a serous exudation begins to issue, which is generally the case within half an hour, although in some cases we may have to wait some hours for this. The flaps are then brought together by means of two or three sutures and three or four strips of adhesive plaster, these last sufficing without the sutures in amputation of the arm. After the patient has been placed in bed the stump is only covered with a piece of linen to protect it from the flies, and in the case only of there being much pain ice is resorted to. The very great swelling which takes place in the stump during the second and third days is evidence of the mischievous effects which must result from its confinement by dressings. By Dr. Burow's plan nature is left unimpeded in her process of restoration of the collateral circulation in the divided vessels, and emboli with pyæmia are far less likely to occur. If the swelling of the edge of the stump be very great, the threads may be divided at their points of insertion and left to be discharged, the adhesive plasters being renewed when they become loosened. The discharges are to be gently pressed out from the deeper portions of the wound, and the greatest attention must be paid to cleanliness. How much less reaction follows this simple procedure is seen by the rapidity of the recoveries, patients who had undergone amputation of the thigh having repeatedly left their beds on the eleventh day, the stump being guarded by a small pledget kept on by adhesive plaster. For some years past Dr. Burow has been in the habit of applying to wounds attended with abnormal discharges the acetate of aluminum lotion, which is a cheap and excellent preparation for hospital practice, removing all bad smell. So after amputation, when the discharge is considerable he resorts to it.—*Deutsche Klinik*, No. 24.

On Catarrhal Inflammation of the Cavity of the Tympanum occurring in Young Persons.—Dr. Roosa in this communication refers to one of the forms of catarrhal inflammation of the tympanum which proves very amenable to treatment. It may come on without any positive exciting cause, more or less impairment of hearing gradually taking place, with little or no pain.

"Objectively, the following symptoms are observed. The pharynx is found in a state of inflammation; little elevations, like granulations, are seen on its surface, and the secretion is in excess. The tonsils may or may not be hypertrophied. The membrana tympani, instead of having its normal 'neutral grey' colour, is of a pinkish hue, with an exceedingly brilliant appearance. The vessels are not generally to be distinctly traced on any part of it. The triangular light spot is either entirely absent or is smaller than normal, indicating that the position of the drum is changed.

"The observance of hygienic rules, the exhibition of various therapeutical agents, and due attention to the condition of the mucous membrane of the pharynx, will in time bring relief in these cases, but the impairment of hearing is a symptom which long remains troublesome. We have, however, the means at our hands, in

Politzer's method of rendering the Eustachian tube pervious, of instantly improving the hearing, thus removing the most embarrassing symptom, while we go on with the proper general treatment, curing the disease on which this symptom depends. It may be supposed that the impairment of hearing in these cases is due to a plugging up of the faucial orifice and the calibre of the tube by mucus, which is suddenly expelled by the column of air driven in. . . . . It is confidently asserted that the use of Politzer's method in the treatment of this class of patients will render the progress of the case highly satisfactory, which under the old method could hardly be said to be the case. After the first use of the instrument the improvement which occurs will probably only last a day or two, but I have never known the hearing to have become worse, and the repeated (say three times a week) practising of the method will render the improvement permanent. It is only an adjuvant, a fact carefully to be kept in mind; and the necessary general treatment should never be lost sight of. I was led to the attachment of a bulb or inhaler containing tincture of iodine to the simple apparatus of Politzer, from the need felt of introducing some substance into the cavity of the tympanum which should promote absorption in the mucous membrane of the tube and middle ear. This want is supplied by the introduction of the iodized air; and my experience serves to show that the combination produces a more powerful and permanent effect than is produced by the use of simple air."—*American Journal of Medical Science*, July.

On Reducing Dislocations by means of Caoutchouc.—M. Anger observes that, having for some time employed this substance in the treatment of anchylosis, fractures, spontaneous dislocations, &c., the good results derived from it induced him to extend its application to traumatic dislocations. A man was brought to the clinic with dislocation of the shoulder, which the ordinary procedures failed to reduce. The trunk being secured, extension was applied by means of a tube of caoutchouc, the thickness of the little finger and sixty centimètres in length. Gently employed at first, the traction was gradually increased until the tube had been wound four times around the bed-post, thus making four traction cords of fifteen centimètres each. The extension was kept up for nearly half an hour, the patient by that time feeling quite exhausted; and the muscles which had resisted the reduction having become relaxed this was easily accomplished. This mode of making the extension by its gradual and gentle, though efficient, character, M. Anger considers as very superior to any of the ordinary procedures. The amount of traction employed must be proportioned to the resistance offered, which varies by reason of strength, sex, and age. In the present case, the subject of which was athletic and the dislocation complicated, four tubes, fifteen centimètres in length, extended to double their length, amply sufficed. The extension should be regulated in ordinary cases so as to obtain complete muscular relaxation in fifteen or twenty minutes.—*Gazette des Hôpitaux*, No. 74.

On amputation of the hip-joint.—Dr. Morton in this paper gives the histories of the eleven cases of this operation which have been performed at Philadelphia. Of these, seven recovered and four died. He terminates it with the following conclusions :—

"1. In cases of long standing disease, tumours involving the bone, soft parts, or both, necrosis following gun-shot wonnds, reamputations, or where suppuration has existed for a long period, the constitution being more or less accustomed to the drain and irritation, a more favorable result after amputation may be expected than in those cases where the operation has been performed for recent injury, where it has almost invariably proved fatal. 2. In regard to the choice of operation, the integumentary flap operation, with a circular division of the muscles close to the pelvis, may be considered a safer operation, and is preferred to all others by Professor Pancoast. By this there is not so much muscular tissue left to suppurate, and we have more reasonable hope for primary union to a considerable extent. In small shrivelled limbs, where there is hardly any tissue about the joint except skin, the flap operation is probably as good. 3. The loss of blood during the operation being primarily the chief source of danger, we find the abdominal tourniquet absolutely required, and no operation at the joint should be undertaken without it. Having the control of the circulation throughout the extremities, we are able to guard against all loss of blood, which otherwise might tend to develope pyæmia, or even immediately to allow of a fatal collapse. 4. The after-treatment of the stump by the application of *pure laudanum* (the parts being constantly wet with it) was first used by Dr. Pancoast in 1860, and answered admirably. In his second case he was able to put the patient asleep at any time by increasing the quantity used, more than a gallon being used in his last case. This dressing was employed in the case which came under my care, and answered every indication."—*American Journal of Medical Science,* July.

On sprains in children.—M. Guersant recommends that cases of slight sprain should be treated either by binding wadding around the joint, or by methodical kneading or shampooing (*massage*). This last may be resorted to either immediately or some hours after the accident, provided always that there be tumefaction and infiltration of the soft parts, a bandage moistened with a spirit lotion and a little extract of lead being afterwards applied. The hands having beeng reased with lard, gentle and prolonged pressure should be exerted on the limb from below upwards, the *séances* being repeated more or less often according to the severity of the sprain. In slight cases the patient is enabled to walk after one or two of these; but when the sprain is more severe, the shampooing may have to be repeated for several days. Where there is great swelling and severe pain leeches should be resorted to; or cold may be kept applied by means of wet compresses or continuous irrigation. At the end of a few days a bandage should be lightly applied, to be followed when the swelling has all subsided by a starch bandage, which may be retained for a fortnight, month, or even longer.

M. Guersant especially alludes to the sprains produced in children by the mischievous practice of suddenly raising them by a single arm, the limb always being more or less twisted into a state of pronation or supination, with distension or stretching of the joints at the wrist and elbow taking place. It is very rare for fracture or dislocation to be produced in this way, but the appearances may be such as to cause alarm to the friends of the child and sometimes even to the medical attendants. In ordinary cases, there is no appreciable deformity present, but the movements of the parts give great suffering to the child, and on the execution of these a sound is sometimes heard, without seeming to proceed from any precise spot, such as might be produced by the sliding of articular surfaces on each other. Quite suddenly, after the execution of some of these movements, the child ceases to complain; and without our seeming to have done anything to remedy the defect, he becomes enabled to move the arm as before the accident. Sometimes, however, the pain persists, and there may be great tenderness around some one of the articulations. It is not always possible to make a correct diagnosis in these cases; but when neither fracture or dislocation can be detected, a sprain may be said to have been produced, *i. e.*, a sliding of the articular surfaces with distension of the ligaments; or, in other words, a tendency to a dislocation which has not been effected. The accident is not always confined to the wrist or elbow, and may implicate more than one joint. The arm should be kept at right angles, either in supination or pronation according to the preference of the patient. The child then complains no more, and in three or four days is cured. If at the end of this time pain persists, a starch bandage may be applied for eight or ten days.—*Bulletin de Thérapeutique*, August 15th.

On Ovariotomy.—M. Kœberlé, who has done so much towards the introduction of ovariotomy into France, observes that the prejudices against it in that country are gradually diminishing, and comments upon the difficulties there are in statistically comparing this with the other great operations. These have so many analogies as to render them comparable, while ovariotomy is not only a more or less serious operation according to the general conditions under which it is undertaken, but also with respect to the particular cases and the different operative procedures. The surgeon who only operates in desperate and complex cases will necessarily obtain less successful general results than if he operated only in simple and favorable cases. The cases which are met with on the continent are generally unfavorable ones, the prejudices against the operation causing great delay in resorting to it. And yet, practised in opportune time, and before the development of grave complications, due to the prolonged existence of the tumour, to the tappings, &c., it has been subjected to, and to the adhesions which have formed, ovariotomy is relatively an operation of slight gravity, as is shown by the statistics which have been published of late years.

M. Kœberlé points out that the fatality of ovariotomy so fre-

quently observed on the continent cannot be explained by the vitiated air of large towns, the nationality of the patients and the like, as other cases operated on in the same localities and occurring in persons of the same country, have succeeded well. At Strasburg, prior to his own successful operations, all cases proved fatal. His first fourteen operations, between 1862 and 1865, were performed at St. Barbe, an establishment which is far from presenting favorable sanitary conditions; and yet he met with ten recoveries to four deaths. Among these fourteen cases there were seven cases of single ovariotomy, one of which, occurring in a patient in very bad health, proved fatal. Of the seven cases of double ovariotomy three proved fatal, in two of these death being due to the operation itself. The number of cases of double ovariotomy which have occurred in M. Kœberlé's practice is very remarkable, viz., ten examples (four dying and six recovering) out of twenty-seven cases. Mr. Wells only met with seven double ovariotomies in 150 cases, and declined operating in seven other cases; and Dr. Keith has only had three double ovariotomies in thirty-five operations. To the present time M. Kœberlé has never, even in the most difficult cases, &c., in spite of most extensive adhesions, had to leave an operation unfinished. In his first fourteen operations there were two series of four successful cases separated by five cases giving rise to three deaths, and the practice of the English surgeons present very remarkable instances of series of successful and unsuccessfnl cases. The only case in which M. Kœberlé cut short the ligature of the pedicle, proved fatal, although the patient presented conditions very favorable to recovery. She died with symptoms of septicæmia forty-four hours after the operation, the only abnormal appearance found at the autopsy being a circumscribed sero-purulent collection around the ligature.—*Gazette des Hôpitaux*, Nos. 77 and 79.

On some Causes of the increased spread of Syphilis.—Professor Sigmund in this paper states some of the causes, explanatory of what he says is a well-established fact, that syphilis is much on the increase. But it seems to us that the causes which he adduces have always existed, and in no wise explain any increase which may have recently been observed. First among these is the ignorance of the patient as to the nature and duration of his complaint, and the insufficiency of the time he remains under medical inspection—so that he is often unaware that he is ill, or believes himself cured long before he is so. The spot where the contagion operates may in the first instance exhibit very slight and fugacious changes of structure which by some simple application, and without medical aid, may disappear—to be succeeded within some weeks by more decided textural changes, such as induration, papulæ, &c., affecting the same part. In women, the diseased conditions are even much less readily detected than in men. During the period of incubation also, which is seldom shorter than two or three weeks, and in not a few cases extends to six, it is impossible for the patient to be aware of an affection which he may have ample opportunity of spreading. How common sexual inter-

course is even after disease has made considerable progress, the experience of every hospital surgeon testifies. Next comes the generally spread opinion that the patient is cured as soon as skinning over of a sore has taken place. The specialist is in no such haste to pronounce on the matter until he has watched the patient duriug the period in which constitutional symptoms may arise. Many patients are thrown into a state of false security by the immediate employment of prophylactic measures, which experienced observers know to be useless or mischievous—the course of syphilis not being preventible by any such means. It will be long, however, before surgeons and their patients accept this opinion. Erroneous diagnosis causes the most different non-syphilitic appearances to be mistaken for the true disease until the characteristic symptoms of the latter appear. Lastly comes the difficult question of latent syphilis, during which persons apparently well continue long to have the power of affecting others. The general conclusion is that more careful examination and prognosis are requisite, and that the public should be indoctrinated in the necessity of these. Professor Sigmund protests against the inactivity of the medical profession in the face of the ever-increasing spread of the venereal disease; and maintains that even individual action may do much by propagating correct ideas upon the subject.—*Wien Med. Wockenschrift*, Nos. 40 and 41.

Observations on the Question of Trephining.—Professor Roser, of Marburg, in his interesting observations on "traumatic sepsis," laid down the following maxim:—"The local decomposition which takes place as a consequence of the splintering and crushing of the bone, and of the contusion of the dura mater and brain, has especially to be considered in determining upon the use of the trephine;" and in the present paper he enters upon the subject at greater length. Every one admits the desirableness of treating injuries of the head as much as possible upon th e subcutaneous method, as the influence of the access of air on the decomposable material is obvious; but, on the other hand, in these compound injuries in which great violence has been done to the deep-seated parts, a similar decomposition may be set up without any access of air at all, and calling for incisions to evacuate the products. The question of prophylaxis in these cases is sometimes a difficult one, for it may be often a matter of doubt whether we should remove splinters and extravasated blood in order to prevent decomposition ensuing, or whether we should carefully exclude them from the access of the air. And is it desirable to remove these fragments at once, exposing, perhaps, the surface of the dura mater or the brain, or wait until symptoms arise calling for operation? Interference at once can only be advised when there is a very obvious disposition to decomposition and suppuration, and when the operative procedure is a very simple one. Much will depend upon the accompanying circumstances, as suitable instruments, sufficient light and assistance. The relative advantage of the chisel over the trephine in such operations is so great as regards danger, that we may lay

down the maxim that it is better to leave the fragments in than to trephine, while it is preferable to resort to the chisel than to leave them in. In cases in which, from the nature and position of the fracture, a certain amount of cerebral contusion must have been produced, the question arises whether the formation of an abscess of the brain, which so often results from this, may not be obviated by the removal at the appropriate time of fragments, balls, &c., which have been driven in or have been followed by necrosis. Awaiting an answer to this, it may be observed (1) that the removal of such fragments or foreign bodies is often attended with great relief to the symptoms present in injury to the head; (2), that in several instances the operation has been performed too late to be of service, suppuration still taking place which might have been prevented by its earlier execution; and (3), that many secondary deaths arise not from the injury alone, but from the miasmatic influences to which the patient is exposed, abscesses now arising from extravasated blood which might under other circumstances have been absorbed, and that whether an operation has been performed or not. The unfavorable results obtained from the trephine in many clinics, as, for example, in the Paris hospitals, may in part be ascribed to their pyæmic miasmata, just as may the unfavorable issue of amputations in the same establishments. The operation of trephining has suffered much in estimation from this cause, just as that of ovariotomy has done.—*Archiv der Heilkunde*, 1866, heft 6.

Reduction of Dislocation of the Shoulder by the Pendulum method. Professor Simon, of Rostock, gives this rather fanciful name to the following procedure:—The patient is laid on his sound side across a stool or bench, having his sound arm first fastened by a towel to the thorax, so as to prevent his deriving any support from it. An assistant, mounting the stool, seizes the dislocated extremity by the wrist and draws it upwards, while the operator, grasping the shoulder with both hands, assists the entrance of the head of the bone into the cavity by direct pressure. When a greater amount of extension is required, for which a higher raising of the body is necessary, a towel must be passed around the wrist, and connected with a strong cord, which is passed over a hook or a frame and hauled at by an assistant. If a pulley is at hand, the rope may be run over it, and the patient raised as high as required. As the body is raised towards the vertical position, support would be given to it by the feet and legs, and this should be obviated as much as possible by fastening the legs with a rope, and causing an assistant to raise them from the floor. The operator, while circumscribing the joint with his hands to guide the reduction, can also increase the amount of extension-power by allowing the weight of his body to press upon the region of the shoulder. The pendulum-movement which occurs while the patient is in this position, assists the head in slipping in through the ruptured capsule.

This procedure, which he has resorted to in several cases with marked success, Professor Simon believes possesses great advantages

over any other in use. It is easily employed even by practitioners unaccustomed to surgical operations and it is very safe. In simple cases the practitioner may reduce the dislocation by simply raising the patient, without any assistance; and although in more difficult cases he requires an assistant, or even two, these may be taken from among the bystanders. In many cases the use of chloroform is superfluous. The extension is made very gradually and equably, counter extension being the work of the patient's body. It can be carried to the extreme extent that is admissible; for, taking the average weight of the patient at from 120 to 130 lbs., which may be augmented to 150 or 200 lbs. by means of the weight impressed by the operator, and we have reached all that is permissible—as clinical observation at Göttingen has shown that an extension-power of 200 lbs. will fracture the humerus. In one of the author's cases, the dislocation, which was of three weeks' date, was reduced immediately that the entire weight of the body was suspended.—*Langenbeck's Archiv für Chirurgie*, Band VIII, heft 1.

Hospital Gangrene.—Dr. Fischer has published an elaborate and valuable report on the cases of hospital gangrene observed in the External Division of the Berlin Charité during 1864-65. They amounted to 44 in number; 33 occurring in men and 11 in women. Many of the cases are given in full details, and the following are some of the conclusions which the reporter arrives at from their observation. 1. Hospital gangrene may occur at any time of the year and in all states of the weather. 2. Although it may attack the strong and the weak, the young and the old, yet aged and cachectic individuals are its especial victims. 3. The hospital genesis of hospital gangrene is not tenable. 4. Hospital gangrene develops a specific contagion. (*a*) This contagion is transmissible by contact to wounds and animals. (*b*) It is not improbable that communicated to the air, it may infect wounds through this. (*c*) It is very questionable whether this contagion gives rise to a specific general infection before or after the development of the local malady. (*d*) Ulcers may be produced by hospital gangrene alike in all the tissues. (*e*) Ulcers and wounds of every description may be attacked by it. (*f*) For its development an ulcerated or exposed surface is not essential, as it may appear spontaneously in healthy skin. (*g*) A first appearance of hospital gangrene does not weaken its disposition to occur, and frequent relapses are not rare. In relation to treatment, Dr. Fischer speaks highly of the advantage derived from treating these patients in the tents placed in the grounds of the Charité. He insists strongly on the importance in the general treatment of pure air and perfect quiet, to absence of depressing emotions, and the necessty of good nourishment.—*Annalen des Charité Krankenhauses*, Band XIII., heft 1.

Improved Form of Probes for the Lachrymal Passages.—Dr. Williams observes that "it has been customary to bend Bowman's probes to such a curve, as to adapt them to the presumed direction

of the nasal duct in each individual: but I have seen false passages formed where undue violence had been employed in their use; and, at best, the mucous lining of the sac or canal was often torn or abraded by the unyielding extremity of the probe causing considerable hæmorrhage, and giving rise to irritation which retarded the cure.

"I have found great advantage in using probes made with bulbous extremities of the six sizes of Bowman's scale, but very slender for some distance from their ends; so that the whole of that part of the instrument, without being unduly flexible, has an elastic pliability, enabling it to adapt itself to any sinuosities of the passage, and to find a route through the obstructions without laceration or contusion of the parts."—*Boston Med. and Surg. Journal,* Aug. 30.

Bandages with Air-compression.—Dr. De Beaufort, after adverting to the difficulty there is in obtaining equable and regulated compression by means of the ordinary roller, however skilfully applied, describes a form of bandage which he has found very effective. He employs a small bag of vulcanized caoutchouc, suited in form to the part upon which compression is to be exercised, and applies it empty, adapting it as exactly as possible. This sac is furnished with a little cock, to which a small forcing pump is attached, and sufficient air is insufflated to produce the amount of tension required, the sac having been first secured over the part by an ordinary bandage loosely applied. In this way a gentle, uniform, and generalised compression is secured, which can be augmented or diminished at will. When a great amount of compression is required, more than one bag may be necessary; and when it is desired to localize this ,a small bag may be placed under the larger and separately insufflated. Compression may be also exerted on parts which present difficulties, as in the case of the eye. In treating fractures, this mode of compression is of great use, substituting for painful lateral pressure a circular one, which is very supportable, and allows of the apparatus being much lighter than that in ordinary use. It renders the use of pads in the splints superfluous; and in children, the simple air-bags may be used without any splints.—*Bulletin de Thérapeutique,* 30th November.

Treatment of Anthrax.—M. Labat sums up an interesting paper read at the Medico-Chirurgical Society of Bordeaux, with these positions:—1. The gravity of anthrax, all things being otherwise equal, is generally proportionate to its extent; and it is purulent absorption which, in the majority of cases, proves the cause of death. 2. The surgical treatment of the first stage—the stage of inflammatory induration—should consist in practising deep multiple incisions, radiating from the centre to the circumference, and involving the entire thickness and the entire extent of the diseased tissue. 3. In the second stage, that of resolution and detersion, small sticks (*flèches*) of Canquoin's paste should traverse the tumour beneath the skin, so as to circumscribe it in all directions. 4. Infrequent dressings

should be applied, consisting of small dossils of charpie impregnated with alcohol, and covered with an astringent and detersive powder, until the wound is thoroughly cleansed. 5. Tincture of aconite should be administered when putrid infection is feared, and ergotine when there is danger of purulent absorption. 6. It is of great importance that the diet should be as substantial as possible.

In the discussion which followed the reading of the paper, M. Denucé observed that anthrax is characterised by a special malignity which may be designated as gangrenous inflammation, being due for the most part to putrid and purulent infection—septicœmia produced by the resorption of gangrenous products. Besides free incisions, he is of opinion that the action of caustic agents is indispensable, which, by obturating the divided vessels, both prevent the absorption of septic products, and the occurrence of debilitating hæmorrhage. For this purpose he has found the perchloride of iron preferable to Canquoin's paste, causing much less suffering, and yet acting very efficaciously.—*Mém. de la Soc. Méd.-Chir. de Bordeaux*, No. 1.

On Covering the Wounds from Amputation or Disarticulation with Diseased, Changed, or Contused Soft Parts.—Professor Simon regards the rule, that the incisions necessary for amputation must only be made through sound and healthy tissues, when carried out absolutely, as very mischievous and as having caused the sacrifice of many limbs, or parts of limbs, that might have been saved. In the small number of amputations which are performed on account of disease of the soft parts, the employment of these as a covering to the wound is of course out of the question; but when, as in the great majority of cases, the soft parts are only secondarily diseased, they may often be as usefully employed as if they were quite healthy tissues. While textures the subjects of gangrene, or which have undergone pseudo-plastic infiltration, can never be so employed, œdematous, tumefied, or inflamed parts, infiltrated with pus or traversed with fistulæ and abscesses, the usual accompaniments of disease of bone in its various stages, are quite fitted to form amputation-flaps, even when their changed condition has reached a high degree. If their connection with the healthy parts is sufficient to maintain the nutrition of the flaps, these may even unite to a considerable extent by primary intention, the œdema disappearing, the pus drying up, and the fistulæ and ulcers contracting with rapidity. Moreover, not only may soft parts be thus utilized, which have become diseased from inflammatory action and its consequences, but also tissues which have had their vitality lowered by contusion or defective nutrition.

Professor Simon gives abstracts of several remarkable cases in proof of the above positions: In all of them he endeavoured to obtain the greatest possible quantity of soft parts for his covering flaps, and especially sought to have their connecting bridges of a sufficient breadth.—*Langenbeck's Archiv für Chirurgie*, Band viii, heft i.

Summary.

Amputation.—Butcher. Amputation at the Hip-joint. (Dublin Journ. of Med. Sc., Nov.)

Aneurism.—Kade. Case of Aneurism of the Brachial Artery. (Petersburg Med. Zeitschrift, No. 4. Compression, ligature of the axillary, and excision of the sac, were successively resorted to, the patient recovering.)—Oré. Case of Popliteal Aneurism successfully treated by a New Compressor. (Mém. de la Soc. Méd-Chir. de Bordeaux, No. 1.)

Bone.—Thomas. On Exostosis. (Australian Med. Journ., May.)—Dolbeau. Clinical Lecture on Excision of the Head of the Humerus. (Gaz. des Hôp., No. 94.)—Von Wahl. Cases of Spontaneous Osteomyelitis. (Peters. Med. Zeit., No. 4.)—Whitehead. Excision of the Upper Maxilla. (New York Med. Journ., June. An interesting case, illustrated by woodcuts.)—Hueter. Statistical Account of the Excisions performed at the Berlin Surgical Clinic, 1862-65. (Langenbeck's Archiv für Chirurgie, Band viii, Heft 1. An elaborate paper in continuation of one by Lücke in Band iii. It relates to 84 cases, with 68 recoveries and 16 deaths.)—Jaeschke. Cases of Excision. (Ibid. Thirteen cases treated in Russia, with 6 deaths and 6 recoveries.) —Two Cases of Excision of the Hip-Joint. (Berlin Klinische Woch., Nos. 35 and 36. One fatal.)—Ollier. Subperiosteal Excision of Bones of the Foot. (Gaz. des Hôp., No. 140.)—Whitehead. On Tumours of the Superior Maxilla. (New York Journal of Medicine, August.) — Mason Warren. On Cystic Tumours of the Jaw. (Boston Med. Journal, July 19.)—Discussion at the Paris Société de Chirurgie on Esmarch's Operation on the Lower Jaw. (Gaz. des Hôp., No. 111.)—Collis. Cases of Operations about the Face. (Dublin Journ. Med. Science, Nov.)—Simon. On Operations for Necrosis. (Deutsche Klinik, Nos. 37, 38, 41.)

Cæsarean Operation. — Koeberlé. Successful Case of Cæsarean Operation. (Gazette Hebdomadaire, No. 34.)

Chest.—Weilinger. Penetrating Wounds of the Chest. (Petersburg. Med. Zeit., No. 3.)

Dislocations.—Richon. Case of Dislocation of the Spine. (Recueil de Méd. Militaire, July. A case of bilateral dislocation of the sixth cervical vertebra on the seventh, which proved fatal on the seventieth day. Illustrated by a woodcut.)—Congenital Dislocation of the Femur. (Union Méd., Nos. 80—87. Discussion at the Société de Chirurgie de Paris.)—Isenmeyer. Pathological Dislocation of the Patella outwards. (Langenbeck's Archiv, Band viii, Heft. 1. A *résumé* of the literature on the subject, and an original case.)—Mignot-Danton. Lateral Dislocations of the Metatarsus. (Archives Générales, Oct.)—Bitot. A New Means of Retention in Supra-acromial Dislocation of the Clavicle. (Mém. de la Soc. Méd.-Chir. de Bordeaux, No. 1.)

Ear.—Pravaz. Treatment of Catarrhal Deafness by Compressed

Air. (Gaz. des Hôp., No. 85.)—Hagen. Practical Observations on Diseases of the Ear. (Wien. Wochenschrift, Nos. 45, 46, and 51: On the Employment of Electricity in Affections of the Ear.)—Hubert-Valleroux. Diagnosis, Etiology, and Prognosis of Otorrhea. (Gaz. Hebdomadaire, Nos. 25 and 26.)—Grossmann. Practical and Pathological Significance of Otorrhea. (Allgem. Wien. Med. Zeit., Nos. 24—27.)

Eye.—Galezowski's Ophthalmoscope as modified by Laugier. (Bull. de l'Acad. de Méd., May 22. With a woodcut.)—Giraud-Teulon. On Perforation of the Unguis in Fistula Lachrymalis. (Gaz. des Hôp., No. 91.)—Wecker. Case of Dacryops. (Gaz. Hebdomadaire, No. 25. With woodcut.)—Delgado. Speculum Porte-caustique for the Destruction of the Lachrymal Sac. (Annales d'Oculistique, May.)—Dolbeau. Clinical Lecture on Sympathetic or Reflex Ophthalmia. (Union Méd., No. 79.)—Küchler. On Exophthalmos and Tumours of the Orbit, especially Benignant Sarcoma. (Deutsche Klinik, Nos. 17—23.) — Galezowski. On Syphilitic Retinitis and Neuritis. (Gaz. des Hôp., No. 106.)—Hirschler. Secondary Syphilitic Ulcer of the Eyelids. (Wien. Med. Woch., Nos. 72—74.)—Von Graefe. Additional Observations on Modified Linear Extraction. (Arch. für Ophth., Band xii, Abth. i.)—Tavignot. On a New *Kératome-fixateur*. (Gaz. des Hôp., No. 101. With an illustration.)—Kloz. On a New Dilator of the Lachrymal Passages. (Bull. de l'Acad. de Méd., Oct. 9. With an illustration.) — Herzenstein. New Operation for Trichiasis. (Arch. für Ophthalm., Band xii, Heft. 1. With illustrations.)—Prince. New Operation for Entropion. (Amer. Journ. Med. Sc., Oct.)—Wilson. Theory and Principles of the Ophthalmoscope. (Dublin Med. Journal, Nov.)—Perrin. Artificial Eye for Ophthalmoscopic Study. (Gaz. des Hôp., No. 123.)

Fracture.—Lannelongue. Fracture of the Middle of the Radius. (Gaz. des Hôp., No. 95.)—A Guérin. Fractures of the Upper Jaw. (Archives Générales, July.) Guérin states that these fractures may be recognised, when there is no displacement, by pressing the finger on the internal wing of the pterygoid apophysis.)—Berenger-Féraud. On Suture of Bones in Ununited Fracture. (Bulletin de Thérapeutique, July 15th and 30th. A *resumé* of the twenty-five cases which have been already published, and a woodcut given of a new form of drill.)—Marit. Double Fracture of the Patella. (Recueil de Méd. Militaire, April. Woodcuts of the apparatus the author has found useful in approximating the fragments of the patella when broken in two places.)—McDonnell. On Trephining in Fracture of the Spine. (Dublin Journal, August.)—Linhart. Fracture of the Lower End of the Radius. (Wurzburg Med. Zeit., Band vii, H. 1. The mechanism of the production of this fracture illustrated by an engraving.)—Senfft. Case of Delayed Union of Fracture of the Femur through Lactation. (Ibid.)—Gunning. Treatment of Fracture of the Lower Jaw by Interdental Splints. (New York Journal of Medicine, Sept. and Oct. With illustrations.)—Lücke. Apparatus for facilitating the application of the Gypsum

Bandage in Fractures of the Thigh. (Berlin Klin. Woch., No. 39. With an illustration. The author also speaks highly of water-glass as a material for bandages in fracture.)—Azam. Case of Old Ununited Fracture successfully treated by Electro-puncture. (Mém. de la Soc. Méd.-Chir. de Bordeaux, No. 1.)—Manz. On Orbital Fractures. (Archiv für Ophthal., Band xii, Abt. i.)—Rose. Diagnosis in Fracture of the Pelvis. (Annalen der Charite-Krank., Band xiii, Heft 2. Eight cases related, with illustrations.)

Gangrene.—Discussion on Diabetic Gangrene at the Paris Soc. de Chirurgie. (Union Méd., Nos. 142, 145, 146.)

Hernia.—Borelli. On the Reduction of and Operation for Hernia (Gazetta Med. di Torino, Nos. 1—35.)—Cheever. On Wood's Operations for the Radical Cure of Hernia. (Boston Med. Journal, July 5. The results of twenty operations given.)

Hydatids.—Paul. On a new mode of treating Hydatid Cysts of the Liver. (Union Méd., Nos. 122 and 125. Describes M. Demarquay's mode of opening with these Vienna paste.)

Hydrocele.—Melchiori. Clinical Observations on Hydrocele and other Aqueous Tumours of the Testis. (Annali Universali di Med., June. Founded on 282 cases, several of which are detailed.)

Laryngoscope.—Tobold. Extirpation of Cancroid from the Larynx. (Berlin Med. Wochenschrift, No. 26. Woodcuts.)—Turck. Communications on the Laryngoscope. (Allg. Wien. Med. Zeit., Nos. 25 and 31.)

Larynx.—Bevan. Report on Scalds of the Larynx. (Dublin Med. Journal, Nov.)—Gottstein. Case of Abscess of the Larynx opened from within. (Berlin Klin. Woch., No. 44.)

Ovariotomy.—Spiegelberg. Two successful Cases of Ovariotomy. (Monatschrift für Geburtskünde, March.)—Jackson. Successful Removal of both Ovaries. (American Journal of Medical Science, July.)—Martin. Two successful Cases of Ovariotomy. (Berlin Klin. Woch., No. 45.)—Koeberlé. Cases of Ovariotomy performed at Strasbourg. (Gaz. des Hôp., No. 115, 133, and 140. Nineteen cases in all.)—Keith. Cases of Ovariotomy. (Edinb. Med. Journ., Dec. This report brings the cases up to 48, with 11 deaths.)—Mackinnon. Fatal Case of Ovariotomy. (Australian Med. Journal, Oct.)

Plastic Operations.—Trélat. Case of Palatoplasty. (Gaz. des Hôp., No. 134; Union Méd., 130. With a discussion at the Soc. de Chirurgie.)—Collis. Two cases of Rhinoplasty. (Dublin Journ. of Med., Nov. With illustrations.)—Cabot. Two Cases of Ruptured Perinæum treated by a new method. (Boston Med. Journal, Aug. 16.)

Polypus—Porta. New Apparatus for Ligature of Uterine Polypi. (Annali Univ. di Med., Sep. With illustrations.)

Prothesis.—Esmarch. Description of an Artificial Leg. (Langenbeck's Archiv, Band vii, H. 3. With plates.)

Syphilis.—Zeis. On Syphilitic Ulcers between the Toes and Fingers. (Deutsche Arch. f. Klin. Med., Band ii, Hf. 3. With illustrations.)

Testis.—Discussion at the Société de Chirurgie on Operations upon the Testis. (Gaz. des Hôp., Nos. 105, 108, and 114; Union Méd., Nos. 103, 106, 112.)

Tracheotomy.—Hueter. Practical Observations on Tracheotomy. (Verhandlungen der Berlin Med. Gesell., Heft 2.)

Transfusion.—Mathieu. On a new Instrument for Transfusion. (Bull. de l'Acad. de Méd., Nov. 13.)

Urinary Organs.—Vedrénes. Affections of Cowper's Glands. (Recueil de Méd. Militaire, July. Four cases of Phlegmonous Inflammation, or, as the author terms it, "Cowperite," two terminating in resolution and two in abscess.)—Stilling. Rehabilitation of Stafford. (Deutsche Klinik, Nos. 20—26. A controversial paper, in which its author maintains that Ivanchich's urethrotome is neither more nor less than Ricord's, and that this last is Stafford's Lancetted Stilette.)—Geinitz. Spontaneous Breaking up of Stone in the Bladder. (Jena Zeitsch., Band iii, H. i. The author adds seven cases which have come under his notice to those collected by Berno Schmidt.)—Corbeil. Neuralgia of the Urethra successfully treated by Urethrotomy. (Gaz. des Hôp., No. 78. A woodcut of the urethrotome employed).—Sentex. Case of Traumatic Rupture of the Bladder. (Mém. de la Soc. Méd.-Chir. de Bordeaux, No. 1.)—Dittel. Fatal case of Catheterism by Brainard's Method. (Wochenblatt der Gesell. f. Aerzte in Wien, Nos. 28—30. The stricture traversed from behind after puncture of the bladder.)—Duplouy. On Internal Urethrotomy. (Archives de Méd. Navale. Account of 10 cases treated at Rochefort.)—Guersant. On Prolapse of the Urethra in Children. (Bull. de Thérapeutique, Oct. 15. M. Guersant has met with 12 or 15 cases in girls from two to twelve years of age, and treats the affection by excision with curved scissors.)

Vagina.—Dolbeau. Clinical Lecture on an Operation in a Case of Congenital Absence of the Vagina. (Gaz. des Hôp., No. 84.)

Vesico-vaginal Fistula—Gaillard. Modification of the Operation for Vesico-vaginal Fistula. (Bull. de l'Acad., Nov. 20, and Gaz. Méd., No, 47.)—Labat. A successful Case of Operation for Vesico-vaginal Fistula. (Mém. de la Soc. Méd.-Chir. de Bordeaux, No. 1. A modification of Collis' operation.)

Wounds.—Roser. On Blotting-paper as a Dressing. (Berlin Med. Wochenschrift, No. 26. Roser states that this makes an excellent substitute for charpie.)—Burggraeve. Plumbage of Wounds. (Presse Méd. Belge, No. 35. Dressing wounds by thin sheets of lead, kept on by adhesive plaster, is here stated to be an admirable procedure, applicable even in comminutive fractures, crushing, &c. It expedites healing, and often saves limbs that would be sacrificed.—Guérin. On an Apparatus for Pneumatic Occlusion. (Gaz. Méd., No. 46.)—Robert and Colin. New Instrument for extracting Bullets from Wounds. (Bull. de l'Acad. de Méd., Oct. 2. With an illustration.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.,

Obstetric Physician to St. Thomas's Hospital; Examiner in Midwifery to the Royal College of Surgeons, &c.

I.—The Non-pregnant State.

Pelvic measurements in different Races of Mankind. By Dr. Carl Martin.—Dr. Carl Martin, son of the eminent professor at Berlin, fills up in a memoir rich in observations and research a void in obstetric literature. Having lived two years in Brazil, he availed himself of his opportunities to measure the pelves of living examples of several races; he has subsequently examined and measured the dried pelves of many European collections, and he has also made comparative measurements of fifty living women in Berlin, of twenty subjects in the dissecting room, and in sixteen of these last he has measured again the pelvis after drying. Between January, 1860, and the middle of 1864, his observations included 800 cases.

He arrives at the following conclusions:—1. The pelvis of the European woman is the most roomy. The great pelvis is broader than in any other race, showing the greatest distance between the anterior superior spines of the ilia, and also between the cristæ. The inlet is naturally transversely oval, the true conjugate diameter being smaller compared with the oblique and transverse diameters. The alæ of the ossa innominata are mostly translucent.

2. The pelvis of the negress is smaller, and especially narrower; its inlet is also transversely oval, but the true conjugate, like all its longitudinal measurements, are relatively greater than in European pelves. The sacrum is broad and long. The obturator foramina are small. The alæ of the ossa innominata are mostly opaque.

3. The pelvis of the Bushman corresponds to the small stature of the entire body, and is smaller than in any other race. The alæ of the ossa innominata were, in all the specimens examined, opaque. The ischia were, in proportion to the length of the cristæ of the ilia, pretty long, and higher than in any other race. The inlet is sometimes in a high degree oval in antero-posterior direction.

4. The pelvis of the Malayan of Java and the neighbouring islands is also narrow; it shows a small distance of the spines and crests in relation to the rather large conjugate. The true conjugate is very long; the inlet of the pelvis is therefore very round, and in many specimens even oval antero-posteriorly. The alæ of the ossa innominata in all Java pelves show large translucent spots. The ischia are relatively smallest.

5. The pelvis of the aboriginal of America is, according to the few specimens examined, on an average, little smaller than the European; the inlet is round, the outlet large, and the ischia long.

6. The pelvis of the Pelasgian and Australian negresses shows a rather large distance between the spines and crests; a large true conjugate, a small transverse and a moderate oblique diameter, and thus a somewhat round inlet.

Of other races and families, Dr. C. Martin has not obtained measurements. The memoir is also deficient in information as to the comparative characters of the fœtal factor in parturition. Dr. C. Martin says that he has not learned either from his transatlantic practice or his reading whether the remarkable variations in the form of the pelvis influences the mechanism of labour, especially whether the approach to the antero-posterior oval inlet brings the sagittal suture more into relation with the conjugate diameter. The coloured women in Brazil had labours neither easier nor more severe than European women. The plausible theory of Vrolik—that Javanese women bear easily children begotten by Javanese fathers, but not so those begotten by white men—is contradicted by the experience of Lehmann in Amsterdam, who often observed that Javanese servant-girls had easy labours.—*Monatsschr. für Geburtsk.*, July, 1866.

Peritonitis, consequent upon Purulent Inflammation of the Fallopian Tube, independent of Childbed. By Dr. v. Dessauer.—The author refers to the accounts given of this mode of origin of peritonitis by Förster and others. (The earliest case in which this connection was clearly traced, however, was described many years ago by Sir Benjamin Brodie.—R. B.) The following is Dr. Dessauer's case:—A woman had been married twenty years without pregnancy. After marriage she began to suffer from pelvic inflammation. In the last attack there was considerable meteoric distension and pain, preventing minute exploration. She died in a few days. Much fluid serum was found in the abdomen. Exsudations bound the intestines together. A considerable quantity of yellow creamy pus welled up out of Douglas' space. Both Fallopian tubes were much distended, a dilatation as large as a hen's egg being formed in the middle of the right one. The left tube, at the point of greatest distension, was ulcerated and perforated; the borders of the perforation were as if torn, not round; pus flowed out. The cavity of the tube was divided off into numerous spaces, forming small and larger cysts.—*Monatsschr. f. Geburtsk.*, Jan. 1866.

II.—Labour.

Inversion of the Uterus after Labour.—By Dr. G. Möller, of Hamburg.—A single woman had just been delivered rapidly and easily of a living child—her first. When seen she was very anæmic. The midwife reported that the placenta was expelled immediately after the child, and that when feeling through the abdominal walls for the fundus uteri she found it contracted; that the patient then suddenly complained of pain, and exclaimed that something had come away from her. The womb was found completely inverted; patient in collapse. The womb was quickly replaced by grasping with both hands and pressure in the pelvic axis. Death followed in a few hours; it was attributed to shock. There was no autopsy. —*Monatsschr. f. Geburtsk.*, June, 1866.

Complete Inversion of the Uterus, and separation of it by Gangrene. By E. Clemensen.—A woman, æt. 50, was admitted in the Frederick's Hospital, in July, 1865. She had, with an interval of four years, borne two children, the last thirteen years before. Both labours were easy. Some eight years ago she observed that the uterus prolapsed; menstruation, regular, but scanty, had ceased during the last six months. A month ago a profuse hæmorrhage took place, attended by sacral pains. The uterus was found between the thighs, the size of two fists, completely inverted. In several spots lacerations were observed, extending into the muscular tissue. Some days later the uterus seemed diminished in size; irritative fever set in; gangrene showed itself in the left side of the uterus. The uterus contracted more and more. At last only the orifice remained, as a scar. The woman recovered. The author attributes the origin of the inversion to the altered texture of the organ, resulting from fatty regression after labour.—*Hospital Tidende*, 1865.

On Inversion of the Uterus after Delivery. By Dr. Denham.—A woman, æt. 23, admitted five weeks after delivery of her first child. Expulsion stage rapid. Placenta forcibly extracted by the midwife ten minutes after birth of child, by traction on the cord and pressure on the fundus of the uterus. Patient suffered much pain during this operation, but suddenly got relief by the expulsion of a large tumour, which led her to exclaim, "Am I going to have another?" A continual hæmorrhage went on until admission, when she was much prostrated and anæmic. Her strength being somewhat restored by diet and medicine, an attempt at reduction was made, unsuccessfully at the time, but three days afterwards the fundus spontaneously returned. A profuse purulent discharge took

place for several days. She perfectly recovered.—*Dub. Quart. Journ. of Med. Sc.*, Aug., 1866.

On a Case of Complete Inversion of the Uterus. By Dr. CASATI.—Dr. Casati, Assistant-Physician to the Royal Obstetric School of Milan, relates an interesting case of complete inversion of the uterus treated by the method of Professor Lazzati. The patient was delivered naturally of her third child, living. The midwife exerting some slight traction on the cord five minutes afterwards, the uterus descended, and became completely inverted. Dr. Casati was soon in attendance. The patient had strained forcibly during the traction on the cord, feeling as if there was a second child to expel. Dr. Casati found the uterus *flaccid*, but the orifice contracted. It was reduced by applying the fingers of one hand to the fundus, whilst the other was applied to the abdomen, pressing down to the position of the neck; then by alternate movements, and using some force, the organ was reduced. It was then felt flaccid. The woman perfectly recovered.—*Annali Universali di Medicina*, Sept., 1866.

Accidental Hæmorrhage, complicated with Convulsions. By Dr. CAIRNS. — Dr. Cairns relates an instructive case of accidental hæmorrhage:—The patient was a primipara, affected with partial hemiplegia of the right side. At term, hæmorrhage occurred. The pains were weak, and had no influence on the os uteri. The hæmorrhage recurred, and then the membranes were ruptured. Labour went on, but in four hours a convulsive fit occurred. There was no œdema, and no albumen in the urine. Several fits recurred. She was delivered by long forceps of a living child. The placenta was found to contain a rupture, extending from the circumference to near the centre. Severe flooding set in on the third day, and recurred daily for a fortnight. She ultimately recovered. (It is extremely probable that the convulsions were caused by the intra-uterine distension arisising from the hæmorrhage. R.B.)—*Edin. Med. Journ.*, Feb. 1866.

III.—CHILDBED.

Report on the Post-mortem Examinations made in the Lying-in Institution of the Grand Duchess Helena Paulowna (St. Petersburg), 1862–3. By Dr. ERICHSEN.—The pathological conditions found in the dissections carried out during two years in the St. Petersburg Lying-in Hospital are classed under three following heads:—1st. Puerperal inflammation; 2. Puerperal thrombosis; 3. Puerperal ichorrhemia. Of puerperal inflammation three kinds are distinguished; peritonitis, phlegmon, and metritis. Dr. Erichsen describes a group of nineteen cases of universal peritonitis. He rejects the view of Martin, Buhl, and others, that endometritis and salpingitis is the starting point of all puerperal affections; and that in every case these

inflammations precede and cause the peritonitis. He found only eleven cases in which peritonitis was complicated with endometritis, and in eight cases of peritonitis there was no trace of endometritis. He found only nine cases of purulent salpingitis, whilst in ten cases the total mucous membrane was either normal or simply affected with catarrhal swelling. Of the nine cases of purulent salpingitis six were double, in three the right tube only was involved. He thinks it a question whether the salpingitis may not frequently be the consequence of the peritonitis.

To the foregoing cases the author adds eight cases of secondary peritonitis following upon other childbed affections. In these he sees evidence of a contagious character; that is, a tendency to rapid, continuous extension, and a disposition to involve other systems in the morbid circle; this is *puerperal phlegmon.* The feature of this is inflammation of the subserous cellular tissue of the uterus and appendages (Virchow's parametritis), then inflammation of the pelvic cellular tissue, and lastly of the subcutaneous tissue of the thigh and abdominal wall. The source of these diseases is mostly a diphtheritic process of the inner surface of the uterus, but the author saw genuine pelvic phlegmon and parametritis arise. Again, vein thrombosis of the pelvis and of the crural veins is frequently the cause of phlegmon. Generally the first form of disease spreads over the whole subserous tissue of the uterus and broad ligaments, which then seem to be pulpy, as if swollen from œdematous infiltration; at a later period there is formed in the same place a continuous layer of pus, whereupon a regressive metamorphosis, fatty degeneration, ensues. Not seldom he found small abscess-like formations, like strings of pearls, which proved to be lymphatic vessels filled with thrombi, and leading to inflamed swollen lymphatic glands.

Puerperal Metritis.—Of this condition he found only a few cases in which the whole organ was affected; but forms of partial metritis were very common. The seat of insertion of the placenta and next the vaginal portion were mostly much infiltrated. Diffuse metritis especially affected the interstitial tissue. He never saw interparietal abscesses.

2. *Puerperal Thrombosis.*—After Virchow, he divides this into three kinds, the placental, the dilatation, and the compression thrombosis. The formation of rapidly extending placentary thrombosis into the abnormally widened vessels is apt to pass into the dilatation thrombi which form in the pampiniform and uterine plexuses, the deeper pelvic veins and internal spermatic. The cause of the clotting is here the retardation of the blood-stream, caused by the abnormal dimensions of the vessels. The compression thrombi are found in the veins of the thighs; and are caused mostly, but not always, by the pressure of the gravid uterus. The most frequent issue of thrombosis is ichorrhæmia, sometimes proceeding from the softening of the thrombi and thus causing self-infection.

3. *Puerperal Ichorrhæmia.*—This is characterised by parenchymatous inflammations in the spleen, liver, and kidneys. The process is shown in its most intense form in those cases where the appearances

amount to acute atrophy of the liver which, he says, is caused by infection.—*Monatsschr. f. Geburtsk.*, Jan., 1866.

On the Ascertainment of the Temperature during Puerperal Processes. By Professor L. LEHMANN, Nederland Tijdschr. vor Genecsk, 1865.

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Some Remarks on Abdominal Typhus in Childbed. By C. HECKER. *Monatsschr. f. Geburtsk.*, June, 1866.

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Case of Tuberous Decidual Endometritis. By Dr. GUSSEROW.—*Monatsschr. f. Geburtsk.*, May, 1866.

Case of Spontaneous Rupture of the Uterus. By Dr. SCHWAHN.—*Monatsschr. f. Geburtsk.*, May, 1866.

Two Successful Cases of Ovariotomy. By OTTO SPIEGELBERG.—*Monatsschr. f. Geburtsk.*, May, 1866.

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THE

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MEDICO-CHIRURGICAL REVIEW.

APRIL, 1867.

PART FIRST.

Analytical and Critical Reviews.

Review I.

The Antecedents of Cancer. By Charles H. Moore, F.R.C.S. pp. 53. *London,* 1865.

The Inheritance of Cancer, and its Relation to Questions concerning the Local or Constitutional Origin of the Disease. By W. M. Baker.

St. Bartholomew's Hospital Reports. Vol. II. 1866.

On the Diagnosis and Treatment of Cancer and the Tumours analogous to it. By M. H. Collis, M.B. Dublin, &c. *London,* 1864. pp. 317.

Cancer: a New Method of Treatment. By W. H. Broadbent, M.D., &c. *London,* 1856. pp. 28.

That cancer is a local manifestation of a constitutional vice, in some cases inherited, in others, where inheritance cannot be traced, inbred in the system, it matters not how; that the essence of this unknown quantity or constitutional taint lives and lies dormant in the blood, and issues thence in form of an exudation or blastema into some one of the tissues, there to undergo development into a cancerous tumour,—is certainly the fixed opinion of some, and the general impression of the majority, of those who will read these pages. Such a doctrine has long been taught in our schools; it is promulgated by the ablest writers on pathology in this country; and, till a short time ago, it passed, almost unchallenged, as current amongst us. It has, however, within the last few years, received more than one rude

shock. The cellular pathology of Virchow dealt the first hard blow at it, and the pamphlet which has lately come forth from the Middlesex Hospital strikes at its very foundations. Although it is quite impossible, in the present state of our knowledge, to write or speak positively on this most important and interesting subject, yet it seems good, considering how ably the question has been argued on both sides,—and, as Mr. Baker wisely remarks, there is no subject that illustrates better than cancer how much may be said equally true and equally forcible on both sides of an argument,—to discuss shortly the arguments put forward by the supporters of the doctrine on the one side and its opponents on the other, endeavouring to act as fairly as possible the part of umpire in deciding to which side the balance of probabilities inclines. Is, then, according to Mr. Moore, "the causation of cancer solely in the part where its first independent tumour springs;" in other words, is cancer purely a local disease: or is the production of the local tumour a consequence of, and controlled by, some pre-existing morbid state of the body generally; in other words, is cancer a local expression of a constitutional disease?

Following, now, the order in Mr. Moore's pamphlet—cancer is supposed to be constitutional, first, *because it can be diffused through all organs and tissues of the body.*

The numerous tumours which can thus, often in a short time, be formed in various parts of the body, are regarded by the constitutionalists as, in part at all events, dependent on a pre-existing cancerous condition of the blood; by the anti-constitutionalists, as all derived secondarily from one primary tumour or "head-centre." The fact that, in some cases of such diffusion, a direct transmission, by the lymphatics and veins, of cancerous particles or cells, from one primary to other secondary cancers, can be demonstrated,[1] is thought sufficient to support the conclusion that all cases of cancerous dissemination depend on a similar transport of disease from one primary local dépôt. There is no doubt that, whenever a direct vascular continuity can be traced between secondary tumours and a primary centre (as from the stomach or intestines to the liver—from the breast, limbs, &c., to the lungs), it is highly probable, especially when it is remembered that cancer-masses[2] and cancer-cells[3] have been clearly found in the veins, that there is a direct connection between the primary and secondary tumours, a conveyance of cancer from one organ to another by means of the circulation;

[1] Paget, 'Lectures,' vol. ii, p. 579. Virchow, 'Geschwülste,' Bd. i, s. 52.

[2] Spaeth, Virch. 'Archiv,' vol. xxx, p. 432.

[3] Heller, 'Archiv für Mikrosk. u. Chem.,' 1846, Heft 1. Rokitansky, 'Allg. Path. Anat.,' 1846, s. 553.

in fact, a sort of cancerous embolism. But such a vascular connection cannot, in a large number of cases, be detected. Indeed, as Mr. Moore has himself shown in another article on the same subject,[1] secondary cancer of the lungs is rare; a fact there instanced to show that transportation of cancer-cells or particles and their arrest in one or another vessel are not proved, and to negative the supposition that either the absorbents or veins of the cancerous part take up any cancerous material whatever, or convey it to other parts of the body. How, then, to explain the cases where, according to the vascular relation of parts, the lungs ought to be the seat of secondary deposits, but where the lungs are free, and some other internal organ, as the liver, is filled with cancerous nodules? Recourse must be had to one of three hypotheses: either that minute cancerous germs, small enough to pass through the capillaries of the lungs, are set free and arrested in some distant organ; or that noxious juices are liberated from the mother-tumour and transmitted by the blood to distant parts, where they exude in form of a blastema, out of which secondary cancers are formed; or that similar juices, similarly transmitted, act as special irritants on certain parts of the body, and excite the formation of secondary cancers in the very tissues of the parts irritated. We conclude that a considerable number of cases occur of which it is impossible at present to speak with any certainty, so as to decide whether the many tumours are all equally dependent on a constitutional predisposition, or whether one primary tumour infects the rest of the body by yielding to the blood, and so dispersing abroad its cells, its germs, or its juices. The latter view, from being in part supported by facts, recommends itself before the former, which rests purely on hypothesis.

Secondly, *because of its occasional commencement in many primary tumours simultaneously*. The above-mentioned instances of general cancer dissemination cannot be distinguished from outbreaks of multiple cancer in *different* organs and tissues of the body. The arguments for and against both are the same; the one side maintaining that all the tumours are independent one of another, but dependent on a cancerous diathesis; the other asserting that there is but one primary focus from whence are secondarily derived all the other tumours, but with so rapid a spread as to give the appearance of a simultaneous outbreak. Mr. Moore treats this question rather summarily. He says, "these cases of simultaneous multiple cancer rest only on traditional authority." "The appearance of a contemporaneous origin of many tumours is due to the rank growth of medullary

[1] Holmes's 'Surgery,' vol. i, p. 547.

cancer, and to the rapidity with which its soft materials are conveyed to all parts with the circulation." It must be confessed that the local spread of a cancer to the tissues round and about it (*de proche en proche*) is so evident as to be incontestable; and that Virchow's description of cancer growth by the agglomeration of cancer units, which form indefinitely round a centre and become incorporated with it, is becoming more generally accepted as correct. It has also been shown how cancer may spread by way of the lymphatics and veins. At the same time, the difficulty of accounting in either of these ways for many cases of cancer dissemination or multiple cancer-formation has not been masked, and must here again be most strongly urged. In the class of cases next to be discussed, this difficulty seems still more insuperable, and the "travelling" theory less able to meet it. We allude to those cases referred to by Mr. Paget,[1] in which numerous medullary cancers are formed simultaneously, and all in *one and the same tissue*, the subcutaneous tissue. These tumours, occurring as they do in the subcutaneous tissue of different and remote parts of the body, cannot be supposed to spread by the agency of the lymphatics, for, as Mr. Moore remarks,[2] "there is no retrograde extension of cancer in the lymphatic system, *i. e.* against the current of lymph:" nor is it possible to conceive why they should be all in the same tissue if they had been transported from place to place by the blood. No one would think of suggesting such explanations as these to account for the fatty tumours which sometimes infest the same tissue in the same multiple form, and to which the multiple subcutaneous cancers bear in their relations a strong resemblance. We must, perforce, feel here that, if not in the blood, yet at any rate in the subcutaneous tissue itself, there pre-exists some unnatural tendency, to account for so remarkable an outbreak of tumours. There must be a constitutional predisposition, but not in the commonly accepted sense of the term. This question will be presently discussed. It remains, therefore, that this second difficulty cannot be settled by any one-sided doctrine; and that those who uphold, in all cases alike, local origin and diffusion, are met with arguments as unanswerable as those who would solve the riddle by referring all multiple cancers to a constitutional predisposition inherent either in the blood or in the tissues, or in both.

Thirdly, *because of its recurrence after operation either in loco or in internal organs.* This fact the constitutionalists have been wont to regard as one of the strongest in favour of their views.

[1] Op. cit., vol. ii, p. 384. [2] Op. cit., p. 545.

Surely, a tumour of which you cannot rid the body, but which, cut it away as freely as you will, returns either in the cicatrix of the wound or somewhere in the neighbourhood, or in some other part of the body, cannot be a mere local growth, having, besides local attachment, no connection with, or dependence on, the body generally. It is, in truth, nothing more, say the anti-constitutionalists: but so subtle is its growth, and so imperceptibly does it creep into and among the tissues around it, that no surgeon, careful though he be, widely though he cut, can hope to remove it. Microscopical fragments must be left behind and grow: or, if the recurrence take place away from the seat of operation, be sure that the tumour which seems to recur had commenced to grow before the operation was undertaken, and now only, after a time, has reached such a size as to be recognisable. Virchow, who is the main upholder of this doctrine, seems to imply by his writing that he has facts to support it, in the following words[1]: "it (the above doctrine) is no mere argument, but the direct result of observation, as any one may with the greatest ease prove for himself, if he will, in every case, carefully examine the cut borders of the part removed by operation." But this is not direct evidence. If he had written that he had himself carefully examined a hundred cases of scirrhous breast which had returned after operation, and could positively assert that he had in all cases detected cancer-cells along the very borders of the extirpated tumour, we must have weighed seriously his words. But until some such positive statement is made by himself or an equally able observer, we cannot accept as a fact that which seems so improbable when employed to explain away all cases of recurrent cancer. For see what happens! Growing germs are supposed to be left in the margins of the wound or somewhere in its neighbourhood; and yet years may and do elapse (twelve years in a case of Mr. Moore's) before the cancer returns. If the return of the growth could in all cases be followed on from the time of the operation or soon after it, there would be a strong reason for supposing that the explanation in question is correct: but the occurrence of a long pause or interval between the operation and the return of the tumour is very difficult to reconcile with any such theory.

The importance of the question cannot be overrated. If cancer be thus local, an operation undertaken when the tumour is yet in its infancy, and which shall cut far and wide of the disease, ought certainly in a large number of cases to remove the disease for ever. But, till surgeons can show a much greater

[1] Op. cit., p. 58.

number of favorable results than they show at present, we shall persist in doubting that cancer is a purely local disease, and must agree with Mr. Baker that the results of operation are among the least hopeful sources of support for any such notion.

Fourthly, *because of its hereditary nature.* And first, it must be asked—is cancer a hereditary disease, or is it not? To judge from Mr. Sibley's statistics,[1] the number of cases is so small (8¾ per cent.) in which cancer can be traced to any relative of the patient, that such an occurrence might be regarded as no more than accidental. But even Mr. Sibley concludes from the few but decided cases in which several members of the patient's family were affected, "that there is all but conclusive evidence that cancer is, to a certain extent at least, hereditary."

Mr. Baker's[2] statistics, however, which are more likely to show a correct average, as being taken from Mr. Paget's practice (whereas those of Mr. Sibley are from hospital patients) show that as many as 24·2 per cent. of all kinds of cancer have one or more cancerous relatives. This is much too high an average to be put down to mere coincidence; and it may be stated positively that cancer is a hereditary disease. If, then, it is hereditary, how is it inherited? Is it a malformation, a mere local defect? Or is it a general, *i. e.* a constitutional, affection? The fact of its inheritance is, as Mr. Baker clearly shows, no proof of its being a general disease. For, not to speak of deformities inherited, the group of tumours itself furnishes well-marked instances of local inheritance. Such are, in certain families, nævi ("a pisis Pisones, ciceribus Cicerones, lentibus Lentulos appellatos esse"), in others epidermal cysts (Paget and Virchow), in others fibrous tumours (Virchow). Mr. Moore, who allows that cancer is hereditary, believes it to be inherited as a local tumour, and remarks on its resemblance to "certain defects and peculiarities which pass from parents to offspring in corresponding organs." In short, he regards an inherited cancer as an inherited absence of the patella or an extra thumb. But, surely, such a comparison is open to the gravest objections. The one is congenital, the other not. The one is an exact repetition of the parental disease, the other often not. The one selects the same part or organ as in the parent, the other often not. And this brings us to the discussion of the very points in the argument which seem to bear most strongly on the nature of cancer. 1st. Does inherited cancer infest always corresponding organs in parent and offspring; or is it often enough seated in different organs to prove that it cannot be a mere local trans-

[1] 'Med.-Chir. Trans.,' vol. xlii, p. 111. [2] Ib., xlv, p. 389.

mission from organ to organ, but must be something more general? 2nd. Is the same form of cancer invariably inherited?—1st. If it can be shown that, in cases of inherited cancer, different organs of the body are attacked by the disease in near relatives or in successive generations, the doctrine of local inheritance is seriously weakened. Mr. Moore himself sees this difficulty, and allows that such cases do occur, that "near relatives do sometimes have dissimilar (*i. e.* in different organs) cancers." He then tries to escape from the difficulty thus: "Nevertheless, it may be asserted, though I cannot state the fact numerically, that this multiplication of dissimilar cancers amongst near relatives is exceptional." He therefore concludes that "the concurrences in question are so infrequent and uncertain as to be more probably due to accidental conditions in the lives of the several persons than consequent on their family alliance." Hear however, on the other side, Mr. Baker, who *can state his facts numerically*. He gives a carefully compiled table of 103 cases of cancer collected by Mr. Paget in their order of occurrence, of which 45 were directly inherited from father or mother. Of these, the disease was in the same organ in 19 only, *in a different organ in* 26. Again, in other 16 cases of inheritance from grandparent or great-grandparent, the disease was 8 times only in the same organ, 8 *times in different organs*. Again, in 49 families, two or more members of which had cancer, not being in relation of grandparent and grandchild or parent and child, the disease was 28 times in the same organ, 21 *times in different organs*. From which we may conclude that at least one half of the inherited cancers are transmitted to a different organ in the offspring from that which they affected in the parent. It may hence be deduced that *cancer is not inherited only as a local disease or deformity*. 2nd. The question of modification of the cancer-material in hereditary transmission is ably treated by Mr. Paget, who relates instances in which epithelial cancer in an ancestor was transmitted as medullary or scirrhous in the progeny, and *vice versâ;* or in which colloid and medullary, melanoid and scirrhous, were similarly intermingled. Mr. Baker's tables likewise enumerate several such cases. It may therefore, secondly, be concluded that cancer in a parent can be handed down in a modified form to the offspring. This conclusion seems equally to militate against the theory of local inheritance. That there should be any hereditary connection at all between the different kinds of cancer, speaks, as Mr. Baker remarks, rather for the constitutional than for the local nature of cancer generally. Now, if cancer is not inherited as a local disease, it must be handed down, in a manner at least, as a general or constitutional defect. What is meant

by this? The term constitutional, as generally understood, conveys to the minds of most the idea of a certain state or condition, not of the constitution or body as a whole, but of the blood as the body's representative. So long as the theory of plastic exudation or blastema held sway, it was necessary to look on cancer, as on other diseases, from this point of view, and to regard a cancer as a tumour whose elements pre-existed in the blood, not in a solid or cellular form, but in the shape of a fluid blastema endowed with a strangely procreative power. It will be well to examine shortly the evidence on which the existence of this blastema is supposed to depend. Cancer, it is said, has such close analogies with other diseases which are manifestly diseases of the blood, that it must of necessity be regarded in the light of a blood-disease. The kindred blood-diseases in question may be divided into two great classes, the contagious and the non-contagious. From the contagious, cancer might, *in limine*, be separated: for the instances of cancerous contagion quoted by writers are so very few that they may fairly be regarded as no more than coincidences.[1] But, even supposing it to be in rare instances contagious, how different its behaviour from that of the typical contagious blood-diseases, the exanthemata! In the one we have a distinct and recognised infection of the blood; a period of incubation during which the imbibed poison by some process or other multiplies; an attack of fever, evidence of the blood affection; an eruption, followed by manifest relief to the blood and the body generally; and, finally, a termination of the disease. In the other we have no evidence of any infection of the blood; no distinct period of incubation; no premonitory fever: but, without any precursory illness, one or more tumours, having nothing in common with the eruption of fevers, form in the body, and show no tendency to even a gradual spontaneous disappearance, but grow and multiply as persistent tumours. From the non-contagious diseases, which are manifestly blood-diseases, cancer differs quite as widely. To take gout as a type of these—a disease in which we are sure of the blood-poison, for it is tangible. In gout we have recurring outbreaks of disease preceded by symptoms of fever and general constitutional disturbance. We are sure that in these outbreaks the blood relieves itself of the noxious matter which it contains, for we find this matter deposited in various parts of the body, and, most important, the system is evidently relieved by the outbreak, the constitutional disturbance abates, and the individual is, temporarily at any rate, restored to health.

[1] See Paget, vol. ii, p. 543. C. O. Weber, Virchow's 'Archiv,' vol. xxix, Heft 1, u. 2.

In cancer we are not sure that any similar discharge from the blood has taken place, for the most careful investigation has failed to detect anything like the elements of cancer in the blood of the cancerous before a local outbreak has taken place; there is no fever to usher in the appearance of the cancer; and, most important, the system, instead of being relieved by this appearance, begins to suffer from the very date of it, and goes on suffering in a constantly increasing ratio with the increase of the cancerous deposit. It may be here urged by the humoral pathologists, that, even though there be no premonitory fever to usher in the first appearance of, say, a scirrhous breast, yet there is evidence of that which corresponds to the fever, viz. a precursory state of mal-nutrition or ill-health, a "primary cachexia," which shows that the blood is poisoned before the tumour appears. But this must be of very rare occurrence: for the cachexia of cancer in by far the larger number of cases is in direct proportion to the hæmorrhage, or discharge, and the pain entailed by the local tumour, or else to the necessary interference with nutrition that the position of the cancer must involve (as when seated in the stomach or œsophagus). Every surgeon must be repeatedly impressed with this fact, that a cancer, provided it occupy a site where it does not thus directly implicate nutrition, and where it is not attended with pain or hæmorrhage, may grow for some time in a person who shall be so plump and well nourished as to make the diagnosis of cancer, if general signs only were to be regarded, a most improbable diagnosis. We entirely coincide with Virchow and Moore in regarding cancerous cachexia as a secondary effect. Again, if it be true that in cancerous tumours "are incorporated peculiar morbid materials which have accumulated in the blood," and if it be true that cancer is inherited as a blood-disease, one would expect to find, at least for some time before the first local appearance of the tumours, if not throughout the previous life of the individual, an unhealthy state of the body, some evidence of faulty nutrition, considering how important an agent in nutrition the blood is. But nothing of the kind is observed in cancer: nay, the very contrary obtains. This interesting question has been very fully entered into by Mr. Moore, and his inquiries enable him to make the following statement: that "cancer is eminently a disease of persons whose previous life has been healthy, and whose nutritive vigour gives them otherwise a prospect of long life." Yet another difficulty in the way of this humoral doctrine is to be found in the very case already brought forward in discussing the question of multiple cancer-formation; the case, namely, of multiple subcutaneous cancers. It is difficult to believe that cancer is here an exu-

dation from the blood, when all the tumours are in the self-same tissue. It seems rather as if the subcutaneous tissue itself were impressed with some imperfection which results in the outbreak of a number of cancerous tumours; just as in another individual some similar, but not identical, defect subjects the same tissue to an eruption of multiple fatty or fibrous tumours. And we are thus brought to recommend for adoption the theory which seems to meet the greatest number of difficulties—that the term constitutional shall be applied to cancer from the stand-point, not of a humoral, but of a solid pathology. On the supposition that the tissues, and, more especially, the connective tissue, can be stamped by inheritance, or otherwise, with an unnatural disposition, in consequence of which cancers shall form at some period of the life of the individual, we are no longer compelled to hold the very improbable theory of a cancerous dyscrasia; we can understand how it is that cancer may be inherited, and inherited in dissimilar organs, the connective tissue pervading every organ; we can account for the local recurrence; we can explain the formation of multiple tumours in the same system (the subcutaneous tissue), or, if necessary, in different organs. The great doctrine of the cellular pathology—that the connective, or a kindred, tissue is the mother-tissue of all cancer—must, it is true, be assumed as correct: but such an assumption is becoming daily more and more warranted by facts and observations.[1]

The endeavour has thus been made to show how impossible it is, as our knowledge at present stands, to hold a strongly one-sided opinion as to the nature and origin of cancer. Careful observation of the various forms and manifold phases of this most wonderful disease only strengthens more and more the conviction that those who uphold only the constitutional doctrine are as much in error as those who would regard all cancers as local diseases. There are, no doubt, cases which, seeming to originate in a blow or other local irritant as local tumours, infest the system by secondary diffusion and tempt observers to an easy generalisation. But how very many instances there are where no such exciting cause is traceable!—or, to put the case in another way, how many thousands are daily receiving blows and injuries in various parts of the body without becoming the subjects of cancer! Virchow,[2] who naturally makes the most of local irritation, points to the great frequency of cancer in the stomach and uterus, and concludes "that those organs which have a soft surface and which are exposed to contact with

[1] See Turner, 'Ed. Med. Journ.,' April, 1863, p. 873. See, too, his edition of Paget's 'Lectures,' p. 790.

[2] Op. cit., p. 83.

foreign bodies are much more often the seat of tumours than those which lie enclosed in the midst of cavities, have but little communication with external agents, and are less often exposed to direct influences from without." Mr. Sibley's observation, that cancer of the uterus is rare in single women, and that the women affected with it have had, as a rule, much larger families than those who have cancer in other parts, accords with this theory. But, on the other hand, cancer will attack the virgin uterus: cancer is found repeatedly in the female breast, rarely in the ovary, which is the seat of more active changes than the breast. How explain the fact that cancer is not at all uncommon in the sigmoid flexure of the colon and in the rectum, but very uncommon in the jejunum, parts of the intestine that must be exposed to similar external influences? It may be that Virchow is working in the right direction, but we have yet to be convinced that this local irritation will account for a large proportion of cancers.

Mr. Collis's book has done great service to surgical pathology in several ways. Its principal and most praiseworthy aim is to show that there is nothing specific in a cancerous tumour which separates it by wide points of difference from the group of tumours generally. Neither in its anatomical structure, nor in its connection with surrounding parts, nor in its physiological characters, nor in its clinical history, is there anything peculiar to a cancer which should isolate it among the tumours. In the shape and size of its cells, in its tendency to infiltrate, in the possession of properties which give it the name of malignant, it has nothing that may not be shared, though in a less degree, by other members of the class of tumours. "Once," he says, "the analogies which exist between tumours anatomically became clear to my mind, the recognition of their clinical points of contact and divergence became clear also."

The first chapter in the book is, accordingly, devoted to the consideration of the "anatomical analogies" of tumours, under the head of "Various Forms of Cell-growth." He begins with the Virchowian doctrine, that the lymph or connective-tissue cell is the starting-point of all morbid infiltrating growths —the cancers and the tumours allied to them. This cell may simply multiply without any material increase in size or development, so as to make up a tumour composed of myriads of such cells and nothing more (the "fibro-cellular" tumours, including the "fibro-nucleated" and "recurrent fibroid;" all showing a marked tendency to recur *in loco*). Or the cell may not only multiply, but develop into fibre (the fibrous tumour). Or it and its progeny may increase in size and assume various forms, becoming caudate, fusiform, large and many-nucleated;

whence arise tumours many of which have the closest affinities with the cancers (the "fibro-plastic" and "myeloid" tumours). Or, lastly, the cell may deviate still farther from the simple and original type, and a cancer results. The cell-elements, therefore, of all these tumours have a common parent in the lymph-cell. In keeping with this theory of cell-development is his classification of the above-mentioned tumours, which are grouped together under the head of "cancroid" tumours; a term taken from epithelial cancer, or, as he calls it, epithelioma, and applied to these tumours, which closely resemble, but are not quite, cancers. This cancroid group, then, forms a link which binds the cancers with the other tumours. That Mr. Collis is, so far, a staunch advocate of the cell-pathology there can be no doubt. The above theory is almost identical with that propounded by Virchow in his book on Tumours; and Mr. Collis's cancroid tumours come, as a result of independent work, to be nearly identical with Virchow's "sarcoma."[1] Again, it is written at p. 8, the lymph-cell "is developed from the same parent-cell as the cell of connective tissue." Or again, "wherever connective tissue is found, there may cancer spring up." But in another part of his book he seems to have gone back to Schwann, when he writes about "the cell of plastic lymph poured out for the repair of injury." The exudation of plastic lymph is unknown to cellular pathologists.

In treating of cancers, he has written five excellent chapters on scirrhus worthy of a good practical surgeon, with cases illustrated by some good lithographs. He speaks very highly of the good effects of ice as an application to scirrhous breasts, both as a sedative and to allay the attacks of acute inflammation to which such breasts are liable.

The cancroid tumours are subdivided into two groups: 1, the fibro-plastic (including myeloid), which come nearest to the cancers; 2, the fibroid (including fibro-nucleated, recurrent fibroid, Laennec's "colloid," and a tumour described by Mr. Collis as "fibrinous"), all recurrent, but rarely malignant. Jacob's ulcer, or the "rodent ulcer," concerning which two opposite opinions are held; some regarding it as a form of epithelial cancer, others as a non-cancerous ulcer, Mr. Collis places among the cancroid tumours. He denies that it has anything in common with epithelioma, but looks on it as a form of fibro-plastic disease which ulcerates; for he has always found the margins, when examined microscopically, composed of a tissue identical with that met with in fibro-plastic tumours. Mr. Paget, whose graphic description of this ulcer is well known to all, has likewise shown that its borders and base are

[1] See this Review for Jan., 1866, No. ii.

composed of granulation-tissue, and contain no trace of cancerous elements. We are surprised to see that Mr. Moore treats it as a cancer, and calls it "rodent cancer," "from the peculiarity of its local progress and its common hostility to every indigenous texture." If that were all, the ulcers of syphilis and lupus are equally entitled to the denomination of cancer.

The tumours called "fibrinous" originate in a distinct effusion of blood, and are generally traceable to a blow or injury. They are composed of the ordinary elements of blood-clot undergoing organisation, the coagulum being sometimes arranged in concentric layers, as in an aneurism. He hazards here a most singular theory in connection with the organisation of clots. He considers that this process commences at the outside of the clot "from surfaces or points of living tissue possessed of cells and nuclei," and that "it requires pre-existing nuclei and cells." But, he adds, throwing again a sop to Schwann, "it is equally certain that the new cells are formed out of the very substance of the coagulum as long as it possesses vitality, which will be as long as putrefactive changes have not set in." Again, "the new cells which spring from the living tissues into the clot make use of the substance of the clot for their development as freely as they make use of material supplied to them from blood still circulating in the vessels." Now, if the statement be right, as we believe it is, that the organisation begins at the surface of the clot from points of living tissue, what need is there of any so very improbable hypothesis as that developing cells, already deriving sustenance from the living tissues with which they are in contact, feed on blood which has coagulated?

Colloid Mr. Collis considers to be removed from cancer both structurally and clinically. All the tumours of the kind which he has seen contained agglomerations of the smallest lymph-cells held together by the finest connective tissue, but never large cells like those of cancer. He therefore calls them "colloid tumours," rejecting the term colloid cancer. The tumours here described are not cancerous tumours, but are evidently of the same kind as Laennec's "colloid," Joh. Müller's "Gallert-geschwülst," some of the softer varieties of Paget's "fibro-cellular tumours," and Virchow's "myxoma" or mucous tumour. To the last of these authors[1] we would refer any one who wishes for the fullest and ablest account of the matter that has ever been written. Mr. Collis has rightly classified these tumours in his cancroid group, for, though not strictly cancerous, they very frequently return *in loco* after extirpation, and occasionally manifest the worst features of malignancy.

[1] Op. cit., Lect. xv.

But, at the same time, there is a variety of true cancer which is very properly called "colloid cancer," for it is cancerous in its anatomical as well as in its clinical features. Lebert's[1] cases cannot be otherwise interpreted. Förster, who has written an excellent paper on the subject,[2] states that scirrhous or medullary cancer may undergo a transformation into colloid; so that perhaps these true colloid cancers may be no more, as was long ago suggested by Paget,[3] than cancers whose elemental structures have become colloid. Förster has actually traced the colloid metamorphosis in cells, and describes it as advancing in three different ways. First, the cell-contents themselves become transformed into a colloid mass, which gradually increases in size and is set free. Secondly, the colloid change commences in the form of a zone round the nucleus of the cell; another zone is laid on this, and then another, till the well-known large laminated colloid body, so like in appearance to the laminated capsule of epithelial cancer, is formed:[4] the central nucleus at the same time undergoes partition and multiplication, and a group of nuclei fill up the centre of the laminated body. Thirdly, the nucleus is itself the seat of metamorphosis, and swells up so as to be converted into a great colloid bladder. The different consistence and appearance of the colloid tumours generally seem to depend on the different ways in which the colloid substance is distributed. For there may be colloid cells in pure fibrous stroma; or cells with ordinary albuminous contents in a colloid stroma; or colloid cells in colloid stroma.

Mr. Collis's chapter on *Epithelioma* will well repay perusal. He has evidently made of this subject a special microscopical and clinical study. The disease is, for him, not a cancer, nor even a cancroid tumour, for its affinities with cancer are not strong enough to place it in the cancroid group. The disease originates in the deeper layers of the epidermis, where its progressive development may be divided into three stages:—1st, the stage of simple cellular hypertrophy; 2nd, that of hypertrophy with ulceration, in which the number of cells has so increased as to press down on the papillæ and cause ulcerative absorption of them; 3rd, hypertrophy with infiltration, in which we find "enlarged papillæ topped with masses of cells, which not only press on and ulcerate the papillæ beside them (*i. e.* the cells), but also push the new-formed cells downward between them" (*i. e.* the papillæ). The wide-spread devastation which an epithelioma may thus cause, results from this continuous cell-formation, which gradually presses before it and destroys whatever tissues it meets with. But this is not all.

[1] Virch. 'Archiv.,' B. iv, p. 192.
[2] 'Würzb. Med. Zeit.,' B. iv, Heft., 5, u. 6.
[3] Vol. ii, p. 523.
[4] See Lebert, 'Path. Anat.,' Pl. xliv.

The cells may push their way between the separated papillæ, and, arriving there, irritate the blood-vessels so as to produce an interstitial effusion of plastic lymph. In this lymph the epithelial cells develop fresh nuclei and cells similar to themselves, and become a fresh centre of irritation. The disease then advances by fresh development of cells in the "plastic effusion" which fills the interstices of the tissues attacked. Here again, though a cellular pathologist at heart, he cannot quite part from the blastema theory; he will not tear himself altogether away from this early attachment. It can hardly be believed that two so opposite theories as the blastema and the cell-pathology can both be correct. Or, if he means that the cell-formation in this case is entirely out of pre-existing epithelial cells and only *in the midst of* this plastic effusion, the cells feeding on the lymph as they were before supposed to feed on blood-clot, why is the effusion called "plastic?" Nutrient it would be, but not plastic. The plastic power is in the cells, not in the lymph. In spite of Mr. Collis's great authority on this subject,[1] we cannot agree with him in separating this disease from the cancers. Mr. Paget's arguments in the opposite direction are not sufficiently answered by him. Recent observations are constantly adding to the number of cases in which epithelial cancer (for we still hope to see it so named) spreads to distant organs of the body in so truly cancerous a manner as to be convincing. Weber[2] has recently related in full several such cases observed by him, in which secondary deposits of true epithelial cancer were found in internal organs, lungs, heart, and liver. Such cases as these; the cases mentioned by Paget[3] in which epithelial cancer in the parent was inherited as medullary or scirrhous in the offspring, and *vice versâ*; and, lastly, the occurrence of epithelial cancer in one organ, of scirrhous in another, simultaneously, in the same individual,[4]—all point to the conclusion that epithelioma ought to be classified with the cancers as epithelial cancer. Whether this disease has its origin in the cells of the epidermis and epithelium, or in those of the connective tissue of the cutis and mucous membrane, must still be regarded as a moot point. As advocates of the former theory stand Hannover, Collis, and Thiersch; as upholders of the latter are arrayed Virchow, Förster, Weber, and others of the Berlin school. It is, indeed, hard to say whether the careful observations and beautiful

[1] See 'Dublin Journal of Med. Science,' vol. xxix, p. 319.

[2] "Ueber die Entwicklung des Epithelial-krebses in inneren Organen," Virch. 'Archiv.,' vol. xxix, Hft. 1 and 2.

[3] Vol. ii, p. 460. See also Baker's last tables for similar instances.

[4] 'Catalogue of the Path. Museum of St. George's Hospital, 1866, p. 611, Case 84.

drawings of Thiersch[1] bring with them more conviction than the no less precise descriptions of the microscopy of epithelial cancer by Weber. The latter finds the nuclei of the cells of connective tissue, as well as those on the small arteries and veins, to be by their partition and multiplication the originators of the disease. His investigations have been made especially into the structure of secondary epithelial deposits in the liver and other organs. The former has made his sections almost exclusively from specimens of the primary disease. We hope to find this question fully discussed, and perhaps finally settled, in the third volume of Virchow's book on Tumours. The different plastic operations in epithelial cancer of the lip are described and illustrated by a series of drawings which will supply the surgeon with some useful hints in practice.

Mr. Collis believes that there are cases of *Melanosis* in man which are not, necessarily, cancerous. He describes an interesting case of what he calls "cystic melanosis," which has already been alluded to, with a short account of the subject, in this Review (for July 1865, p. 39).

The woodcuts in this book, from drawings by the author, are most beautifully executed; and some of the lithographs are equally praiseworthy.

Dr. Broadbent, reasoning upon the fact that acetic acid does not coagulate the albumen of the tissues, and upon the effect produced by that acid on the cells of cancer when placed on the microscopic slide, has made the attempt to destroy cancer by injecting into the tumour the dilute acid, in the hope that this acid would *diffuse* itself through the tumour and dissolve the cancer-cells. His expectations have been, in a great measure, realised. The four cases which he relates show that, by a process more like that of solution than of sloughing, and, if the operation be carefully performed, without pain or constitutional disturbance, acetic acid acts most powerfully on cancer. He injects the acid, diluted with about three parts water, in considerable quantity (30–40 minims at a time), attacking the tumour from different parts and by repeated operations. The great advantage of this plan is that it can be made painless. The fact is remarkable, that if the acid be injected into healthy tissues, great pain is experienced; but that if it penetrate fairly into the body of the tumour, the operation is devoid of pain. As the plan is being extensively tried in London and elsewhere, we shall soon have further evidence to show how far, and more particularly in what cases, it may be undertaken with a fair prospect of success.

[1] 'Der Epithelial-krebs,' Leipzig, 1865.

Review II.

Twentieth Report of the Commissioners in Lunacy to the Lord Chancellor. London, 1866.

Eighth Annual Report of the General Board of Commissioners in Lunacy for Scotland. Edinburgh, 1866.

The Fifteenth Report of the District, Criminal, and Private Lunatic Asylums in Ireland; with Appendices. Dublin, 1866.

The course of time brings before us again the official statistics of the insane in Great Britain. The three blue books preserve their distinctive characters, their general plan being the same as in former years. As insanity is the one disease of which the State takes the control, the profession might expect something from it in return; but, as far as London is concerned, we confess we are disappointed. We learn from the Whitehall Board how many lunatics there are, how authority has been set at nought or neglected; the defeats of the Commissioners are gently touched on, and we find where they have interfered: the only other inference we can draw is that more asylums are being built, and still more are required. The Scotch and Irish Reports are far more ample and serviceable, and the dry details of routine duty are relieved by information which is welcome to those of the outside world who are anxious to learn somewhat of the social or statistical aspects of insanity.

The number of insane in asylums in England and Wales on the 1st of January, 1866, was 30,869, of which 23,414 were in county and borough asylums; but there were also 9,756 in workhouses (in 1865), making a total of 40,625 recognised lunatics.

In Scotland the statistics are only made up to 1st January, 1865, when the insane were thus distributed:

In Public Asylums	3125
In Private do.	788
In Parochial do.	497
In Lunatic Wards of Poorhouses	428
In Private Dwellings	1630
Total	6468

Of these, 3005 were males, and 3463 females.

In Ireland, on the last day of 1865, there were—

In Public Asylums	4835
In Private do.	583
In Gaols	505
In Poorhouses	2733
In Lucan (Government)	64
In Central Criminal Asylum	125
Total	8845

Of these, 4403 were male, and 4442 females.

Of the lunatics in England and Wales, 14,630 were males, and 16,239 females.

The most serious case mentioned in the first of the three Reports is one of homicide at the Wakefield Asylum, which illustrates how such acts are committed under the influence of delusion. The victim in this instance was an old case of harmless dementia, and the aggressor was a young man of 23, with suicidal impulses and auditory hallucinations, who, during the absence for a few minutes of the attendant, killed the patient by striking him on the head with an iron shovel. "The bones of the face were extensively fractured, and the blood-vessels and other structures between the face and base of the skull were lacerated in such a way as to have caused almost instantaneous death." The murderer "subsequently said that he was led to commit the act by a sudden impulse which he could neither explain nor resist; that for some time he had been under the impression that he should die an unnatural death; and that on the morning of the murder the voices kept telling him, "Kill a man, and you will save yourself."

The man was, after a trial, sent to Broadmoor Asylum. The chief topics in the rest of the Report are such as concern individual asylums—cases of ill-usages, or matters which have no interest outside this specialty.

We cannot refrain, however, from noticing a characteristic piece of obstructiveness on the part of the Wigan Workhouse authorities. The Commissioners, after visiting the house, advised the removal of iron bars from some windows, and an improved dietary; but the "guardians decided that no changes were required, and the medical officer to whom the subject of diet was referred, was of opinion that any *increase would be likely to produce injurious rather than beneficial effects!*"

The position of pauper idiot children continues to attract notice, and it is to be hoped the benevolent efforts for their proper treatment will be continued and extended, such cases being quite unfit for association with the adult insane.

As regards the Scotch statistics, it is stated that the proportion of female paupers to 100 male is 288·3, and that of

female pauper lunatics to 100 male is 117·7. The amount of pauperism and of pauper lunacy does not vary much from year to year for the whole of Scotland; but in different counties the variation is considerable. Thus, in Forfar county, the proportion of pauper lunatics to paupers per 1000 is 105; in Perth, 97; in Edinburgh, 92; in Haddington, 76; in Fife, 74; in Argyle, 69; in Inverness, 63; in Linlithgow, 55; in Ayr, 46; and Renfrew, 42—(omitting decimals).

But the amount of pauperism or of lunacy to the whole population does not at all preserve the same order; Forfar, which has the most lunatics among its paupers, having only one county with less paupers than itself. The table given by the Commissioners (p. xxxiii) to illustrate this, clearly indicates, they say, "a preponderance of pauperism among the Celtic portion of the population; but, as has been already stated, it does not appear to support any special views regarding the prevalence of insanity in certain districts or among certain classes."

The number of those in public asylums increases yearly, while those in private dwellings diminish. The cost of pauper maintenance is, on an average, about 1*s.* 1*d.* a head per diem; being about 1*s.* 4*d.* in private asylums, a farthing less in public, 1*s.* 0½*d.* in poorhouses, and 6*d.* in private dwellings. Taking the general average for all Scotland, the pauper lunatics are distributed as follows:—53 per cent. in asylums, 17·1 in poorhouses, and 29·9 in private houses. The recoveries, for the year, on the admissions, were for the public asylums at the rate of 36 per cent. for both sexes, and the deaths 6·7 per cent. for males and 6·9 for females.

At p. 133 we noticed a very good table giving the causes of deaths amongst the insane in asylums during the year. The average that have been resident in these establishments was, for men 2341, and women 2697. Of deaths from cerebral and spinal disease, 88 were male and 50 female; from thoracic disease, 50 male and 77 female; from abdominal disease, 14 male and 20 female; and from general debility and old age, 17 male and 28 female. Of particular diseases, it may be remarked that 38 were from apoplexy and paralysis; 18 from epilepsy and convulsions; 34 from general paralysis (only 8 females); 12 from maniacal and melancholic exhaustion; and 24 male and 12 female from organic disease of the brain, tumours, &c.

In Ireland, the average cost per head at the seventeen asylums now in operation is £20 7*s.* 6*d.*; an average which, if based on the same items of expenditure, is probably 30 per cent. less than in England, where, as the inspectors say, there is more appearance of comfort and more social enjoyment in similar

institutions; "for it would be idle to deny the difference which exists in domestic habits and surroundings between the humbler classes in the two countries. In fact, in Irish asylums, by this very difference, a double duty is imposed—that of treating the patient for his malady, and not unfrequently also of teaching him for the first time to observe habits of neatness and order."

It is rather amusing to find that at the Belfast Asylum a majority of the directors still persist in refusing a chaplain admission to the asylum, on account of the religious discords so prevalent in the town; and we feel disposed to agree with the remark that "to carry out the principle enunciated by the Board and justify it, if consistency be one of its elements, churches and chapels all through the town should be closed, and the inhabitants precluded from public worship—a proposition not more anomalous upon a large scale than a small."

There are some peculiarities in the Irish statistics. Thus, for instance, the diminution of the population from emigration and other causes materially affects the amount of insanity:

"For although the decrepit and mentally affected are, for the most part, left at home, they gradually die off in poorhouses—the only receptacle open to them. The reverse naturally obtains in England, where each decennial period shows a large increase to its inhabitants, and with it an apparent spread of insanity, and the greater necessity for additional lunatic accommodation."

Another peculiarity, too, to which we have before referred in this Review, is the small amount of idiocy in Ireland. We read (p. 13) that, in workhouses,

"Many are to be found utterly demented or destitute of all reasoning power, the result of cerebral disease, or of a protracted series of epileptic attacks; but *idiocy, in the true acceptation of the term, is rare in Ireland:* neither from the inquiries we have made is there reason to think that there is much of it in the poorer classes throughout the country. Certain it is, that in all the union workhouses and asylums in Ireland there are only 1175 simple idiots reported to us by the medical attendants of these establishments."

The recoveries on admissions in the public asylums are at the rate of 46·16 per cent. The total mortality in the same, for the year, was 400; of which 69 were from abdominal affections; 98 from cerebral, 120 from pulmonary, and 9 from cardiac diseases.

The gaols continue to be used most unfairly as depôts for outrageous lunatics; the number of those committed to and confined in them in 1865 being 801, making the total in prison, during the year, 1242, of whom 395 were sent to asylums, 290

were discharged by order of the magistrates as recovered or no longer dangerous, and 37 died. The gaols in Ireland are thirty-nine in number, but the distribution in them of the lunatics is by no means equal, and eight of them contain 315, viz., those of Dublin, Donegal, Down, Monaghan, Mayo, and Wexford. The system, as the inspectors remark,

"If it has much to be complained of in itself, is not devoid of practical utility; in saying so much, however, we do not mean to uphold it, except, so far as it is, under existing circumstances, an absolute necessity; however, it is now gradually but distinctly on the decline."

The Dundrum Criminal Asylum maintains its old efficiency and high character.

The number of private asylums in Ireland is small, about eighteen, and the total number of patients in them was 583 at the end of 1865.

The chief impression left on one's mind, after the study of these Reports, is that the number of lunatics is increasing yearly for whom public provision has to be made, and the great question is how this can best be done. It is one which involves political, personal, and professional considerations. There are the rights of the individual and the public to be thought of, as well as the question of health; and the subject is one which presses every year more urgently on public attention. In England, the one remedy is an asylum,—for the use made of workhouses is very limited; and all that the Commissioners can bring themselves reluctantly to say on this topic, in their last Report, is the following, which, we think, looks rather like the process known vulgarly as "damning with faint praise":—

"Enough has been done to show that under proper regulations, provided no recent cases are received, and with the protection afforded by rendering necessary the keeping of medical and other statutory records, a larger proportion of imbeciles and old chronic cases of insanity might without impropriety be retained in workhouses, and the pressure for increased accommodation in county asylums be thus proportionably reduced."

In order to put the subject more clearly before our readers, we will add the other official opinions.

The Irish inspectors observe—

"As in England, so in this country, there must eventually be an intermediate place for a portion of the chronic or incurably insane; otherwise the building of asylums should go on *ad infinitum*, at a vast expenditure both for erection and maintenance. We think the difficulty admits of an economic and easy solution, as already defined by us."

They then refer to their last Report, in which they gave a scheme for utilising certain workhouses for the idiots, demented, and epileptic.

The opinions expressed in the Scotch Report are more emphatic still. Dr. Mitchell shows (p. 238) that by the plan of placing certain of the insane in private dwellings, a saving of £15,000 a year at least is effected to the country; adding—

"No saving, however, should be sanctioned which would be injurious to the interests of the insane poor. This would defeat benevolence, and would not be a wise economy. But the *amount of happiness acquired is not always in proportion to the sum paid out;* and this is as true in public as in private affairs. The mode of expenditure influences the extent of the benefit, which is the return. So far as concerns the insane poor, the soundest philanthropy seems to me to be that which makes a reasonable provision for the care of the *whole;* and *all* pauper lunatics in Scotland are held to be under the guardianship of society or the State. The care of some of them cannot be carried properly out except at a large cost, and this should be liberally given. Others, again, can be properly cared for at less cost, and here a saving should be effected. There are *now* about 1500 incurable and inoffensive fatuous paupers in Scotland disposed of in private dwellings, for whose care, in my opinion, a reasonable provision has been made, and whose happiness and comfort would not be increased by any other mode of management. They enjoy life more and will live longer than they would do if placed either in poorhouses or asylums, and to leave them where they are is the course which is at once humane and economical."

Again, he says—

"The forms of disease which, in my experience, have been found to be most suitable for care in private dwellings, are idiocy, imbecility, and dementia; and of the existing single patients more than 80 per cent. labour under these forms of insanity, 60 or 70 per cent. being idiotic or imbecile. The class sometimes spoken of as the "semi-insane" do not, as a rule, prove easily managed in private dwellings; nor do those patients labouring under "delusional insanity," especially if the delusions be those of suspicion. In the class of cases which I have found to be most suitable the unsoundness of mind is well marked, but in the direction of weakness or destruction rather than of perversion of the mental faculties. Their mental state should be one of defect rather than disease, and should be a settled and well-established condition, and not a progressive or changing one.

"The class, therefore, which my experience has shown to do best in private dwellings, is that class which it is often desirable to remove from asylums, either to obtain space, or to check excessive accumulations."

Dr. Paterson (at p. 252), in his report, says—

"It has recently been a subject of discussion among psychological physicians and others, whether the class of single patients might not be still further extended by systematically weeding out of asylums many of their chronic and incurable cases, and so enabling these institutions, which have been erected and are maintained at a great cost to the public, to meet the growing demands for fresh admissions without the necessity of erecting additional buildings. The subject is one which involves not a few considerations of difficulty and importance, on which differences of opinion will naturally prevail, and, probably, is hardly yet ripe for a decision. But if anything is calculated to render such a course practicable and safe, it appears to me to be the policy which the Board of Lunacy has from the first consistently followed, of recognising the fact that certain classes of lunatics, amounting on the whole to a large total, do at the present time reside in private houses under the care of relatives and other persons in their own condition of life, and of directing its efforts to raise and improve the character of the provision thus afforded. As one of those to whom it has been entrusted to carry out this policy, I feel bound to say that every year evidences of its beneficial effects are seen, and that, with some local difficulties still to contend with, and a very few flagrant exceptions, there has been a perceptible advance in the scale of allowances, the clothing, and the general accommodation and treatment of single patients throughout the country."

These extracts are, we think, enough to convince anybody that the plan referred to is a practicable and useful one, and that there is some better alternative open than that of indefinite asylums. We made some remarks last year on this subject, and return to it now because of its importance. As might be expected, our comments were not to the taste of some asylum superintendents. One of the best known of them appeared in defence of the aggrieved Commissioners—talked of Dr. Mitchell as "indulging in high-flown theories," and asserted that the Scotch Board did not sanction them. But what is the fact? There are 1630 lunatics in private dwellings under "official sanction." The truth is, that the men who rule in the public asylums have but little experience of any other plan than treating lunatics *en masse,* on the same scale, whether curable or not; and as their "standard of requirements" is pretty much what they fix themselves, it is not wonderful that they do not like a less costly one. There is no real experiment in the matter, for the nominal governors of asylums are led by the only person who knows anything of the subject, as a rule. Indeed, we are naïvely informed that the "*unvarying tradition of the Board* (*i. e.* at Whitehall) has been to bring into prominence and *in every way to exalt the position of the medical superintendents,*" which is, doubtless,

very gratifying to the gentleman alluded to, but, we must add, very unfortunate for the public—for, however high his personal character, he is fallible; and it is rather hard that the important interests of a whole country should be left to the mercy of one individual, particularly when, as is quite possible, he also might indulge in "high-flown theories!" The question of the future disposal of our lunatics is one of general interest, and will, before long, excite a greater amount of attention. We do not advocate any particular system, but we point out what is done elsewhere; and those who are most concerned in it would do well to study it in all its aspects before the reaction against the present needlessly expensive system comes. The conservative "traditions" of the English Board may even undergo a process of differentiation into a higher type, less consolatory to superintendents.

Review III.

De la Tuberculose, ou de la Phthisie Pulmonaire et des autres maladies dites scrofuleuses et tuberculeuses, étudiées spécialement sous le double point de vue de la nature et de la prophylaxie. Par le Docteur L. Perroud, &c. &c. Mémoire couronné par la Société Impériale de Médecine de Bordeaux, dans la séance publique du 25 Janvier, 1861. *Paris:* Savy. Pp. 292.

On Tuberculosis, or Pulmonary Phthisis and other scrofulous and tubercular complaints, with special reference to their nature and prophylaxis. By Dr. Perroud. An Essay which obtained the prize awarded by the Imperial Medical Society of Bordeaux at its meeting on the 25th January, 1861.

The stream of medical science has too much resembled that of the fabulous Alpheus; its theories and modes of treatment have disappeared from one age or country only to rise anew at some more distant date or place, decorated afresh with fancied virtues, and sparkling with many-hued hopes as brilliant, but, alas! too often as airy and unsubstantial, as the air-bells on the foaming stream.

In the early ages of medical science, Hippocrates, the great founder of our art, distinctly pointed out that diagnostic phenomena were to be heard on applying the ear to the chest; yet for more than two thousand years this valuable idea was allowed to slumber amid the unavailing lamentations of practical phy-

sicians: "Oh, quantum difficile est curare morbos pulmonum—oh, quanto difficilius eosdem cognoscere!" And when half a century ago this mournful wail was answered by the glad Eureka of Laennec, the science of medicine was thought to have doubled its realms, and chased doubt and despair from a very large portion of the kingdom of death.

Now—stethoscope in hand—

> "The grave physician
> By the trembling patient stands,
> Like some deftly skilled musician;
> Strange! the trumpet in his hands,
> Whilst the sufferer's eyeball glistens,
> Full of hope and full of fear,
> Quietly he bends and listens
> With his quick accustomed ear.
> * * *
> Then thou whisperest in his ear
> Words which only he can hear;—
> Words of woe and words of cheer.
> Jubilatés thou hast sounded,
> Wild exulting songs of gladness;
> Misérerés have abounded
> Of unutterable sadness."

And the result is, that we give our consumptive patients ass's milk to drink, or send them to Appenzel to the Molkenkur, just as our forefathers sent theirs to similar pure-aired sub-alpine valleys to gain fresh strength for the battle of life from the milk of goats, rendered healing and balsamic by browsing on the aromatic herbage of those rocky regions; or, to go further back still, just as the physicians in the days of Pliny sent theirs to Stabia for country air and cow's milk. Galen sent his phthisical patients to sail up the Nile—"non propter se," he wisely adds, "sed propter longinquitatem navigandi:" is it much otherwise yet? Lisbon, Madeira, Nice, and Algiers have waxed and waned in estimation as places of resort for phthisical patients, and the rolling years have again brought Egypt prominently forward—is it now "propter se?" Is it not rather still for that other reason which that wily old practitioner knew so well how to state correctly? Could the churchyards of Naples or the pyramid of Cestius at Rome speak, what a tale they could tell of the multitudes sent to perish beneath Italian skies—exiles from their northern homes! "I have seen," says a well-known author—Professor Casper—"the gravestones of German, English, French, and Russians in Rome and Naples, in the beautiful churchyards of Marseilles, Pisa, Nice, and Malta; and bitter thoughts have crossed my mind as to the sad state of medical science and the blasted hopes of hundreds of families." Baglivi's mournful wail yet floats on every passing breeze, and is

echoed from many a sorrowing heart, though the words are somewhat transposed, and we now say, "Oh, quanto *difficilius* eosdem *curare!*" And yet in every morning paper we find liberally advertised the great discovery of modern bookmakers —"Consumption is curable,"—while almost every manual on phthisis is stuffed to repletion with so-called proofs of this fact; a fact which, when qualified by certain conditions as to circumstances, and as to the interpretation of the term consumption, none of us are inclined to deny even while regarding a large part of the evidence upon which it is founded as untrustworthy and fallacious, partly in regard to the disease treated, and partly as to the result attained. The rubric "Phthisis" has not yet been struck out of the Registrar-General's reports; nay, the very number of deaths under that denomination still maintains its original appalling numerical proportion: in fact, as one of our late writers upon this subject has said, consumption "is still essentially and almost as universally as fatal a disease as it has been in all ages" (Dr. Edward Smith). Along with that of every other disease, the treatment of phthisis is profiting by modern advances in science, is more readily recognised, and is now never submitted to the exhausting treatment formerly thought necessary to remedy the phlogistic diathesis. We do not now take six ounces of blood from our phthisical patients every second or third day for thirty or sixty times, as was the fashion with some in the end of last century; a fashion which was carried out in a modified manner far into this century, and the discarding of which has been like life from the dead to many whose lives were trembling in the scales of fate. Neither do we now bury our phthisical patients in "baños de tierra," that is, up to the chin in earth, leaving only their heads sticking like turnips above ground, as was done by Solano de Luque, putting them into a new pit each day and continuing the treatment from May to October, and many probably profit by their escape from this and other similar likely-to-be-injurious methods of treatment. But, in spite of the many cases just about to become consumption, which modern science has unquestionably arrested in their downward progress, in spite of the many cases of apparent consumption which modern science has actually cured, we still regard the curability of consumption as its weakest side, and its prevention and extinction its most hopeful ones.

The proofs of the cure of consumption, whether post-mortem or vital, are all too uncertain, and the longer we live the more inclined we are to doubt many, if not most of them.

In regard to the post-mortem proofs, to reason backwards from cretaceous and calcareous masses found within the lungs,

or from puckerings and cicatrices of their substance to imaginary attacks of phthisis, is but a simple begging of the question, because we know that these cicatrices, &c., are of such common occurrence as to have given rise to the statement that very few of the inhabitants of the civilised portions of this world have passed through life without having been phthisical at one period or other (Boudet, 'Recherches sur la Guérison,' &c.; Paris, 1843; p. 12).[1] Had the writer said that there are few who die without having previously suffered from catarrh, or even bronchitis, his statement might have passed unchallenged; but to say that few pass through life without having been phthisical, is simply to state that there is a wide difference between the ordinary term "consumption" and the pathological term "phthisis," as thus understood; and, by consequence, however strong may be the evidence afforded by these cicatrices in favour of the curability of the hypothetical pathological condition termed phthisis, they cannot be considered as of any value in proof of the curability of what ordinarily passes under the name of consumption. This argument, and it seems to us a legitimate one, disposes of all those cases whose history is simply unknown, or rests upon such questionable authority as the memory of "respectable-looking and intelligent" individuals; while, on the other hand, in estimating the probative value of those cases which have been unmistakably benefited by treatment, we must ever remember —as was, we believe, first distinctly pointed out by Dr. Latham —that both internal and external tuberculosis (scrofula) run their course in a threefold manner: rapidly, as galloping consumption or acute scrofulous abscess of the glands; chronically, as in the case of cheesy infiltration of both lungs and glands; and, lastly, intermittingly,[2] the acute symptoms in the case of both organs giving place to a more or less lengthened period of quiescence, which is, in its turn, replaced by a recurrence of the acute symptoms. To this latter class belong, alas! only too many reputed cures: *Sanantur in libris, moriuntur in lectis.* Months after we have read of their cure in medical journals, we see their deaths in the obituary of our morning paper.

M. Perroud, in the volume now before us, has done a great work in pointing out that, however little we can actually do in the way of positively curing consumption, we can yet do a great deal in the way of maintaining the diathesis, where it already exists, in

[1] *Vide* also Walshe in 'Cyclop. of Anatomy and Phys.,' vol. iv, p. 110.

[2] Andral, 'Clin. Médicale,' tom. ii, p. 276, Bruxelles, 1847. "Cette maladie nous offre ainsi l'exemple bien constaté d'une lésion constante qui ne donne lieu qu'à des symptômes intermittents."

a latent condition, and preventing its increase or spread in the community by a proper system of prophylactic measures applicable both to individuals and communities. There can be no doubt that in the case of this fell destroyer of so much of our youth, beauty, and intellect, the true mission of medicine is to provide an euthanasia for the afflicted, and to prevent by every possible means the spread of this disease amongst the community. If we aim at more than this, we lose the substance in grasping at a shadow, and we base our hopes upon an idle dream. In seeking an euthanasia, many of the afflicted will find at least temporary relief from all their distress, while, in avoiding the steady and constant pursuance of prophylactic measures in the vain hope of finding a cure when the disease has once commenced to develop itself, many more will lose their lives: let such be warned in time. Already the great panacea of modern specialists—cod-liver oil—is drifting to the tomb of all the Capulets. Its great reputation *was*—and we use the past tense advisedly—based on its employment in hospitals among a class of patients on whose ailments of every kind, good diet—and this is an article of diet—speedily tells most favorably. For the very same reason, when employed seventy years ago among the same class as a cure for rheumatism, it attained so great a reputation that no less than 60 gallons were annually dispensed at one hospital alone. It was then well known to be a product of the fisheries of Newfoundland and our own coast, and yet within fifty years it had so completely fallen into oblivion, and its very name forgotten, that it was imported, as a novelty, from the heart of Germany, in odd-looking flasks shaped like liqueur-bottles; and a writer in a medical journal, descanting on its virtues, gravely informed his readers that for the last fifteen years (1839) the Germans had been employing as a remedy the *oil of asses liver!—oleum jecoris aselli.* When this last sheet anchor shall have finally drifted through the silt of public opinion, whither shall we turn? Let us face the difficulty ere it comes upon us, and freely acknowledging the "experientia fallax" of practical medicine, try if we cannot find some pathological and physiological stepping-stones to help us over this slough of despond. First of all, in order to understand what we may hope to do and what we need not attempt, it is absolutely necessary to have some definite ideas as to what the disease is, and what are its most probable proximate causes. Following M. Perroud's somewhat empirical division of the subject, we shall first consider what *tubercle* is; second, what *tuberculization* is—viz., the process by which tubercle is formed; and, thirdly, what *tuberculosis* is—viz., the totality of the conditions, local or general, which predispose to

the formation of tubercles—to tuberculization; and, lastly, we shall consider the various means of *prophylaxis* as regards the public, the individual, and the various local deposits.

What is tubercle? has ever been a *quæstio vexata* with pathologists, and it would occupy too much space to little purpose to run over even shortly the various theories that have been propounded as to its anatomical nature; we shall, therefore, point out, as concisely as possible, what are M. Perroud's views, exhibiting, equally curtly, wherein we differ from him, and what we hold to be, if not the true state of the case, at least what seems to us to be such. M. Perroud, then, while disclaiming as effete the doctrine of heterologism, still clings to what seems to us even more untenable—the *generatio equivoca* of pathologists—the development of an amorphous fluid blastem or exudation. Now, granting such a development to be possible, we hold that, reasoning from the infinite variety of nature in other departments, we ought, *à priori*, rather to expect that every disease affecting our tissues *per viam exudationis* should have its own specific anatomical element, and we ought, therefore, to regard inflammation-globules and tubercle-corpuscles as not only possible, but probable elements in every tissue affected with the corresponding morbid process. But, on the other hand, if we find that all these so-called heterologous elements can be shown to be at least homologous if not identical with the normal elements of our tissues, then we hold that there is at least a very strong presumption that these normal elements have been excited into increased or altered growth by the exciting morbid process, and that any exudation present has acted as a pabulum, and not as a nidus—has supplied food merely, and not germs. In spite of his predilections, our author unconsciously affords very strong evidence in favour of those modern pathological doctrines based upon the dogma *omnis cellula e cellulâ*, for he labours most diligently and successfully to prove that there is no true tubercle-corpuscle, but that in its anatomical nature tubercle is nothing else than a conglomeration of the ordinary anatomical elements of the tissues, so changed, however, as not to be very readily recognised (page 25). Now, this is precisely Virchow's statement; and we are much mistaken if, in spite of M. Perroud's avowed predilection for the pathological equivalent of spontaneous generation, his own statements, descriptions, and reasonings are not some of the strongest things yet published in favour of the newer and simpler if not truer doctrine.

M. Perroud, in common with all other pathologists, acknowledges the existence of two apparent kinds of tubercle—the amorphous and the cellular. *Amorphous tubercle* he supposes

to be produced in two modes: 1st, by the direct tuberculization of a blastem; 2nd, by the granular degeneration of other tubercular elements. Neither of these suppositions is at all opposed to modern theories, but is rather in complete unison with advanced pathology; for to suppose that an exudation (or effusion, to speak more correctly) of nutritive plasma (the occurrence of which no one is disposed to deny under certain circumstances, perhaps with limitations as to quantity under all) may become decomposed into a mass of fatty granules and saline crystals, is a widely different matter from supposing that the same plasma is able to create within itself nuclei and cells possessed of various degrees of vitality.

Lebert and Bennett agree in supposing that the intercellular blastem softens and breaks down into granular matter, and in so far they agree with M. Perroud, who, however, as we have seen, goes yet further, basing his opinion upon the observation by M. Michel, of Strasbourg, and himself, of certain depôts of amorphous tubercle from which every trace of corpuscular element was wanting—observations which we hold we are far more justly entitled to regard as merely an evidence of the completeness of the granular degeneration of the pre-existing cellular elements; but this will be more evident when we point out what we regard as the true mode of formation and development of tubercle.

The *corpuscular element of tubercle* he regards as a compound of—1, an irregular fragment of coherent amorphous matter; 2, a cell more or less altered in form and structure; 3, a nucleus, also more or less altered.

If we suppose all amorphous tubercle to arise from pre-existing cells, we ought to delete the first head as being sufficiently included under the second—an altered cell incompletely broken down; but it is just possible, under certain circumstances, that small masses of this amorphous matter may re-adhere in the form of irregular corpuscules: such irregular fragments are, however, easily recognised, and are scarce worthy of mention.

In regard to the cellular or nucleolar origin of the corpuscles of tubercle, M. Perroud thinks that they may arise from every kind of cell or nucleus, but chiefly from the epithelial or connective-tissue cell, or nucleus, which undergoes an alteration or passes through a process which he terms *tuberculization.* This consists in the cells becoming, during their multiplication, very much diminished in size and irregular in form, and filled with fatty granules (fatty degeneration).

Some authors suppose that these fatty granules originate externally to the cells, and get into them by a species of

infiltration. M. Mandl is a strong maintainer of this doctrine, which our author denounces as untenable, proving, from very many examinations of the epithelial cells mixed with the sebaceous matter covering the skins of new-born children, as well as those lining sebaceous cysts, that such an infiltration never occurs. M. Perroud regards fatty degeneration as produced by a process of chemical metamorphosis—a retrogressive act by which the homogeneous protein cell contents, passing under some chemical process not yet accurately determined, break up into granular matter composed of those component ingredients of protein—amylaceous, fatty, and albuminous matter; and he seems to suppose (pp. 34, 35) that, from some pathological cause, the fatty element is most prevalent in tubercle, while in other morbid processes the amylaceous or calcareous element may take its place, and prove in its turn the most prominent chemical constituent. This idea seems worthy of consideration in regard to that obscure morbid process termed amyloïd degeneration, while it is opposed to any special *curative* action of oils or fats in true phthisis. M. Perroud regards tubercles as being formed by *malformation* and by *deformation*—that is to say, the fatty granular matter is, in the one case, so to speak, primary, and this in one of two modes: 1, the original nutritive fatty particles remain uncombined within their anatomical element; or, 2, they result from the incomplete transformation of the amylaceous portion of this original nutriment, which, instead of assisting to form albuminoid tissues by combining with azotized products, undergoes a much simpler transformation, and is only metamorphosed into fat. In the other case, the fatty degeneration is supposed to be *secondary* by the retrogressive transmutation of the albuminous cell contents into fatty matter already mentioned, which he bases on the known facts that albuminous substances under the influence of heat or alkalies are spontaneously changed into fatty acids, such as butyric or valerianic acids, &c.;[1] that crystalline lenses, morsels of fibrine or of veal, introduced into the abdomen of pigeons, speedily assume a fatty granular aspect from fatty degeneration (Burdach, Wagner, Michaelis), and also on the saponification of the fœtus retained in uterô after its death, as well as that of dead bodies under certain, as yet little known, conditions. Finally, the fatty matter retrogrades still further from its vitalised condition, and is found in a crystalline form as cholestearine; and, last of all, the calcerous elements are set free by this retrograde metamorphosis, and the tubercle is then said to be cretaceous.

Such are M. Perroud's ideas of what tubercle is. He believes

[1] Wurtz, 'Compte-rendu de l'Acad. d. Sciences de Paris,' 1844.

that it may arise from an inflammatory exudation either in a directly amorphous form, or by the previous formation of cells (inflammation-globules or pus) of a low degree of vitality, which subsequently partly break up into amorphous matter, and partly, as it were, wither and mummify; or the natural histological cell-element of the part may undergo the same transformation; these being induced or favoured by the anormal retention of effete cells (Piorry), by perverted nutrition of the neighbouring tissues or by their defective vitality;—that the retrogressive metamorphosis of the cell-element may commence *ab origine*—that it may die, so to speak, before it is born, its very elements refusing to combine in a healthy life-bestowing way, or that it —the cell-element—may subsequently break up and undergo a process of retrogressive differentiation, dying cell by cell, each yielding to the chemical influences around it and within it, and the contents of each gradually receding still farther from the state of vitality till it becomes a mere mineral—cretaceous—mass.

Based upon appearances, these, like all other pathological opinions, are only valuable in so far as they are consistent with nature. And advanced opinions are only of importance when not rashly hazarded, but carefully evolved out of former views corrected by more extended and more accurate observations—observations rendered more accurate by the employment of all the advances made by all the cognate sciences, the increased light attained by them being brought to bear upon the matter in hand. Now, in regard to tubercle, it has been distinctly ascertained, mainly by the researches of Virchow confirmed by others, that in many cases what is called tubercle is nothing but the result of the chemical differentiation of ordinary pus, which by reason of some—probably temporary and acquired—defective vitality, instead of becoming broken up into a homogeneous emulsion or pathological cream which can be absorbed, the natural and usual ending to all abscesses not evacuated, differentiates into low organic crystalline forms, and becoming deprived of its moisture, remains as a permanent cheesy or cretaceous deposit. This cheesy deposit, according to our present ideas, must always continue to act more or less as a foreign body, and when from any cause inflammatory action is lighted up around it, and it becomes softened by the infiltration into its substance of inflammatory exudation (effusion), it can never become wholly reabsorbed, but is liable and is extremely likely to give rise to ulcerative destruction of the tissue surrounding it, of a nature analogous to and not to be distinguished from that arising from true tubercle, but with this difference, that these purulent cretaceous deposits occur in healthy, or, at

least, in non-tubercular constitutions temporarily depraved in health; and even where they are of considerable extent, and where softening does occur, they are more likely to end in restoration to health than cases of true tubercular disease: hence the vast importance in regard to prognosis of attending to family and personal history, which in many cases is the only thing capable of giving even a probable answer to the question—Is this a case of true tuberculosis? with all the momentous results to the patient which flow from that answer.

On the other hand true tubercle is a disease *sui generis*, consisting of miliary masses of more or less transparent cells, containing one or many nuclei, so far resembling pus-cells as being developed like them from epithelium or connective-tissue cells, but differing from pus-cells in that they do not, as a rule, break up into a pathological cream capable of being re-absorbed, but after being massed together in the form of miliary semi-transparent tubercles, they break up by a retrograde metamorphosis, beginning in the centre, into a fatty granular detritus, becoming more fully softened, as it is termed, by inflammatory exudation (effusion) from the surrounding tissues, and giving rise as an irritative foreign body to ulcerative destruction of these. Exceptionally, tubercle may obsolesce, shrivel up in its miliary form, and cease to be an object of danger, or it may pass like pus into a true pathological cream and be wholly reabsorbed: but this is a most rare termination; more generally these miliary tubercles conglomerate together into masses of greater or less bulk, destroying by pressure all but the larger vessels passing through them, and thus paving the way for extensive destruction of the tissues when softening does occur, and for serious and often fatal hæmorrhage when that does happen in the course of the subsequent ulceration.

The importance of these views as to the origination of spurious and true tubercle, in relation to the prognosis in special cases and the actual results of the treatment of phthisis, will come to be seen when we speak of treatment. Meanwhile, we see, to recur to M. Perroud's nomenclature, that tuberculisation means something different in each of these two classes of cases; in the one we have the defective completion of a process, in the other the process is defective from its very commencement.

M. Perroud supposes that *tuberculosis*, the totality of the symptoms local or general, which predispose to the formation of tubercles, may be either general or local. In proof of its occurrence as a mere local affection, he quotes the occurrence of local phlegmons or trifling suppurations from local causes, as thorns, blows, &c., without there being necessarily any general affection of the system, any actual purulent diathesis; and he

goes on to support his views by the frequent occurrence of calcareous and cretaceous masses, of puckerings, cicatrices, or even of actual—so-called—tubercular deposits in the lungs of those whose age, antecedents, history, and general vital conditions forbid the assumption of any diathetic cause. We perfectly agree with our author in supposing the possibility of the occurrence of purely local suppurations, because these are the result of healthy inflammation, and what we term *inflammation* is merely the manifestation of the resistance of the organism to an injury received; if that injury be but trifling, then the organism as it were resents it slightly, and we term the results local; but with *tuberculosis* the case is otherwise, for, even supposing its active development to be the result of a trifling local inflammation, its fundamental meaning is vastly different, because the formation of tubercle from the defective vitality of the nutritive plasma cannot but be regarded as of general constitutional origin, inasmuch as this inefficient plasma must of necessity pervade the whole mass of the blood, and must originate in defective assimilation, which, whatever be its source, is indubitably a cause of general or constitutional disease, or proclivity to disease, and which in tuberculosis efficiently modifies the results of what may be but a trifling local inflammation.

We cannot, therefore, ever suppose tubercle to be of local origin; the apparent cause, if it can be traced, which is rarely if ever the case, may be local and trifling, but the results are but the expression of a general dyscrasia or defective vitality not confined to the fluids, but pervading the whole system, and manifesting itself by various unmistakable symptoms, all attributable to deficient activity in the nutrition of the body and consequent sluggishness of the capillary circulation. In regard to the frequent occurrence of cicatrices and calcareous masses in the lung, we have already pointed out that their very frequency proves too much.

It is long since M. Fournet showed how many of these cicatrices and calcareous masses originate altogether outside of the lungs, and are the results of ordinary local pleurisy; and we also know that not only, as already remarked, may ordinary pus become cheesy by absorption, and by further chemical differentiation be changed into calcareous masses (Cruveilhier, Andral, Graves, Reinhardt, Virchow, all except the latter regard the change as into true tubercle), but that ordinary sarcoma and even cancer may undergo a degeneration of a character so precisely similar that it is extremely difficult to say from the mere inspection of the lung what has been the origin of the cheesy masses (Virchow, 'Cellular Pathologie,' s. 430);[1] so that not only is the

[1] *Vide* also Rokitansky's 'Handbuch der Path. Anatomie,' vol. i, p. 352.

presence of cicatrices or calcareous masses, or even of cheesy matter in the lungs utterly worthless as evidence of the pre-existence of tubercle when unaccompanied by correct histories of the cases, but even with histories this evidence is always to be received with caution, unless this mode of origin and the development of the peculiar deposit can be accurately traced within the lungs themselves. Of late years it has been very much the fashion to base the curability of phthisis upon the frequent occurrence of these cicatrices, &c. We have already pointed out that the appearances are far too frequently observed to have any connection with those morbid phenomena which are comprised under the trite term "consumption," and we now see that we have very high authority for refusing to acknowledge them, in very many instances at least, as of tubercular origin at all; and not only is this the case in regard to the appearances after death, but even the phenomena observed during life are liable to be misinterpreted; so much so that one of our greatest clinical teachers, Dr. Graves, observes respecting such cases, "I do not see how, in either case, a physician was to distinguish them from tubercular abscess. Had the disease in either been more acute, the diagnosis might have been possible; but in both its progress was at first insidious, occupying many months previous to the formation of the cavities, and accompanied by gradually increasing constitutional symptoms and hectic fever;" both of the cases referred to were cases of chronic pneumonia ending in pulmonary abscess, and both recovered. Dr. Graves relates several other remarkable cases of the same kind.[1] He also adds that probably a microscopic examination of the matters expectorated might afford important information as to the nature of the disease, and in this he has but given an additional example of the soundness of his judgment and the advanced character of his views, the presence of pulmonary tissue in the sputa being now regarded as one of the most convincing objective signs of that destructive ulceration, if we may call it by that term, attendant on the formation of true tubercular cavities.

Facts such as these ought to prevent any great confidence being placed in mere pathological appearances as evidence of the pre-existence of phthisis, except under such circumstances

[1] 'Clinical Medicine,' Dublin, 1843, p. 889, &c. In an introductory letter prefixed to a recent French translation of this work, the celebrated physician M. Trousseau gives it a very high character indeed, and says he seldom passes a day without consulting it, all of which we can endorse. He thus concludes his panegyric: "Graves is, in my acceptation of the term, a perfect clinical teacher, an attentive observer, a profound philosopher, an ingenious artist, an able therapeutist. He commends to our admiration the art whose domain he enlarges, and the practice which he renders more useful and more fertile."

as permit of the origin and development of the peculiar deposit being accurately traced within the lungs in question. Such facts ought also to prevent any practitioner placing too great reliance upon the mere result of a stethoscopic examination, however careful or accurate, as a means of positive diagnosis,[1] but in every case to weigh carefully the family and individual history of the patient, and to add to this a microscopic examination of his sputa; and no case of supposed cure ought for one instant to be accepted in which all these points have not been carefully attended to, and in which their evidence does not go to support the view taken. For we know that wherever we have tubercle, crude or softened, the body has commenced to die within itself; that wherever this body of death is of any considerable volume the corresponding constitutional defective vitality must be of some considerable continuance and extent; and we also know that even where the local manifestation of disease is of but trifling extent and character, the general constitutional depravity is often so great as to give this apparent trifle all the effect of extensive organic disease, the constitutional effects being altogether disproportionate to the structural changes discovered; that, therefore, it is highly improbable that any case of extensive disease of such a character should be restored to health under any treatment, while it is equally unlikely, for the reason given, that even a case of similar disease to a less amount is of a much more hopeful character in regard to prognosis. These strong assertions are, we think, borne out by the experience of every thinking practitioner; they are also amply justified by the records of the Registrar-General, which show no diminution in the vast mortality caused by phthisis—amounting to one fifth part of the deaths from all causes. They are in unison with the statements of almost all the modern authors on the subject, from Laennec downwards. "La phthisie se termine presque toujours par la mort," says M. Louis; and Dr. Walshe says that he has, during a search carried on under "peculiarly favorable circumstances, *in vain sought for a single specimen of cicatrised tuberculous cavity;*" and he cannot avoid deliberately questioning the fact of such cicatrisation ever occurring, and they are also warranted by what we believe to be the true pathological course of tubercle.

It is well known that, as we have already pointed out, there are two species of morbid deposits termed tubercle—the grey and the yellow. The first of modern pathologists to question their identity was, we believe, Dr. Graves, of Dublin, who in

[1] "J'ai entendu, par exemple, un veritable gargouillement (cavernous râle) chez des individus dont le poumon fut trouvé exempt de tubercules." Andral, op. cit., tome ii, p. 62.

his chemical lectures (op. cit., p. 285) distinctly states, that "these," the grey tubercles, "may and frequently do occur along with the yellow purulent tubercles, and they may have purulent points deposited in their centres, or at their circumference; but it may be doubted whether there is a true conversion or growth of one into the other, or, speaking more precisely, whether greyness, transparency, and minuteness of size in tubercles, necessarily precede opacity, yellowness, and considerable bulk."

This idea received a great impulse from the teaching and writing of Rokitansky, in many respects the greatest of modern pathologists; but his statements in regard to a matter of fact the proof of which depends very much upon accurate microscopic observation, can never for one instant be put in competition with those of more experienced microscopists, such as Virchow, &c., more especially when the practical experience of, we believe, every other pathologist also goes into the opposite scale. We believe that Rokitansky, in the absence of sufficient microscopic skill accurately to trace the one form into the other, has been misled by the very wealth and accuracy of his own valuable observations, for finding the grey tubercle, and it alone, passing into a state of obsolescence, hornising or ossification, while the yellow as constantly was found to soften, break down into tubercular matter, and finally become cretaceous, he was led to believe that these two forms were the result of distinct morbid processes. We do not question the accuracy of his observations, but we do not agree with the opinions based upon them.

It has long been a source of dispute among pathologists whether tubercle is the result of inflammation or no; formerly, when an affirmative answer to this question involved "confinement, heat regulated by the thermometer, flannel, low diet, and venesection,"[1] it was a serious matter for the patient to take this view; but now-a-days we dare venture to say that we regard tubercles as the result of inflammation, without being *ipso facto* held bound to adopt any peculiar line of treatment. Inflammation in our creed means neither wine nor venesection, and though we regard it as a natural process, we yet feel bound at least to attempt to check or modify it whenever we fall in with it, just as we would combat any other natural process arising from an abnormal cause, as death from hanging, drowning, or poison, &c. We regard inflammation as the sum of the manifestations of the resistance of the organism to an injury received; the senses may recognise this sum as accompanied and apparently evinced by pain, redness, heat, and swelling; but this is by no

[1] Graves, op. cit., p. 287.

means necessarily the case; and in certain instances we may have actual inflammation without the manifestations of all or any of these phenomena, and on the other hand they may be all present without there being any actual inflammation.[1] Whenever the injury received is so severe in its nature or extent as to involve the whole system in the reaction excited, more or less of the antiphlogistic regimen may be required, but whenever the injury is in its nature so trifling that the reaction called forth is imperceptible, or only to be discovered by its results, then it were vain to attempt to check the development of these results by any means only calculated to prevent general constitutional reaction, and which have no appreciable effect upon mere local action. Therefore we may regard tubercles as the result of inflammation without being led into any kind of violent treatment, because the inflammation is of so insidious and reactionless a character as only to be recognised by its results, and therefore it is that we can so seldom ward off these results—we see our patient first because of these, when it is in the vast proportion of cases impossible to do more than to provide an euthanasia for the afflicted one, who has already begun to die. As the result of this insidious inflammation, we have the development within the tissue of the lungs, of more or less numerous greyish, semi-transparent granulations, which when discrete are of minute size, but which may become conglomerated to a very considerable bulk. One author, M. Louis, mentions masses of grey tubercle the size of a hen's egg ('Recherches,' &c., p. 4). It has been the general opinion of all pathologists from the days of Laennec that this is the first stage of tubercle, or at all events its nidus (Andral); and this opinion is based upon the facts that in the bodies of those who die from acute tuberculosis, or in the early stages of chronic tuberculosis, this kind of tubercle forms the whole or chief part of the pathological change discovered; while in more advanced cases this grey matter is found more or less apparently changed into the soft or yellow tubercle, which has gradually spread from a point or points, either central (Laennec and Louis) or on the surface (Andral), right through the whole mass, has been more recently amply confirmed by careful microscopic research (Virchow, Path. Cell., p. 428, &c., M. Perroud, p. 23). The opposition of Bayle and Chomel to this doctrine has no sufficient foundation, while that of Rokitansky must fall with his already obsolete "Crasenlehre," though the observations upon which he founded his opinion are valuable, as proving that tubercle even at this early stage may

[1] Brown-Séquard, 'Experimental Researches,' New York, 1853, p. 9, &c. Graves, op. cit., p. 874.

become extinct by obsolescence. The cellular origin of grey tubercle has been long ago pointed out by M. Robin, who thought it arose from embryonal connective tissue. This view has been confirmed by Van der Kolk; while others, as Addison, Black, and Van der Kolk himself, have regarded it as occasionally originating in epithelial cells. The probability is, as Sieveking[1] has already pointed out, that tubercular matter may be seated either within the secretory *cul de sac* or external to it, incrusting the basement membrane like a parasitic mould; so according to its situation it may derive its origin from the cells of the connective tissue, or from those of the epithelium; and this is Van der Kolk's opinion (Noederland, 'Lancet,' July, 1852), only he has added this untenable modification, that epithelial tubercle is *always* yellow, while parenchymatous tubercle is always grey. Other cells may possibly also give rise to tubercle, though from the very general distribution of epithelium and connective tissue, one or other of which is everywhere to be found throughout the body, it is most probable that they are the sole originals of tubercle as of most other forms of abnormal cell growth, and that exudation, if in the present instance (the growth of tubercle) it can be said to have any existence, can only refer to the pabulum from which the cells are formed in the ordinary course of cell generation, and not to any hypothetical blastem in which these cells are supposed spontaneously to arise. We have already pointed out that the observations of M. Perroud himself unintentionally afford very strong testimony in favour of this cellular origin of tubercle. These grey granulations may remain unchanged for an indefinite time, varying from several weeks to a much more extended period of unknown length (Louis, p. 8; Broussais, 'Hist. des Phleg.,' tom. 1, p. 26, &c.). They are capable of undergoing two modifications or kinds of metamorphosis: 1st, obsolescence—by which they dry up and wither away into opaque cartilaginous corpuscles incapable of farther change, incipient phthisis being thus capable of being cured even before its actual presence can be positively recognised. 2nd. They may undergo that retrograde chemical metamorphosis already described, termed softening; this may commence at any part of the grey tubercle, at its centre or its circumference—it is essentially a post-mortem (cellulæ) process, and very closely analogous to the saponification which the dead body undergoes under certain unknown conditions, and which is purely a chemical change, and of course not vital. Dr. Graves (op. cit., p. 284) has argued, from the existence of fluid matter in the centre of tubercular masses, that they are nothing else

[1] 'British and For. Med.-Chir. Rev.,' 1853.

than inspissated scrofulous pus. This opinion is also held by Cruveilhier, Andral, Reinhardt, and others; there is, however, no proof in its favour pus may no doubt become inspissated into a cheesy mass, incapable of being distinguished from tubercle, but cancer itself (Rokitansky) may undergo a change into matter so similar that it shall be impossible for any one on mere inspection to pronounce upon its nature (Virchow), yet these two morbid matters are so essentially distinct that the one is looked upon as the very type of excess of all life, and the other as the reverse, and the very diathesis upon which they depend are by many regarded as antagonistic the one of the other; while the mere fact that the mass is more fluid in the centre than at the circumference may be equally readily explained by the supposition that the retrograde metamorphosis is farthest advanced in the centre and has produced this moisture chemically (Perroud). In a few very rare cases this softening may be so complete and attended by so little local reaction, that the pathological cream resulting is absorbed without any destruction of tissue, and a perfect cure is thus obtained even at this stage. More generally, however, the local reaction is more considerable, and in such cases, from the intimate manner in which the connective and epithelial tissues surround and, as it were, inclose the actual trabecular framework (the yellow elastic tissue) of the lungs, we can easily understand that when any tubercular mass even of but moderate size, comes to be thus softened and discharged in the process of forming a cavity that along with the true tubercular sputa, consisting of granular matter, mummified or withered cells, with perhaps some few cells swollen by endosmosis, and so assuming more of the character of pus, we are sure to have more or less of the pulmonary yellow elastic tissue discharged, but this, though it may aid the diagnosis, and is certain to cause a greater or less destruction of the pulmonary tissue according to the amount of the tubercular matter in process of discharge and consequent size of the cavity about to be left, is only of consequence in so far as it reveals the true nature of the disease, which owes its fatality not so much to the amount of tissue destroyed as to the defective constitutional vitality upon which its existence depends. This defective vitality M. Perroud thinks reason and observation alike prevent us from localising in the blood or in any tissue, but in a morbid condition of the corporeal vitality as expressed in nutrition to which the term diathesis[1] truly belongs, and however obscure this statement may seem, it would be difficult to express the same idea more clearly; he regards it as entirely identical with scrofula, and the weight of opinion would seem

[1] Baumé's 'Precis Theorique et Pratique des Diathèses,' 1853.

to be in favour of this idea, though it has, no doubt, many famous opponents, as Lebert, &c. This diathesis may be hereditary or acquired, and from this we have two distinct classes of prophylactic measures. M. Perroud, in opposition to Clark ('On the Influence of Climate, &c.,' p. 243, London, 1829), considers that the child acquires from its parents not merely the predisposition to the diathesis but the diathesis itself, and in this we think most observers will agree. The hygienic means to be employed to prevent or neutralise the hereditary transmission of so fatal a disease are in the first place, to endeavour to avoid all the conditions upon which this transmission depends, and in the second place to accumulate all those circumstances capable of opposing it. It is well known that a new species of sheep has actually been created, so to speak, in Massachusetts, by judicious breeding; horse breeders and pigeon fanciers know well the influence of hereditary descent in influencing form and qualities. We ourselves well remember an instance in which a gentleman, well known in sporting circles, persisted in breeding year after year from a favorite horse who happened to be broken-winded (asthmatic with emphysematous lungs), and year after year, though his progeny were in appearance such likely animals as to induce the offer of large sums of money for them as yearlings, £2000 in one instance, yet not one ever reached the starting-post, the lungs of all gave way before they were two-year olds; and can we wonder that so fatal a disease as tuberculosis can be propagated by hereditary transmission, or may possibly be averted and got rid of by careful nuptial selection? Ulloa, Tavis, and others state that three or four generations, where proper attention is paid, are sufficient to make a negro white or a white man black; and M. Serres asserts that a proper system of intermarriage will cause a degenerate race to return to the normal type. What a noble object to set before one—to marry for no ignoble selfish end—but that after a course of years your family may be restored to their pristine vigour! The idea is somewhat French, nevertheless it is certainly practical and unquestionably practicable. To marry for rank or wealth is a matter of daily occurrence, but how much less noble than to marry for health which rank cannot bestow nor wealth purchase. It is regarded an object of legitimate ambition to marry for money to replace an ancient family in their proper position in the world: ought it to be less an object of ambition to seek by an infusion of healthy blood to restore to a decaying family the health they have lost? Unquestionably the latter is the nobler and least selfish object. To gain this end, M. Perroud proposes that the union of tuberculous individuals or of those pertaining to tainted houses should be

avoided, and that the union of those individuals not tubercular themselves, but in such a state of health as might favour the development of tubercle in the children, should be shunned.

The whole system of cattle breeding in Great Britain is based upon the principle that the union of two individuals presenting certain peculiarities of form or constitution is much more likely at least—if it be not certain—to ensure the reproduction of these peculiarities in their progeny. Geoffroy Saint-Hilaire has shown that the peculiarities of certain races of men have been impressed upon them by the conditions in which they live, while these accidental characters have been perpetuated by hereditary descent. Brown-Séquard found that in guinea-pigs in whom epilepsy had been artificially produced by operation, this acquired epilepsy was transmitted to their offspring; and, indeed, the whole system of modern life-assurance is, to a large extent, based upon the acknowledged hereditary transmission of constitutional peculiarities, the importance and evil influences of which increase in force the farther back we can trace them in the ascending line. M. Perroud's proposition may, therefore, be readily assented to. But, not stopping short with these negative propositions, M. Perroud has sought to discover whether there are any morbid diatheses which could be employed to cross the tubercular one with the view of annihilating it, but after examining the claims of the cancerous, the gouty, and the rheumatismal diatheses, as well as the claims set up by some pathologists in regard to the incompatibility of certain pyrexiæ with general tuberculosis, and of the preservative influence exerted by them—typhoïd fever, typhus, dysentery, smallpox, &c. (Sniffet)—upon constitutions modified by these affections, he comes to the conclusion that the only safeguard is to select a spouse of an unimpeachably healthy stock for the tainted one, the temperaments being as far as possible antagonistic, especially preferring to cross the lymphatic with the bilious or bilious sanguine temperament, endeavouring thus to antagonise the evil influence of the one spouse by the favorable and healthy influences of the other. We must seek to give effect to these favorable influences by securing every possible advantage in way of climate, situation of the dwelling, and food during the pregnancy, infancy, and childhood, and as far as possible rendering the acquired beginnings of a restoration to health permanent by a series of well-selected marriages, in the hope of causing the hereditary taint to disappear in the course of three or four generations.

But as consumption is not hereditary but acquired in a very large—according to many authors, by far the largest—proportion of cases, a different means of prophylaxis must be brought

to bear upon them, varying according to the originating circumstances.

M. Perroud regards tuberculosis as not capable of being conveyed by inoculation ;[1] he also holds with Laennec, Jos. Frank, Sir James Clark and others, that long-continued exposure to the inhalation of the deleterious morbid effluvia from a tuberculotic patient, though it may not play the same active part in inducing tuberculosis as the miasmatic exhalations in inducing marsh fever, nevertheless exercises an indubitably predisposing influence upon those individuals submitted to its action, and he also holds with Andral (*Auscultation mediate,* tom. 2, p. 179) that it is possible that, under certain circumstances of climate, constitution, &c., this tuberculous infection may become a true contagion and give direct origin to the disease. Now, however startling these views may seem at first sight, there is unquestionably this substratum of truth in them, that under the circumstances of continued exposure to the influences surrounding a tuberculotic patient, and the depressing effects of attending on a tedious illness, tuberculosis is specially apt to be developed in those hereditarily predisposed, and may even be apparently thus acquired by those not so predisposed: so that in our utter ignorance of what contagion really is, it may be better, instead of attempting to confute these views, to permit them to stand provisionally, putting a mark of interrogation after them. The idea that the tubercular diathesis may be transmitted from one spouse to the other by the mere act of cohabitation, belongs to the same doubtful category as the theory of contagion. It is otherwise, however, with the opinion that this diathesis may be transmitted from the male to the female by the medium of a fœtus, and this even in such a way that children begotten by a second and healthy husband, may yet inherit the diathesis thus acquired from a former tubercular husband. This is the effect of that peculiar but well-known law by which the maternal organism becomes inoculated by the peculiarities of the paternal organism by the influence of the fœtus in utero—a law which has been ably illustrated in a series of papers by Dr. Alexander Harvey of Aberdeen, in the 'Edinburgh Medical Journal' for 1849-50 and 1854. M. Perroud relates two instances in which a healthy mother was inoculated with fatal tuberculosis by means of a fœtus begotten by a tubercular husband; and amid a host of similar cases Dr. Harvey relates several instances where children begotten by an apparently healthy father out of an apparently and certainly originally healthy mother, inherited

[1] Quite recently MM. Villemin and Lebert (of Breslau) have promulgated certain very strange and very strong statements in regard to the inoculability of phthisis, but as yet these require further confirmation.

the tubercular taint conveyed to the mother from a former diseased husband through the medium of unhealthy children begotten by him. Dr. Harvey very properly adds that possibly only appropriate external conditions were required to call into full activity in the person of the mother herself that taint which she had acquired from her first husband through the medium of his offspring while in her uterus—an opinion to a certain extent confirmed by M. Perroud's cases. It is also unquestionable that a wet-nurse exercises an important influence on the vitality and temperament of her fosterchild; this also is a matter too much neglected and deserving of more inquiry.

M. Perroud then enters upon a long and important discussion as to the means of prophylaxis to be derived from the physical and psychical agents surrounding and capable of influencing the patient, and in doing so, while fully recognising the atrophic and atonic nature of tubercle as indications that every source of debility and exhaustion to the system ought to be avoided, and every means taken to strengthen and restore its tone, he yet, as we shall see, holds peculiar views as to what are debilitants and what are restorants. He first directs attention to the importance of a carefully selected and nutritious diet, showing that roast meats, and especially meats rich in osmazone, ought to be preferred; that wine when taken should be tonic—"amers"—and not deficient in alcohol; that brown bread, made of whole flour, should be preferred to white, as being more highly azotised; and that the use of tea and coffee, where they agree, should be encouraged as favorable to digestion, and productive of a beneficial excitement both in regard to the vegetative and intellectual functions; recommending, however, a careful choice of viands and drinks, according to the state of the patient and the season, so as to avoid the production of any undue erethism. He also strongly recommends the use of cod-liver oil as an analeptic, its use to be regulated by the laws laid down for the employment of the tonic regimen; adding that it is by no means to be regarded as a specific for the prevention or treatment of the tubercular diathesis, as quinine is specific for marsh fever, and guarding against the idea that the mal-assimilation of fatty matters is the great source of danger in tuberculosis —an assertion which M. Perroud says is a little rash, though he does not make any allusion to the fact that albuminous matters are more easily digested when administered along with fatty matters; nor to the fact that a purely animal diet which tubercular patients so often delight in necessitates, for the production of animal heat, an amount of exercise they are wholly unable to take, the rapid wasting of

the fatty constituents of the tissues, or the coincident administration of an extra quantity of fatty matter in the food, both of these facts tending to explain the benefit derived in certain cases from the use of cod-liver oil, which is certainly an easily assimilable form of fat.

In regard to the phosphates and hypophosphates so much vaunted of late years in the treatment of tubercular complaints, M. Perroud points out that in animals low in the zoological scale phosphorus seems to play a part in the acts of assimilation and retrograde metamorphosis, somewhat similar to that taken by oxygen in those of higher development, while he also refers to the fact pointed out by Mège Mouries and M. Beneke, &c., that there is an abnormal absence of phosphorus from the tissues in all maladies associated with emaciation, particularly in the scrofulous, tubercular, or rheumatismal cachexias. But M. Perroud also points out that if phosphorus be such a necessary ingredient in our tissues, it ought to be regarded as a true aliment necessary for their perfect formation, and not merely as an evanescent modifier of those tissues—a medicament; and that therefore more permanent and real advantage may be obtained by administering it in an organised form—that is, in such a state of combination as shall facilitate its assimilation. Therefore, all organic substances containing phosphorus ought to be regarded as most valuable nutrients; eggs, animal flesh, gluten, and the cruciferæ, are all, therefore, preferable in this respect to merely farinaceous and fatty articles of diet, and in this respect experience fully bears out theory. But M. Perroud goes further, and deprecating with M. Baud the injurious influence of cooking upon the chemical constitution of these articles of diet, he recommends meat to be given raw, chopped small, and mixed with various condiments; or the administration of several wineglassfuls daily of blood recently drawn from a young and vigorous animal, or the use of the highly phosphuretted fatty matter of the brain, recommending for this purpose a sweetmeat prepared by M. Garot from the pulverized medulla oblongata of the herbivora mixed with sugar. Such remedies have been largely administered on the continent, but, with the exception of raw meat, which has been frequently employed as a favorite and most useful remedy in certain cases of diarrhœa, they have not been much, if at all, used in this country, where, moreover, the prevailing mode of administering animal food in a lightly cooked form renders them less necessary.

M. Perroud has also pointed out that chlorine, in the form at least of chloride of sodium, plays a most important part in the animal economy, and advises that it should be administered

alimentally—to coin a word for the occasion—in the form of the milk of cows, asses, or goats, amply supplied with saline food. This idea seems, however, to be far-fetched, and not worth the trouble involved in its carrying out. Chlorine itself, we know, exercises anything but a beneficial effect upon the health, and even supposing that we do not hold with the Chinese (Tchanseng) that the free use of salt is to be avoided as the root of all evils, still all its benefits may assuredly be obtained with less trouble and in a more agreeable manner than by drinking saline milk.

Our author also very properly points out that the nutrition of the body may be defective not only from insufficience in the quantity or quality of the food supplied, but also from an inability of the digestive functions to make a proper use of the nourishment ingested, observing that Dr. Hedouin asserts that the anæmic condition thus arising is a far more frequent cause of tuberculosis than is commonly suspected. But M. Perroud does not agree with Hughes Bennett and others in regarding this mal-assimilation as more dangerous in this respect when it affects the fatty matter of the food than when it affects its albuminous constituents, because it has not yet been proved that fatty aliments contribute more to the formation of the blood-corpuscules than fibrinous aliments; the fact being, as already pointed out, that a due admixture of both kinds of food is necessary to the proper nutrition of the body, and that the supply of an extra quantity of fatty matter, however much it may, under certain circumstances, contribute to a temporary and delusive appearance of *embonpoint*, is of no real advantage in the treatment of phthisis. Unquestionably, however, there are certain cases in which cod-liver oil, from being more easily and readily assimilated along with the albuminous constituents of the food than other fatty matters, acts as a veritable tonic, and by increasing the number of the blood-globules improves the whole tone of the system. M. Perroud advises, therefore, with great propriety, that in the averting of tuberculosis the utmost attention should be paid to the digestive functions; that care should be taken to avoid all compression of the stomach by the clothing or the habitual position of the body; that the meals should not only be well chosen as to the nature of the food supplied, but that they should also be supplied at regular intervals, and that these purely hygienic means should be aided by the employment of other therapeutical measures capable of preventing and of remedying the results of perverted nutrition. Among these, he recommends iron as specially applicable in those numerous cases in which anæmia is not so much the consequence as the cause of the dyspepsia; chloride of sodium as

a medicament acting analogously to the iodides and bromides in modifying digestion; and also the salts of iodine, which are specially efficacious in stimulating the functions of assimilation and of retrograde metamorphosis, and which have been found particularly useful in dyspepsia attended with febrile exacerbations more or less well marked. M. Boinet recommends these to be given, in certain circumstances, in their natural condition—that is, as they exist in such plants as the cruciferæ, the fuci, &c., which he proposes to dress for table as vegetables. Bitter tonics are of course recommended, but especially arsenic, which exerts such a powerful and favorable influence upon the digestive functions of feeble persons, and which he recommends to be given in such minute doses as a milligramme (·0154 gr.) of arsenious acid, or one or two drops of Fowler's solution, daily—doses which we venture to say will not be found too minute to be useful, but will on the contrary be sometimes, though rarely, found to be rather powerful to be continued long.

Turning next to the respiratory function, which, on the one hand, completes the function of digestion thoroughly, completing or rendering fit for assimilation the materials already elaborated in the primæ viæ; while on the other, it is equally useful in perfecting the transformation, and rendering fit for excretion (as urea, carbonic acid, and water) those substances resulting from that retrograde metamorphosis which has already commenced in the tissues, M. Perroud points out that not only, as Milne-Edwards has said, does the vital activity regulate that of respiration, but that the converse is at least equally true, and the activity of the respiratory process regulates that of vitality. Hence the importance of attending to the nature of the air respired, and of the condition of the respiratory organs in any attempts to ward off tuberculosis. In overcrowded dwellings there are three circumstances all tending to produce that languishing state of vitality so dangerous as predisposing to this fatal diathesis; we have, first, a diminished supply of oxygen; second, a large amount of carbonic acid gas, which, as Claude Bernard pointed out, presents decided obstacles to the mutual interchange of the gases contained in the blood; and, thirdly, a small proportion of carbonic oxide gas, which, as the same author has shown, possesses the peculiar property of depriving the blood-globules of their power of assimilating the oxygen of the air, and the evil influence of such an atmosphere in producing general tuberculosis is unhappily but too well proved by clinical statistics.

If possible, then, the household apartments ought always to be large and spacious, and where this is not possible, free venti-

lation ought to be promoted in every possible way, and open fireplaces with chimneys ought especially to be insisted upon, these means being supplemented when necessary by placing trays with quicklime in the rooms to absorb a part of the free carbonic acid gas as well as of the watery vapour. The inhalation of oxygen in a larger proportion than usual has not been found useful, but the contrary. Beddoes mentions one case in which there was no phthisical tendency, yet the inhalation of oxygen for a half or a whole hour daily for seven weeks produced at first a heightened complexion, decrease of fat, increased muscular development with desire to exercise it, increased power of withstanding cold, and subsequently epistaxis, burning of the hands and feet, the pulse rising from 80 to 100, especially at night, when there was a febrile exacerbation; by keeping quiet, using a diet in which sweet butter, cream, and oil predominated, this patient was relieved from these unpleasant symptoms. Another patient (phthisical) breathed twice a day for five minutes a mixture of one pint of oxygen and three of atmospheric air; at first he felt relieved, but in two days' time the cough and fever were both much increased; this he attributed to a cold caught, but a repetition of the experiment after an interval was attended by the same results. The employment of the binoxide of hydrogen, vaunted at one time as one of the most powerful of remedies, has not been attended with any better results. On the other hand, the use of ordinary atmospheric air at an increased pressure (compressed air bath) first recommended by MM. Tabarié and Pravay, of Lyon, has been found not only to check effusion from the pulmonary mucous surfaces, as in chronic bronchitis, but also to increase in efficacy the chemical action of respiration, and thus, by the close connection subsisting between the two, to improve the digestive functions. Every means ought also to be employed to develop the thorax and pulmonary organs by means of a properly devised system of gymnastics, and especially of gymnastic outdoor games, such as bowls, &c., as well as by prohibiting all injurious postures, too close confinement at the desk, too assiduous application to the piano or the embroidery frame, and paying strict attention to the clothing, especially in females, in order to prevent any injurious compression of the thoracic cage or hurtful interference with the respiratory movements that might be caused by it. Exercises in recitation are also extremely useful in promoting the dilatation of the chest; it is well known that Cuvier was threatened with phthisis in his youth, and attributed his preservation to his professorship, that is, as he understood it, to his being obliged to lecture; but there are other and better ways of understanding it in relation to phthisis,

especially as we every day find men with the fullest development of chest, and with large, well-developed lungs, falling victims to this disease; and we think it is Groshaus who directs special attention to what he believes to be a fact, viz., that those with enlarged pulmonary vesicles—which deep inspiration tends to produce—are specially liable to phthisis.

To these means of improving the pulmonary activity ought to be conjoined every possible means of stimulating the cutaneous respiratory functions which are no less important;—to this end, friction, and baths of various kinds, of pure and mineral water: but M. Perroud has neglected to point out the immense importance of sudden, short, and sharp applications of cold in developing the power of producing animal heat, and of thus bracing the system and enabling it to withstand those sudden vicissitudes of temperature which he acknowledges to be among the most frequent sources of danger in the production or development of tubercle, and more especially of those dangers which, like the sword of Damocles, are constantly impending over the tuberculotic. To attain this object, no better means can be adopted than to heat the body first thoroughly in a warm bath, and then, while still standing in the hot water, to bring a sudden shower of cold water of short duration over the shoulders and back. The very first application of this produces a sensation of being braced; and its continuous employment aids powerfully in developing the power of producing animal heat, so apt to be, in a great measure, lost in those whose delicacy of constitution necessitates the utmost care in exposing them to the ordinary vicissitudes of the weather: and this is a most important point; for the more care is required in regard to such exposure, the more do dangers multiply for the objects of that care. Great attention is very properly directed to the important stimulant action of all climatic agents, such as the pressure, moisture, and electricity of the air, and especially to the action of solar light, the absence of which is so powerful a cause of debility of constitution and of the development of the lymphatic constitution, while its action is so important in all that concerns the chemistry of life. Under the head of temperature, M. Perroud condemns the too common but vicious habit of combating the cutaneous intolerance, or susceptibility, by multiplying the articles of clothing, instead of seeking to overcome it in a normal and and physiological manner by appropriate gymnastic exercises, by richer and more abundant food, or by what is still better, the sudden alternations of cold and hot bathing to which we have already referred, or the appropriate use of stimulants, especially given early in the day, as claret, light ale, or even a modicum of stronger alcoholic fluids given diluted—rum and

milk is an old and well-known form—for drink at breakfast, by which the development of animal heat receives a fillip early in the day, which often saves the patient from many an otherwise inevitable chill. Our author then passes in review various other general means of stimulating the nutritive functions, in regard to intellectual employments, the passions, travelling, residence at the coast or in hilly regions, the influence of the natal air and of mineral springs; recommending thermal springs as better than cold ones, and sulphurous waters as superior to saline, and the latter to alkaline, but pointing out that, in selecting a spring, not only the fitness of its constituents must be taken into account, but also all its other adjuncts, such as its altitude, climate, exposure, and means of amusement to be obtained. He then points out how these various hygienic measures ought to be employed methodically in accordance with the age of the patient, pointing out, in conclusion, that an active open-air life is one of the best preservatives against phthisis; but into this part of the subject we need not follow him.

In considering the influence of local, diathetic, or pyrexial diseases upon the development of tubercle, M. Perroud points out that these act in proportion to the interference with the general nutrition of the system, and are therefore but very indirectly the causes of tuberculosis. Measles, however, he points out as being one of the most dangerous of the eruptive fevers; not because it is capable of producing the tuberculotic diathesis, but because it has a most prejudicial effect upon the development and multiplication of its pathological product—an effect from which, he asserts, scarlatina and hooping-cough are entirely free. To typhoïd or enteric fever, he asserts, with Andral, Louis, Rilliet, and Barthez, and the Vienna school, an effect antagonistic to the tuberculotic diathesis, alleging that the apparent truth of this dogma is to be found in the fact that enteric fever attacks chiefly those strong constitutions which are precisely those least favorable for the development of general tuberculosis. This dogma will, we apprehend, be found to have but few supporters in modern days, as enteric fever is now recognised as a not infrequent forerunner of phthisis, and that probably for the very reason already pointed out—viz., that it does interfere materially with the general nutrition of the system. The idea that variola is less likely to be followed by tuberculosis than vaccinia, which MM. Rilliet and Barthez supposed they had proved statistically, our author very properly concludes to be not proven, and chiefly for this reason—that these two pyrexiæ being but modifications of the same morbid condition, it is impossible to conceive that their relations to tuberculosis can be different in any respect.

Very properly considering that the prevention of consumption, which year by year cuts off so many thousands of the people, ought to be even more the desire of a wise and paternal government than the mere prevention of occasional epidemics, which at comparatively rare intervals, and to a comparatively trifling extent, devastate the nations, M. Perroud has devoted one entire chapter to the subject of the public prophylaxis of tuberculosis, by which this scourge of humanity may be exterminated, not merely from an individual or a family, but from the inhabitants of a city, a province, or a country. For this purpose he proposes, in the first place, to give to the people at large such an amount of medical instruction as shall enable them to unite intelligently in endeavouring to attain one common end—rooting out, in this manner, those ancient prejudices, and that love of old wives' (empirical) remedies, which charlatanism has known so well how to make use of for its own profit, to the injury of the public and of scientific medicine. He thinks that this object could be easily attained by giving a few hours' lessons per week in the larger schools and colleges upon the elements of hygiène, or even by creating, in larger towns at least, chairs of popular hygiène, in which the professor, descending to a level with his audience, should instruct them in the resources of this science, and the best modes of making use of them. We cordially endorse our author's opinion as to the salutary influence of such a measure in regard to the welfare of the public, not only in regard to tuberculosis, but also in regard to disease generally; and we are sure it would have a no less beneficial effect upon the well-being of the profession, elevating medical men in the eyes of the public to their rightful position as the counsellors and advisers of mankind, and depriving charlatanism of the advantage it knows so well how to make use of in pointing these out as a mere class of privileged nostrum-mongers. The other measures by which M. Perroud hopes to attain his great object are, perhaps, a little too French, or too advanced even for our nineteenth century; though, indeed, we agree with him in thinking that if the legislature considers it part of its duty to secure to the best of its ability the monetary heritage of the offspring, it ought to consider it no less empowered to secure its heritage of health, in so far as that lies within its power. Nevertheless, as yet the public are not sufficiently enlightened to see it to be for their good, or for that of the State, that Government should interfere with individual freedom in regard to marriage, or to dress; and whenever they are so enlightened as to perceive the benefit of such measures, the interference will be no longer required. Still, much may be done in the way of promoting the general welfare of the people

—poverty being a powerful predisponent cause of tuberculosis, as well as in enforcing all general sanitary regulations capable of improving the public health, and in encouraging gymnastic and open-air exercises.

Having thus in the two preceding chapters considered the prophylaxis of the diathesis, the third and last chapter of this interesting work is devoted to a consideration of the prophylaxis of diathetic tubercular deposits. In this chapter the author first gives a tolerably full account of the diagnostic signs of the tubercular diathesis, and then proceeds to inquire whether the tubercular diathesis is of itself sufficient to create tubercular deposits, or whether some additional local excitement be not also necessary. In the one case, he very properly adds, the only true prophylaxis would be the curative treatment of the diathesis itself; in the other, much good may also be done by protecting the patient from these occasional secondary causes. He arrives at the conclusion that the diathesis alone is not sufficient of itself to produce tubercle, and hence that much good may be done by dissipating inflammatory congestions when they have occurred, or by preventing their occurrence either by giving to the organism a mode of vitality inconsistent with their development, or by carefully guarding against all their exciting causes. M. Perroud includes all congestions of the lungs as exciting causes of tubercular deposits; but the strong statements of Louis ('Recherches,' &c., p. 608), and especially of Rokitansky ('Handbuch,' vol. i, p. 427), as to the antagonism between cyanosis and tubercle, are sufficient, we think, to induce us to modify his statement so far as to restrict it to inflammatory congestions, which are not only sources of danger as productive of tubercle primarily, but are still more dangerous as causes of secondary affections where tubercle already exists. We also do not share his predilection for bloodletting as a remedial agent in such cases; but in this place and at this time it is sufficient to point out the danger of inflammatory congestion, leaving to each one to select for each individual case the most appropriate remedy. Neither do we agree with him in thinking that an anæmic condition is unfavorable to the production of inflammatory congestions, and is therefore to be produced artifically by the administration of preparations of lead, of digitalis, &c.; and when it naturally exists in such cases, is not to be attempted to be cured by appropriate diet and ferruginous tonics. The speculations in support of this theory are ingenious, but are not, we conceive, either warranted by existing facts, or by the present state of pathological theory. Assuredly, anæmia is not antagonistic to inflammation; and when this does occur in such

habits, it is more than usually hazardous, and is all the more likely to be so when the danger increases with the low and insidious character of the affection, as is sure to be the case in tuberculotic patients. M. Perroud then points out the beneficial effects which have occasionally seemed to arise from the occurrence of pregnancy and of uterine diseases, neither of which can of course be employed as remedial agents, and the influence of both of which have probably been much overrated. He also approves of tuberculotic patients nursing their children, the mammary congestion and secretion acting, as he supposes, in some sort as a revulsive, protecting the lungs. On his own shewing, however, the practice is dangerous to the infant, while it is also by no means free from danger to the mother, all the good effects supposed to be derived from it being capable of being otherwise produced at less expense to her constitution. M. Perroud very properly points out that the period of commencing menstruation is fraught with great danger to girls possessed of the tubercular diathesis, in whom this discharge should be carefully promoted, and whose health at this period ought to be most diligently watched over. In considering the subject of surgical operations in such subjects, he arrives at the following general rules:—1st, that all urgent operations should be performed without hesitation; 2nd, that slighter and less urgent operations, such as the opening of abscesses, &c., may also be undertaken without fear; while, 3rd, that all graver though not urgent operations should be performed or not, according to the peculiar circumstances of each case; and that when such operations are undertaken, care should be taken to prevent the disastrous consequences of the sudden suppression of any copious drain to which the constitution has become habituated by the employment of revulsions or derivations.

In considering how best to avoid the direct causes of inflammatory congestion M. Perroud commences by pointing out that, according to M. Devay ('Hygiène des Familles'), there is in every individual some one organ deficient, relatively to the others, in that vital energy which it ought to possess in a state of well-balanced health. From a series of statistics as to the relative frequency of tuberculosis in the organs, quoted from MM. Rilliet and Barthez, he deduces that the parenchymatous glands, and especially the lungs, are those organs most subject to tubercle, and that, consequently, these should be specially preserved from all sources of irritation; and he arranges the different means of prophylaxis under the three following heads:—1st, the removal of every source of mechanical irritation; 2nd, the placing of the organs in a species of salutary repose,

and, 3rd, the avoidance of all perniciously exciting climatic influences. Under the first head, he shows the importance of avoiding all traumatic influences, whether these are produced by actual wounds or bruises, by the injurious excitement of the intestinal or mesenteric glands, by aliments dangerous in their quality or quantity, by injudicious kinds of clothing, or by the inhalation of various powders, such as are in colliers, stone-hewers, millers, &c., not only a frequent source of pseudo-phthisis, but also of true tubercular consumption. Under the second head—though various partial means might be devised for each set of organs—he confines himself chiefly to the consideration of the means for procuring the artificial repose of the lungs, which is most readily effected, and which has a powerful influence not only locally, but generally. In regard to the artificial repose of the lungs, M. Perroud remarks that it may be obtained in two modes; by diminishing the amount of their excitant—the oxygen of the air, or by altering its quality. The first of these ends he recommends to be attained by increasing the temperature of the air, this lessening the amount of oxygen inhaled in a given bulk of air; the second, he states, may be attained by mixing the air with the vapour of tar, which, according to the experiments of Dr. Sales-Giron, alters the quality of the oxygen by diminishing its chemical action; thus, phosphorus, shut up in a bottle of air, with a few drops of tar on its bottom, was found to lose its phosphorescence; although the air preserved its normal amount of oxygen, its phlogistic quality was found to be modified. Since the inhalation of the vapour of tar was first recommended by Sir Alexander Crichton in 1823, it has been often employed with varying effect; but its *modus operandi* has not previously been so distinctly stated. There can be no doubt that the treatment of phthisis by inhalation is yet in its infancy, and that while the past records of medicine prove that much good may thus be attained, there is a lamentable want of precision in these records as to the nature of the cases to be benefited by the special remedies employed. Tar, sulphur, chlorine, and chloroform have all been vaunted as almost specific in this complaint, and each has at different times produced striking remedial effects; yet no attempt has hitherto been made to systematise these effects, to show their causes, and thus enable us to prescribe them in succeeding cases—not empirically and upon chance, but with a reasonable probability of their doing good. The theory of consumption in the work we are at present reviewing offers, we conceive, much that may be profitably made use of under this head. No doubt, many of the views and statements it contains are to be rejected, or only accepted cautiously and after due inquiry; yet in the

wide and extended views it takes of the nature, origin, and prophylaxis of consumption, there is much to be found which may be profitably made use of in its treatment, and particularly in this respect. Thus, if we clearly understand phthisis to be what all pathological evidence tends to prove it is—the falling to pieces by retrograde metamorphosis of the product of an inflammation of low degree, because occurring in an organism defective in vitality, with necrosis of the enclosed elements, while pseudo-phthisis is something very similar in its results, though the mischief is more limited in its extent, and being the product of temporary causes, is not so likely to extend, and is far more hopeful as to its prognosis,—then we can not only see how important a matter family history may be as to prognosis, but also how important an influence it ought to exercise upon the treatment of the case, and especially as to its treatment by pulmonary dietetics.

Where we have to deal with a patient whose family history is unimpeachable and where signs of softening of a limited extent of the lung are present, our great object ought to be to limit the existing inflammation by external counter-irritation, and by sedatives, not the least important of which is a reduction of the amount of oxygen inhaled or an alteration of its quality. These objects are most readily attained by sending the patient abroad to a suitable climate, or, where circumstances forbid that, keeping his ordinary apartment, which ought to be spacious, at a uniform medium temperature, and qualifying the properties of the oxygen in it by diffusing through it a moderate amount of tar-vapour, a tar-respirator being supplied when he has occasion to go abroad. These means, coupled with the ordinary tonic regimen modified for each case, will in general be sufficient to restore the patient in time to health. On the other hand, if signs of softening be present where the family history is bad, residence abroad may be permitted, but, as a rule, ought never to be recommended. In these cases the benefit to be obtained by change of climate is rarely sufficient to counterbalance the deficiency in home comforts, and the prognosis being most serious, we ought to be all the more chary in sending our patients away. In such patients, too, it is of the utmost importance that the air breathed should be as unirritating as possible, and should contain the smallest amount of oxygen consistent with the maintenance of life, so as to lessen that process of erema-causis or wasting which is more dangerous to life even than the destruction of lung-tissue. Various means have been at diverse times proposed for this end. The late Professor Home, of Edinburgh, introduced the use of mephitic air, as he termed it, that is, carbonic acid gas generated in a bottle by the addition of

sulphuric acid to chips of marble or carbonate of lime; and this has been of late years revived. But as carbonic acid gas cannot be inhaled when constituting more than a trifling percentage of the air breathed, this treatment can have no effect, and those thus treated merely *smoke* the gas, and perhaps unintentionally their doctors also. On the other hand, air diluted with pure hydrogen in the proportion of 1 part to 3 or 4 can be readily respired, and has been found by Dr. Beddoes and others, as well as by ourselves, to be productive of much benefit in many cases. The hydrogen acts not only negatively by reducing the amount of oxygen in the air, but positively as a direct sedative both local and general: it lessens the cough, diminishes the febrile heat, and soothes the patient to sleep as effectually as opium, and, if properly carried out, we think that much benefit might be derived from its use even in hopeless cases, especially as its use seems wholly unattended with any known drawback. On the other hand, the more stimulating terebinthinate vapours, fumigations with creasote, chlorine, or sulphurous acid, which have all at times proved beneficial, seem to be more suited to cases of simple chronic abscess, or those cases which, from the expectoration of a local mass of real or pseudo-tubercle, have become similar to cases of chronic abscess, and from their stimulating qualities are, as a rule, wholly unsuited for cases in which tubercle still exists in the lung, producing in them often much distress and exacerbation of the symptoms. Sulphuric ether and chloroform, either pure or combined with various narcotics, often give much relief to the cough, but always either with exacerbation of the other symptoms, or at all events without any alleviation of them. The residence in byres, long ago recommended by Bergius of Sweden, and which was at one time a fashionable mode of treating consumption in this country, seems to have derived what little efficacy it had solely from the high temperature and the reduction of the amount of oxygen in the air inhaled; for we think the fancied balsamic vapours of the cows' breaths could have had little real action in the matter. Indeed, it is possible that, as Mason Good remarks, the extra quantity of hydrogen in the air of such places, as well as in that of slaughter-houses, is the real source of the fancied virtues of animalised air. We think, then, that in the treatment of phthisis and of pseudo-phthisis, too much attention cannot be directed to the influence of a reduction in the quantity and quality of the oxygen, as forming a system of pulmonary dietetics not merely supplementary to, but of quite as much importance in the treatment of these diseases as the more usual dietetics of the stomach; never forgetting that when we have to deal with the mere diathesis, the amount of oxygen ought to be

as much as the system can bear, so as to promote the healthy physiological action of all its functions; while, when there is actual disease present, the amount ought to be the least that the system can do with, while its quality ought also to be modified by sedatives capable of acting both locally and generally.

We need not follow our author into the history of climate as influencing phthisis; in that he offers nothing novel: we may only remark, as cognate to what we are now speaking of, that in the favorable influence known to be exercised by the climate of marshy districts upon phthisical patients, which disappears when these districts are drained, other circumstances remaining the same, he recognises the influence which marsh miasma has in diminishing the amount of ozone—oxygen in its most active state—in the atmosphere. In conclusion, we hope that this little book will direct attention to the facts—1st, that there is no pre-tuberculotic stage recognisable by physical signs, but that it is solely to be ascertained by family history; 2nd, that the treatment of that stage is that required for the diathesis; 3rd, that when the presence of tubercle or pseudo-tubercle can be recognised by physical signs, the treatment to be adopted is a judicious combination of pulmonary and general dietetics, the prognosis depending upon the family history; 4th, that while the very nature of true phthisis precludes the prospect of a perfect cure in all but the very rarest cases, yet a judicious system of treatment must prolong life in every case, and is that best adapted to secure a cure where that is possible; and, 5th, that phthisis may in time be as effectually extinguished as enteric fever, or any other contagious disease, by a judicious system of prophylaxis directed towards the individual and the public. However difficult this task may be, it is one well worthy of being attempted, and specially so in an age so pre-eminently distinguished as this is for its consistent and enlightened endeavours to establish sanitary science upon a broad and sure foundation.

Review IV.

1. *Nogle Bemærkninger om Befolkningsforholdene med særligt Hensyn til Antallet af mandlige og quindelige Individer.* Af Professor Dr. Faye. (Aftryck ur 'förh. vid de Skand. Naturf. nionde möte år 1863.') *Stockholm,* 1866. P. A. Norstedt og Söner.

Some Remarks upon the Proportions of the Population, with special reference to the number of Male and Female Individuals. By Professor Faye, M.D. (Reprint from the 'Transactions of the Ninth Meeting of Scandinavian Naturalists.') 8vo, pp. 16.

2. *Statistiske Undersögelser angaaende den operative Födselshjælp i Norge i Tidsrummet fra* 1853 *til* 1863. Ved Dr. F. C. Faye, Professor og Overlæge, og E. Schönberg, Reservelæge. *Christiania :* Carl C. Werner og Co. 1866. (Aftrykt fra 'Norsk Magazin for Lægevidenskaben,' xx Bd.)

Statistical Observations respecting Operative Midwifery in Norway in the period 1853–1863. By Dr. F. C. Faye, Professor and Physician in Chief, and E. Schönberg, Physician Extraordinary. 8vo, pp. 63. (Reprint from the 'Norwegian Magazine of Medical Science,' vol. xx.)

3. *Statistiske Resultater stöttede til* 3000 *paa Födselsstiftelsen i Christiania undersögte Svangre og Födende samt Börn.* Ved Dr. F. C. Faye, Professor og Overlæge og H. Vogt, Reservlæge. *Christiania :* Carl C. Werner og Co. (Særskilt aftrykt af 'Norsk Magazin for Lægevidenskaben,' xx Bd.) 1866. 8vo, pp. 110.

Statistical Results based upon the Examination of 3000 *Pregnant and Parturient Women, and Children, in the Lying-in Institution in Christiania.* By Dr. F. C. Faye, Professor and Physician in Chief, and H. Vogt, Physician Extraordinary. (Reprint from the same.)

4. *Om Hudens Reactionsevne.* Ved Prof. Dr. F. C. Faye. 8vo, pp. 12. (Aftrykt fra 'Norsk Magazin for Lægevidenskaben,' xx Bd.)

On the Reactive Power of the Skin. By Professor Dr. F. C. Faye. (Reprint from the same.)

1.—Professor Faye's object in the first named of the above works is to investigate the numerical relations of the two sexes, and to glance at the conditions on which the disproportion found to exist depends, so far as our knowledge on this subject will permit.

It is well known that in all countries where statistical records are kept, the female sex is found to predominate. Of the countries mentioned by the author, Sweden, which possesses such records extending through a period of 105 years, shows the greatest disproportion—viz., 109 females to 100

males. During the last quinquennial period the disproportion was not so great, the numbers being 106 to 100. In the towns the proportion is 116 females to 100 males, exceeding that of London, where there are 113 females to 100 males. It is evident that such a disproportion must depend upon migration, as the same statistics, extending over 105 years, show 984·9 deaths of females to 1000 of males. In the period 1856–60, the proportion is 969·1 deaths of females to 1000 of males.

The author next proceeds to examine the circumstances which seem intended to counterbalance the disproportion referred to. Wappæus, giving the average of 58¼ millions of human beings, found that 106·33 living boys were born for 100 girls. Professor Faye gives numerical data for several European countries, varying from Russia, which exhibits 1089 boys to 1000 girls, to Sweden (1855–60), which has but 1047 of the former to 1000 of the latter. In Norway, each of the last three decennial periods has shown a diminution of the compensating disproportion, the numbers being respectively 1062, 1061, and 1057.

Professor Faye remarks that a still greater preponderance of males would be met with, if death struck both sexes in equal proportion in the womb and during birth. But the fact is, that for every 1000 stillborn females, we have, in the several countries, from 1346 to 1449 stillborn males. It would at first sight appear as if many male germs are destined to die, that the number of that sex may not preponderate. But whether this be so, the subsequent occupation of the man, and probably also his want of a natural mode of life, has inverted the order, so that notwithstanding the preponderance remains on the woman's side. During the first years of life, too, the mortality is greater on the male side: for example—in England, in the first year, 126 boys die for every 100 girls—a proportion which in France is still more unfavorable. This preponderance gradually diminishes, until at 4 or 5 years the mortality is about equal for both sexes. From 15 to 40 years the mortality is greater on the female side, but not sufficiently so to restore the equilibrium. A fact remains to be taken into consideration—viz., that the relative excess of the male sex is less with respect to illegitimate than to legitimate children.

Properly speaking, Johan Peter Süssmilch[1] was the first who endeavoured to show that one of the most constant natural laws or divine arrangements is, that more boys than girls are born in the proportion of 21 : 20, or of 26 : 25 = 105 : 100, or

[1] 'Die göttliche Ordnung in den Veränderungen des menschlichen Geschlechts,' u. s. v. 4 Ausg., Berlin, 1796, 2 Theil. [The Divine Arrangement in the Variations of the Human Race.]

104 : 100; a proportion which is somewhat lower than that deduced by Wappæus from more extensive statistics. A natural result of this announcement was, that other investigators began to study more closely this supposed law, with a view, if possible, to fathom its conditions. Among these, Professor Hofacker of Tübingen, and Michael Thomas Sadler in England ('Law of Population,' London, 1830, vol. iv), put forward the relative ages of the parents as an important element in determining the sex of the children. From the researches of these two writers, the Hofacker-Sadlerian natural law has been deduced, which many have received as certain and well established, and which, according to Professor Leuckart (Wagner's 'Handwörterbuch der Physiologie,' Band iv), may be thus formulated:

1. Elder fathers give a preponderating number of male births.
2. Parents of equal age make the number of girls a little greater.
3. Elder mothers give a preponderance of female births.

But the numbers on which these conclusions are based are very small, although Sadler considered them sufficient; and later investigations have shown that the proportion is not so constant as is here put forward.

J. V. Göehlert[1] collected data from 25 years of the Gotha Genealogical Almanack, respecting princely families in most European countries, taking only first marriages into consideration, and from rather larger numbers corroborated the above law.

The researches of Legoyt, Noirot, and Boudin, in France, tend also, though not so strongly, in the same direction.

Other writers have recently partly decided that the so-called natural law above referred to is not constant; and Dr. Stampe and Professor Breslau of Zürich, have come to a conclusion quite opposed to the law in question—viz., that in marriages o elder mothers with younger fathers, male births preponderate Professor Breslau's[2] conclusions are based upon 16,492 observa tions.

"It is undoubted," adds Professor Faye, "that age is not the only element at work, and that we can scarcely say that we have here an actual natural law, invariable in its simplicity, before us. For, according to numerous observations, a certain natural power, which will also within certain limits depend upon difference of age and a not too frequent intercourse, will exhibit an especial influence.

[1] 'Sitzungsberichte der phil.-hist. Classe der kaiserl. Acad. d. Wissensch.,' B. xii.

[2] "Zur Würdigung des Hofacker-Sadlerschen Gesetzes 2ter Beitrag," in 'Monatsschrift f. Geburtskunde,' 22 B., 2 H., 1863.

This latter condition must come pre-eminently into consideration as an explanation of the fact that poor people very frequently beget a large excess of boys." P. 12.

The author adds a curious table of the relative numbers of males and females born in the different ranks in Sweden, as shown by the statistics recently published for the quinquennial period 1856–60:

The nobility show the proportion of males,				971·3	to females,	1000.
„ clergy	„	„	„	108	„	100.
„ gentry	„	„	„	105	„	„
„ burgher class	„	„	„	104	„	„
„ peasant class	„	„	„	105·8	„	„
All others	„	„	„	103	„	„

Professor Faye remarks, that it may

"with tolerable certainty be inferred that difference of age in the parents plays a not unimportant part in determining the difference of sex in their posterity; but as several later investigators have found so many deviations from the rule, that these cannot be without significance, we cannot put forward the results of experience on this point as an established natural law."

He further directs attention to the fact that in some districts of Norway there has been, during a decennial period, a steady deficiency of boys, while in others the opposite condition has existed. Dr. Ploss, of Leipsic, has, in a couple of essays, referred to many causes as co-operating in producing difference of sex—as the quantity and nature of the food, the elevation of the residence, the temperature, the mode of life of the parents, the rank, frequency of intercourse, &c., besides the age.

"The fact, that, for example, after great wars, and to a certain extent after epidemics, in which a disproportionate number of males has perished, more boys than usual are born, is still unexplained; but no doubt more accurate inquiry will throw light also upon this circumstance. Another matter of experience, which tends to show that men who lead a sedentary life, and especially men of learning, who use their nervous power extensively, beget comparatively more girls, may, if it be confirmed for larger numbers, certainly also be with advantage more closely studied; for, although it may be true that the relative difference in age is of great importance, it cannot be less true that this has its limits, and that a very advanced age in the man, a weakened system, and especially too frequent intercourse, diminish the number of males among the children." P. 14.

The author proceeds to remark on the many efforts now being made to obtain for the weaker sex an independent position in society. He does not, however, sympathise much with these efforts, but considers that—

"Woman herself will find her greatest happiness and most satisfactory activity in a circle where she feels herself most at home. No one can tend an infant so well as woman in her home; and it is a fact, fully established by experience, that many children who now die off in the first years of life, and of them especially boys, might be saved by good nursing." Pp. 15, 16.

It will be seen that Professor Faye's essay is rather suggestive of investigation than containing any very positive information, or at least presenting any very decided conclusions. If it be really the case that "after great wars, and after epidemics, in which a disproportionate number of males has perished, more boys than usual are born,"—a fact which, the author says, "is still unexplained,"—we are, we think, at once "shut up" to the inference that the arrangement is due to the providential care of Him who has "weighed the mountains in scales, and the hills in a balance."

2.—We now turn to the second work upon our list, the 'Statistical Observations respecting Operative Midwifery in Norway in the period 1853–1863,' the joint production of Professor Faye and Herr Schönberg. The materials from which the information in this work is derived are principally the Reports on the sanitary condition and medical state of Norway, separately published since 1853 by the Ministerial Department of the Interior. We shall now proceed briefly to extract from the work a notice of some of the more striking facts recorded in it.

In the eleven years 1853–1863, 576,095 births took place in Norway, of which were—

Single births	568,941	
Twin births	7,058,	or 1 in 81·6.
Triplet births	96 „	6000.

Consequently, these births brought into the world 583,345 children.

In giving the numbers of obstetrical operations performed in Norway during the period above mentioned, those which took place in the Lying-in Institution in Christiania are not included; consequently, the 1903 children born there must also be subtracted, leaving the total number of children born 581,442. The reports for the first six years being less complete, the authors divide the whole period of eleven years into two periods of six and five years respectively.

In the six years 1853–1858, the number of children born in Norway, exclusive of 1151 who first saw the light in the Christiania Institution, was 307,386. The operations and

births occurring in the institution opened in April, 1861, in Bergen, have been included.

Of the 307,386 children born in 1853–1858, 1488 were delivered with the forceps, 310 by turning, 29 by breech extraction, 88 by perforation, 43 by embryotomy, 10 by premature artificial delivery, 2 by Cesarean operation on the living, 5 by the same on the dead subject, 5 by turning in the dead; the placenta operations were 105. Total operations, including 2 the nature of which is not given, 2087.

The second period exhibits a difference in the frequency of operations, which the authors attribute to defective reports during the first period.

In the five years 1859–1863, the deliveries in the whole kingdom were 271,425; the children born were 274,808. In the Christiania Institution the births were 752, leaving 274,056 as the number of children born elsewhere.

The operations in these five years were—forceps, 1670; turning, 352; breech extraction, 48; perforation and cephalotripsy, 95; embryotomy, 45; artificial premature delivery, 9; Cesarean section on the living, 3; do. on the dead subject, 2; placental operations, 152. Total, 2376.

At page 18, a table is given, which, the authors observe,

"may be assumed to give a tolerably correct representation of the frequency of obstetrical operations in Norway, and which may, upon this supposition, be best employed as a means of comparing the frequency of operative interference there and in other countries outside the lying-in institutions, so far as the necessary data exist." P. 19.

According to the table referred to, it appears that in the five years 1859-1863 the proportion of the respective operations to 1000 children born were as follow:

Forceps, 6·1; turning, 1·2; extraction, 0·14; perforation and cephalotripsy, 0·34; embryotomy, 0·16; artificial premature delivery, 0·03; Cesarean section on the living, 0·01; ditto on the dead subject, 0·01; placental operations, 0·55. Total operations, 8·6.

The authors lament, however, that, with the exception of some countries and districts of Germany, accurate statistics of operative midwifery outside the lying-in institutions do not exist—a want which their present work is intended to supply with respect to Norway. In Sweden the list published in the 'Yearly Reports' from the College of Health embraces only obstetrical operations performed by the midwives, who, in that country, employ both the forceps and cutting instruments.

A comparison of the statistics published for Saxony, Wür-

temberg, part of Bavaria, Nassau, Curhessen, and Baden, Circle of the Upper Rhine, of such operations as are in these countries not performed by midwives, viz. delivery by the forceps, perforation and embryotomy, artificial delivery and Cesarean section, exhibits a greater frequency of such operations in the countries enumerated than in Norway in the proportion of 16·1 to 6·6 per thousand. Of these delivery with the forceps is represented by 15·5 to 6·1 per thousand.

It appears that there is a great difference in the frequency with which operative interference is resorted to in the different parts of the kingdom. This difference is so great, that the authors are inclined to think that it must depend partly on imperfection in the reports. Among the other causes which may produce such a difference in different countries or parts of a country, are the views prevailing in the schools where the practitioners have received their education, the constitution and figure of the female population, the relative number of accoucheurs, the density of the population, and especially the difference between town and country residence. Thus in Finmark the population consists of different nationalities with remarkable peculiarities of race in the form of the pelvis, and in part with one of the advantages of a low scale of civilisation, facility in giving birth.

The authors observe that the number of women who from year to year die undelivered would show that in Norway the need of operative obstetrical aid exceeds the supply. The total number of such cases for the eleven years 1853-1863 is 222, being in the proportion of 1 in 2596 of the total number of births. The largeness of this proportion is, no doubt, owing in great part to the prejudices of the lower orders against operative interference in midwifery.

The returns which have been made of the results of obstetrical operations throughout the kingdom are as yet very imperfect. From those available of the results as concerns the mother, it would appear that in the eleven years 1853-1863, of—

1678	cases of	forceps delivery	. .	136	were fatal,	or 1 in	12·3.
269	„	turning		65	„	1 „	4·1.
19	„	extraction		4	„	1 „	4·7.
128	„	perforation and cephalotripsy		64	„	1 „	2.
62	„	embryotomy . . .		27	„	1 „	2·3.
14	„	premature artificial delivery		5	„	1 „	2·8.
5	„	Cesarean section . .		5	„	1 „	1.
29	„	placental operations . .		10	„	1 „	2·9.

Showing a total mortality of 316 out of 2204 operations, or 1 in 6·9.

Ploss, in the work already mentioned, gives the results for the mothers, of obstetrical operations in several German States. According to him, there died in Saxony in the years 1835–1841, after artificial deliveries of all kinds,—of 26,253 mothers, 821, or 1 in 31·9: but in these cases the time for making the report is limited to fourteen days after delivery.

According to Riecke, there died in Würtemberg, in the years 1821-1825,—of 7970 mothers, 618, or 1 in 12·9. According to Sick, the proportion of deaths in the same kingdom in the years 1846-1856, after obstetrical operations of all kinds, was 1 in 23 or 24.

According to Schwörer, there died in Baden, in the Circle of the Upper Rhine, in the years 1843-44, 33 out of 587 mothers, being in the proportion of 1 in 17·7.

From Mayer's report on part of Bavaria, it would appear that the proportion of deaths in the district referred to was 1 in 23.

It does not appear what the limit of time is in these later cases after delivery, within which the result of the operation is deduced. It is probable that, owing to local conditions, such as the density of the population, the operations were performed under more favorable circumstances than in Norway, where the distances are great, and where the physician has to be brought from remote places to women exhausted with tedious labours, and where the manual operations are often undertaken after fruitless efforts at delivery by the midwives.

We have next a chapter on the mortality in childbirth. During the four years 1851-1854, there died in Norway, of 189,485 puerperal women, 1411 = 1 : 134·1 = 0·74 per cent.

At page 54 a table is given of the results *for the child* of operations in which embryo-mutilating instruments were not employed. We may observe that the term "stillborn" is applied to children which either come into the world actually dead, or die within twenty-four hours after birth.

In 1981 forceps-cases occurring in the eleven years 1853-1863, 591 children were stillborn=1 in 3·3. In 393 cases of turning, 249 were stillborn=1 in 1·5. In 39 of extraction, the stillborn were 24=1 in 1·6. In 18 of artificial premature delivery, the stillborn were 11=1 in 1·6. In 5 of Cesarean section in the living, 2 were stillborn=1 in 2·5. In 3 of the same operation on the dead subject, 3 were dead=1 in 1. In 4 of turning in the dead subject, 4 were dead = 1 in 1. In a total of 2443 operations, 884 were stillborn=1 in 2·7.

The authors conclude their work with a somewhat detailed tabular account of all the Cesarean sections performed in Norway since 1843, previously to which date there is no record of the operation having been undertaken in that kingdom. The

total number of Cesarean sections performed on the living subject is 10, in all of which the result was fatal to the mother. In 5 the result was fatal also to the children; in 5 the children lived.

3.—The statistical results based upon the examination of 3000 pregnant and parturient women and children, in the Lying-in Institution in Christiania, are a monument of the laborious industry and zeal of Professor Faye and Hr. Vogt. Of these results we shall endeavour to lay, as briefly as possible, an abstract before our readers.

The records from which these results are deduced extend over a period of seventeen years, from 1846 to the middle of 1863, during which Professor Faye has presided over the institution, and in which 3030 births have taken place therein. The calculations are made on 3000, as a round number sufficiently near the truth. The youngest parturient woman was 16½ years of age, the eldest was 49½. Only 716,= 23·9 per cent. of the total number of women, were married. Of the 2284 unmarried women, 86·6 per cent. belonged to the class of domestic servants —"a sad evidence," observe the authors, "of the morality (immorality ?) of these individuals, or, probably more correctly, of the temptations to which they are exposed in this city, as in other great cities under similar circumstances."

Fifty-five per cent. of the women delivered in the institution were primiparous. The average number of pregnancies per head was 1·85.

Menstruation set in first in one case at 27 years of age. In this instance the woman had become pregnant without having menstruated, and the function commenced after delivery. In 4 instances menstruation occurred first in the 24th year—in 6 it began in the 11th.

The case is recorded of one woman who had a regular sanguineous excretion from the genitals every month from the close of the *first* year. She was small, almost dwarfish, and both her mother and grandmother had menstruated at a very early age. The upper opening of the pelvis measured only 2″ 10‴. She was twice delivered by mutilation of the fœtus; and on the third occasion premature delivery was induced, after which she died of puerperal fever, then prevalent.

The average age at which menstruation commenced in 2691 women was 16·375 years; 302 were unable to state when it commenced, and in 6 primiparæ it had never occurred.

In 72·9 per cent. of the women the interval of menstruation was regular—four weeks. In 26·7 per cent. the menses lasted from three to four days, in 16 per cent. from two to

three days, and in 14·7 per cent. from seven to eight days. The per-centage of other periods was much smaller.

Only 11 women considered that they menstruated after pregnancy had set in. One woman stated that she had menstruated once, two that they had menstruated three times regularly, one four times with eight days' interval in the beginning of pregnancy, three during the whole pregnancy except the last month, three during the whole of one pregnancy, one during repeated pregnancies. The authors consider that some of these were instances of slight hæmorrhage, which it would be incorrect to look upon as menstruation; in any case, they think it probable that the excretion took place from the vagina, and not from the inner surface of the uterus.

The total number of multiparæ among the 3000 was 1327; and of these 125, or 10·6 per cent., menstruated while nursing. An extended table is given of the period at which this occurred in each case.

In 2476 accurately examined pregnant women, the dark abdominal line was present in 2269, or 91·6 per cent. (distinctly seen in 1643, indistinctly in 626). In 207, or 8·4 per cent., it was not present; of the latter number 96 were primiparæ. But a distinct brown line may be met with in women who have never been pregnant.

The follicular hypertrophy which in pregnancy frequently exhibits itself in the areola of the breasts was distinctly found in 2282 women; in 26 it was indistinct, in 97 it was absent, in 595 it was not mentioned.

Lineæ albicantes abdominales were absent in 30 cases out of 514, that is, in 5·8 per cent. The authors therefore consider this sign to be not reliable.

In 2881 births in which the position of the fœtus was accurately noted, the head presented—

In the first position (forehead towards the right half of the pelvis) in	2178	cases.
In the second position (forehead towards the left half of the pelvis) in	588	„
In the first position, head with the forehead presenting backwards in	2143	„
„ „ „ „ forwards in .	35	„
„ second „ „ backwards in	578	„
„ „ „ „ forwards in .	10	„
Position of the vertex not accurately determined	24	„

The percentages of these presentations may be thus stated:

	Per cent
Head presentation with the forehead looking backwards and to the right side	74·38
„ „ „ forwards „ „	1·21
„ „ „ backwards and to the left side .	20·06
„ „ „ forwards „ „	0·34
„ not accurately determined	0·87

The face presented eight times, being once in 360 deliveries = 0·28 per cent.

The pelvic extremity presented 71 times, or 2·46 per cent. Of these presentations, 41 were breech, 20 were footling, 1 was knee, and 2 were footling and breech combined. The others are not more accurately stated.

The cross presentations were 12 = 0·42 per cent. The twin cases were 46; of the 92 children in these cases 57 were head presentations. Triplets occurred only once since 1846 in a total of about 3500. The triplets were of the female sex.

The children born under 5lb. weight were 202; of these the head presentations were only 68 per cent. instead of 94 per cent., the proportion in mature and full-grown children.

As to the *sex* of the children—of 2995, 1502, or 50·3 per cent., were males; 1490, or 49·7 per cent., were girls. The authors account for the slight excess of males by the fact that a large number of girls are born of loose characters. Of the 91 twin and triplet children, 39 were males and 52 were females (of the above 2995, 88 were twins and 3 were triplets).

We shall pass over the chapters on the weight of the children, their length, the circumference of the head with reference to the sex, the place of insertion of the funis, and on the length and twisting the cord.

As to the *duration of pregnancy*, Professor Faye is able to state positively one case in which the period between insemination and the commencement of labour was 279 days and 15 hours. This case is mentioned in the table given at page 566 of the second edition of the late Professor Montgomery's work on 'The Signs and Symptoms of Pregnancy.'

The date of the last menstruation was stated to the day by 240 women. The shortest period from this occurrence to the completion of labour was 210 days, in which case the child weighed 7lb. 15oz. The longest period was 337 days, the average 281·13. This extensive range of the duration of pregnancy is highly improbable; accordingly, the authors add, at page 53, a table of the particulars furnished by 63 women, on whose statements as to the day of conception the authors, after due investigation, found themselves justified in placing a considerable amount of reliance. According to this table, the mean duration of pregnancy would be 270 days, which is probably not far from the truth. Herr Vogt is able positively to testify to one case in which pregnancy lasted in a married woman from 278 to 280 days, reckoned from the date of conception.

We have next a chapter on the duration of labour, and one on the same in connection with the size of the head. From an

investigation as to the relative frequency of the occurrence of labour in the different periods of the twenty-four hours, it would appear that of 2845 births, 51·6 per cent. took place during the night, and 48·4 per cent. during the day.

The remaining sections of the work treat of the mortality among puerperal women according to their various circumstances of age, condition, &c., and also of that among the children. The concluding pages of the work are devoted to a brief review of the statistics of operative midwifery in the institution, which it would be interesting, did our space permit, to compare with those we have already given of the same outside the hospital.

4.—The last work on our list is a very short essay, by Professor Faye, on the "supposed property in the human skin, that its susceptibility to external irritants may gradually, during their repeated employment, undergo such a change that it becomes less and less capable of, or disposed to, being affected by a given irritant; in other words, a condition which I have elsewhere denominated immunity of the skin with respect to certain irritating agents." The subject is wholly different from that of the works we have been considering; and as the author does not seem to have arrived at any very definite conclusions, we shall content ourselves with giving, in his own words, the purport of the pamphlet.

Review V.

English Public Asylum Reports for 1865.

In their main features these Reports differ but little from those of preceding years, and it requires some ingenuity to extract from them anything of interest to the profession at large. We are glad, however, to notice an improvement in one important particular, viz. in the statistical records. We have more than once in this Review spoken of the evils resulting from the utter want of concert or uniformity in arranging the statistics of the various county asylums, and the consequent loss to the profession of valuable information; and we are, therefore, the more glad to welcome the first traces of reform in this matter. After years of reform and varied legislation, during which time many able men have been attracted to the service of the insane, it has come to pass that *six* of the public asylums in this country have agreed to frame their statistics on a common plan, and

thus make them of some use beyond astonishing the curious in individual counties. This change has been mainly brought about by the efforts of a few of the Association of Asylum Superintendents; and we may fairly hope that others will imitate their example, and assist in a measure of such obvious utility.

Passing to the details of individual asylums, we notice that in more than one smallpox has been prevalent, as was the case in 1864, and several deaths from this cause are reported. Thus, at the Birmingham, Sussex, Lincoln, and Prestwich (Lancashire) Asylums, variola showed itself; and at Sussex forty cases occurred, of which five died; while at Prestwich there were also patients with typhus at the same time. At the Lincoln Asylum every person was vaccinated, as a measure of precaution.

Several curious accidents to patients are reported. For instance, at the Stafford Asylum, we read—

"A female, suffering from melancholia, succeeded in scaling a fireguard, 4 ft. 6 in. high, and crawling up a chimney whilst a fire was burning in the gate. Although the nurse, who was only a few yards off at the time, succeeded in a few moments in removing her, at great risk to herself, the charred corpse of the suicide alone remained."

At the Hants Asylum, a man dislocated his shoulder twice in the year, having done the same thing five times before in the asylum.

Dr. Needham mentions the following case in his report of the York Lunatic Asylum:

"A female patient was admitted with acute mania, and in a most feeble state of bodily health. She had been ill for more than eight months, and had during that time frequently refused food, *never* taking a sufficient quantity to repair the rapidly progressing waste. From the day of her admission, large quantities of food and stimulants were administered to her, together with appropriate medicines, but for many months the emaciation continued to progress, until she became a complete skeleton, weighing little more than four stones, and for long appeared to be daily at the point of death. The treatment was, however, persevered in; and when at the worst, amendment suddenly commenced. The craving for food became immense: she was fed frequently, and the powers of digestion increased; she gained nearly three stones in less than as many months, and is at the present time rapidly approaching convalescence both of mind and body."

Still more remarkable is a case detailed by Dr. Crichton Browne, at the Newcastle Asylum—that of a young woman who died from phthisis and hemiplegia—and we quote the striking description he gives of it in his own words:

"Before her insanity was officially recognised, she had been in the habit of escaping from home, and living in solitude in the woods, feeding upon wild fruits, or what she could occasionally beg at a cottage, and sleeping in the brushwood. She had frequently lived in this manner for a fortnight at a time, seeking no converse with human beings. While upon one of these expeditions, she was delivered of twins. At this time she was without attendance or help. She sought out a sheltered hollow for her delivery, and there, reverting to a primitive instinct, she actually gnawed through the umbilical cords with her teeth. The twins were alive when found two days after birth; but the mother was in a very exhausted state, having had no food or covering since her delivery. When brought to this asylum, she was the most perfect specimen of brutalised humanity that I have ever encountered. The most casual observers on seeing her had powerfully suggested to them her striking resemblance to a wild animal. Her hair had all been cut or broken off short, while over the summit of each parietal bone the skin was quite denuded, owing to frequent rubbing, not with the fingers, but with the inner side of the hand and wrist. The forehead was small, receding, and deeply wrinkled. The cheek-bones were naturally prominent, and appeared even more so, in consequence of the emaciation of the face, which also strongly delineated massive temporal muscles. The eyelids were without lashes, and so opened as to expose a large rounded portion of the eyeballs, thus giving the countenance a staring appearance. The width between the eyes was considerable. The bridge of the nose was depressed, the alæ wide-spread, the nostrils forward, opening. The lips were thin and always retracted, so as to display a grinning set of teeth. The lower jaw was protuberant, and the chin was always held forward. The complexion was sallow, and the features exhibited no play of expression. The limbs were wasted; and the fingers were fixedly contracted or flexed, and had the aspect of claws. The habits of this woman, as well as her bodily configuration, had many points in common with those of wild creatures. She was averse to a sitting posture, and preferred to lie upon the ground, or to run about in a half-stooping attitude. As she lay, she drew up her legs, and twisted herself into the most singular contortions. When stared at, she winked forcibly and repeatedly. She used her teeth for tearing and gnawing, as well as mastication; and she bit and licked the hands of those who fed her, as a mark of her fondness for them. She never expressed coherent thoughts, but used a few words strung together without apparently attaching any meaning to them. She constantly emitted undescribable gibbering noises. Her only attachment was for the nurses who fed her, from whom she would snatch the food brought with sudden avidity, turning away to devour it in secret. Her greatest pleasure seemed to consist in chewing a hard sea-biscuit. When awake, she was invariably restless, picking, rubbing, rocking her body, and moving about. Sometimes for a whole night she would pace round her room sideways, placing her feet heavily down with the most rhythmical regularity, and clasping a pillow in

her arms. She was destructive to furniture, especially to crockery, which she delighted in breaking; but she did not injure her own clothing. When out in the airing court, she would pull up the flowers and grass, and toss them wildly above her head. And yet this woman, presenting so many animal traits, had been of average intelligence previous to her insanity, and had adequately discharged the duties of her station in life."

Of the deaths in asylums, quite one *half* are due to diseases of the brain. Adding up the deaths in thirteen of the reports, representing a total of 667, we find that 338 were classified under the head of cerebral disease, including, of course, general paresis. This, perhaps, hardly accords with the general notion that the insane enjoy good health, and that the mental symptoms are, as a rule, something superadded, and would indicate that they die from diseases of the nervous system in larger proportion than obtains among the public at large. Some of the deaths are from unusual forms of disease. Thus, at Surrey, one is reported from rupture of the left ventricle of the heart; at Oxford a patient died from "pericarditis and abscess of the lung and brain," and another, in cutting his throat, managed to open the internal jugular vein. At Abergavenny a man died from ruptured intestine, caused by a kick from another patient; and at the Lincoln Asylum, the sudden death of an epileptic is accounted for by Dr. Palmer as follows:

"The easy position in which she was found, and the entire absence of venous congestion of her features, indicated that she had not died of asphyxia, but in all probability of *paralysis of the heart*, occurring at the onset of convulsions."

At Rainhill we read of a patient who, while eating her dinner, got a small piece of bone impacted in her throat:

"This was removed immediately; but inflammation was excited, which was followed by an abscess, having its seat behind the windpipe and gullet, and the great vessels and nerves of the neck. An attempt to open the abscess failed, from its extremely dangerous position, and the patient, though much relieved by the attempt, died suddenly a few hours afterwards from pressure on the nerves of respiration."

Dr. Boyd, of Somerset, gives some very complete tables on a variety of subjects, and has noted the hour at which death occurred in 476 men and 340 women. From this it appears that among the men 21 per cent. died between midnight and six a.m., and 27 between six a.m. and noon; 23 during the next six hours; and 24 per cent. from six p.m. to midnight. Taking

the same periods, the figures are, for women, 19, 26, 24, and 26 per cent. respectively.

Of 152 deaths from general paralysis, 27 per cent. were men and 6 per cent. women.

Dr. Clouston, of Carlisle, alludes, in his report, to the great preponderance of physical causes in the cases admitted, and says—

"This would seem to be a characteristic of insanity as it manifests itself in an agricultural district and among a rural people. In no less than 10 of the 34 women admitted was the insanity attributable to childbirth and nursing. In 4 of these cases had they given birth to illegitimate children. This merely confirms what has long been known, viz. that puerperal insanity is much more frequent among the mothers of illegitimate children than other women."

Epilepsy is such an important item in every asylum, that we have been tempted to inquire what proportion of the patients were so affected in the various districts; and we have been able to find in eleven reports some information on the subject. Taking the total population of these eleven asylums, amounting to 6220, 15·2 per cent. prove to be epileptic; but the proportion between the sexes varies thus:—At Somerset the percentage is 19·5 among the men, and 12·9 among the women; at Essex, 16·9 of the men and 11·6 of the women; at Bristol, 24·5 men and 20·4 women; at Oxford, 24·5 men and 20·4 women; and at Stafford, 19·8 men and 21 women.

As regards the treatment of this disease, bromide of potassium seems the favorite remedy. The Superintendent of the Denbigh Asylum considers, in his report, the question of the amount of lunacy in his district, and comes to the conclusion, that decidedly "insanity is not so prevalent in North Wales as in other parts of the kingdom." He bases this opinion on a comparison of the number of patients in the Denbigh and several English asylums relatively to the population of the counties, and thinks that as the proportion of patients to the population is lower in Denbigh, there is less insanity. Such a method, however, is no fair criterion, for the number in the asylum does not represent the whole of the recognised pauper insanity in the county, and all asylums are not similarly situated. For instance, in the list given for comparison, we find the Leicester Asylum, which takes private patients, not, of course, merely from the district; the Cambridge, which has patients from Ipswich; and the Cumberland, which had to refuse admission to forty-seven in the year. If a county asylum takes patients from a town in another county, or private patients from various places, or has to refuse admis-

sion to forty-seven cases in a year, the statistics of such an asylum must necessarily be very little guide to the amount of insanity in the county whose lunatics it accommodates. These are all sources of fallacy; and when, moreover, we cannot find the figures given by Mr. Jones correspond in one single instance with the numbers stated in the reports of the various asylums quoted, nor with those in the 'Lunacy Commissioners' Report,' we feel still more doubtful as to the accuracy of his conclusions. Thus, the list alluded to is headed by the Denbigh Asylum with 248 patients, "according to returns made on the 1st January, 1865;" but if we look at p. 14 of Mr. Jones's own report, we find a total of 275 patients on the very same day! The fairest way to estimate the insanity in any county is to take the whole number of insane both in and out of asylums, and find the ratio to the population. Adopting this plan, we can point to several districts more favoured than North Wales as regards their lunacy rate. Reckoning under the term North Wales, Denbigh, Flint, Anglesey, Carnarvon, and Merionethshire, we find 1 insane of every 458 in that district; but in the united counties of Monmouth, Brecon, and Radnor, the ratio is 1 in 500; in Lincoln, 1 in 518; in Devon, 1 in 482; in Glamorgan, 1 in 655; and for Staffordshire, 1 in 722,—all less than North Wales. The general rate of insanity for England and Wales is supposed to be 1 in 561 of the whole population, but it varies in districts very greatly.

The following table gives a view of the proportion of the insane in the several counties named, according to the lists of lunatics and idiots known to the parish authorities, as published in the asylum reports.

Table showing the proportion of the Insane Poor to the population in the following counties:

		Male.		Female.
Lincolnshire . .	1 in 518, or 1 in	605	and	453
Cambs . . .	„ 439, „	461	„	420
Surrey . . .	„ 411, „	470	„	369
Bucks . . .	„ 419, „	477	„	365
Devon . . .	„ 482, „	546	„	435
Wilts . . .	„ 344, „	411	„	297
Kent . . .	„ 542, „	679	„	452
North Wales . .	„ 458, „	502	„	421
South „ . .	„ 500			
Dorset . . .	„ 371			
Stafford . . .	„ 722			

Taking the total number of insane included in the Commissioners' returns on Jan. 1st, 1865, of all classes, the propor-

tion of insane to the population would be 1 in every 556; and this probably is no unfair standard.

Dr. Thurnam, in his report, gives two valuable tables of the weights of the brain in 470 cases. His investigations have been very complete and laborious on this point, and were published at length in the 'Journal of Mental Science' (April, 1866). We can only give an outline of his conclusions in this place. Of 257 men, he found the average weight of the cerebrum to be 40·7 oz. in those between 16 and 20 years of age; 40·3 between 20 and 60; and 40·1 between 60 and 87. For the same periods, the weights of the cerebellum were 5·9, 6, and 6 respectively; and of the encephalon, 46·6, 46·3, and 46·1.

Among women the average weights of the cerebrum (in 213 cases) were—between the ages of 9 and 20 years, 40·1; between 20 and 60, 35·7; and between 60 and 92, 35·4: of the cerebellum, 5·5, 5·4, and 5·3; and of the encephalon, 45·6, 41·1, and 40·7.

Another worker at cerebral pathology, Dr. C. Bastian, has made some extensive and elaborate analyses of the specific gravity of different portions of the brain and spinal cord. It is difficult to speak too highly of such conscientious and painstaking work, so valuable, and yet so little appreciated; but the details are too voluminous to transfer to these pages.

We notice a curious case in the obituary at the Broadmoor Criminal Asylum, viz. that of a man who died from the effects of foreign bodies in the intestines. The following note was made of the post-mortem appearances:

"Lower part of sigmoid flexure and rectum distended, filling pelvis, and adherent to parts around. When opened, found to contain portions of rag, &c., sufficient to fill a pint and a half measure; five small rib bones; and an iron tap-handle, five inches in length. Considerable ulceration of mucous membrane of this portion of intestines."

We should be glad to notice more real professional work at the hands of those holding appointments in the public asylums, and probably the increased number of students of insanity may in time bring about such a result; but in the meanwhile we must be thankful for the labours of the few who have so honorably distinguished themselves, and have risen superior to the attractions of the business details that weigh so heavily on the bulk of their brethren in office.

Review VI.

Report on Leprosy by the Royal College of Physicians, prepared for Her Majesty's Secretary of State for the Colonies. London. 1866. Folio, pp. 344.

The inquiry, the results of which are given in this Report, was commenced, as our readers are aware from notices contained in preceding numbers of our Review, as long ago as 1863. That there should have been so considerable an interval between its initiation and completion is not to be wondered at, taking into account the sources from which the embodied information was obtained—these almost all our extensive colonies, so widely apart, including India, besides several foreign countries—and the delays almost unavoidable, when, as in the present instance, the contributors have been so numerous.

Before noticing the results or offering any opinion regarding their value, we think it but justice to advert to the obligations we are under to all concerned in the laudable undertaking of endeavouring to elucidate a disease so formidable, so loathsome, and so obscure as that in question.

It appears that the Governor-in-Chief of the Windward and Leeward West India Islands, Mr. Walker, first suggested the inquiry to the Colonial Secretary, the Duke of Newcastle, in a despatch of the 2nd of February, 1862; which his Grace, in the May following, referred to the Royal College of Physicians; and that the college approving, undertook to carry it out, appointing a Committee for the purpose, which consisted of the following gentlemen: Dr. Budd, Dr. Owen Rees, Dr. A. Farre, Dr. Gull, Dr. Milroy, and Dr. Greenhow.

The first step which the Committee took was to prepare a series of queries of a very comprehensive kind relative to the disease, which was forwarded to the Colonial Office in July of the year above mentioned for circulation under official authority. A copy of it is to be found in the number of our Review for January, 1863. The last returns to these interrogatories, we are informed, were received in the spring of 1865; the replies altogether amounting to between two hundred and fifty and three hundred, the respondents, with few exceptions, being medical men.

On these replies the Report of the Committee is framed. This elaborate document consists of three parts: 1st—An arranged selection of the replies under the successive interrogatories; 2ndly—"In like succession, the conclusions they have formed on the subject matter of each interrogatory from a review of the whole evidence before them," with appended

notes to most of them from the Norwegian official Report on Leprosy in 1847, by MM. Danielsen and Boeck; 3rdly—Closing observations, relating to the several topics which could not conveniently be brought under consideration before, with an appendix comprising contributions on the disease by the Honorary Secretary, Dr. Gavin Milroy, Mr. Nourse, Mr. Erasmus Wilson, and other contributors.

It is not often that the design and the execution of an undertaking are equally deserving of commendation: in this instance, we are of opinion that the Committee are as much entitled to thanks for what they have done as for what they proposed. Their labour, be it remembered, has been altogether gratuitous—their reward solely the consciousness of doing good. The great body of information they have brought together cannot, we think, fail of affording material for a better history of the disease than we are yet in possession of; and the conclusions and the remarks they have made can hardly fail of yielding great assistance towards arriving at a true pathology of the malady, and especially of its etiology, and thereby of its prophylaxis.

To do full justice to the Report and to the documentary evidence it contains would require more space than we can command. All we propose to attempt is to give briefly an account of what appears most interesting and most likely to make our readers acquainted with the most remarkable features of the disease. For the sake of order, we think we cannot do better than follow that by which the Committee have been guided in preparing their interrogatories.

The wide spread of leprosy at the present time is very remarkable; hardly any country forming a portion of the British Empire, now so extensive, is absolutely free from it: indeed, from the sketch given of the geographical distribution of the disease by the Honorary Secretary of the Committee, this remark applies almost to the entire globe, and this irrespective of climate and of position as to proximity to the sea, distance inland, or elevation above the sea-level; we must so far qualify this, however, by stating that it is much more common in some countries than in others, and amongst some races, and probably conditions of life, than others; but why this is so, is not very clear. The evidence contained in the Report on these points is very strong: Madagascar affords one of the most striking examples, as shown in the following quotation from a contribution by Dr. Davidson:—

"In Madagascar there are a number of different races of all shades of colour, from the pure negro to the Howah, whose complexion is not darker than a native of Spain. These occupy widely varying climates. The central provinces, from their great elevation, have a

temperate climate, similar to that of the South of France. The climate of the plains, on the other hand, is tropical, and towards the north excessively warm. The circumstances and modes of life of these races are as varied as their origins and the nature of the localities in which they reside. Yet leprosy affects all alike. The Howah, who lives in European fashion and in a temperate climate, is not less exempt from this scourge than the African slave. It is found amongst the Betasmasarahas, who eat pork, and amongst the Betanmenas, who abhor it. It occurs where fish is an article of food; but it is also to be seen where no fish is to be had, and where rice and vegetables satisfy the simple wants of the population. It exists in town and country, at the elevation of 7000 feet above the sea level, along the coast line, and through all intermediate elevations. Probably, the dirty habits so prevalent in half-civilised nations must tend to aggravate it; eating from a common dish with fingers—the custom very common in Madagascar, of interchanging garments, and of all lying huddled promiscuously together at night, cannot fail to render it more inveterate, even if they do nothing in the way of originating it."

The forms in which the disease appears are generally acknowledged to be chiefly, if not exclusively, two, leprosy of the tuberculated, and of the anæsthetic kind, often mixed and sometimes associated with the elephantoïd enlargement of the lower extremities, sometimes with a "leucopathic" affection of the skin. The complications add not a little to the obscurity of the disease and the difficulty of describing it. Instances of their occurring in an unmixed form are comparatively rare; the tuberculated kind oftener presents itself exempt from admixture with the anæsthetic than the latter with the former. At one station in Rajpootana, that of Ulnur, the anæsthetic kind alone has been observed: it is stated that a case of "lepra tuberculosa" had never there been met with by the reporter.

It appears to be pretty satisfactorily proved that neither of these forms is necessarily connected with yaws or syphilis, though both are not unfrequently associated with, and perhaps aggravated by, the latter. Mr. Erasmus Wilson, in the paper appended to the Report, lays great stress on the analogy which he thinks exists between leprosy and syphilis, at the same time holding that each is a disease *sui generis*. The analogy he dwells on consists in each having three stages—the latent or incubative, the febrile, and the persistent.

From the "notes respecting the leprosy of Scripture"—for which we are indebted to Dr. Gavin Milroy, and which form a part of the appendix—it would appear to have been commonly of the tuberculated kind, and often associated with a leucopathic scaly condition of the skin; and though rendering the

individual affected "unclean," was not considered contagious or hereditary.

We do not think it necessary to enter into any minute descriptive particulars of the phenomena, which in each form of the malady vary extremely, according to conditions more or less difficult of appreciation. We shall select a few extracts affording details best adapted to give an idea of the disease in its aggravated state, briefly premising that the premonitory symptoms are commonly uncertain, as is also the duration of the period of incubation; and, further, when the disease is established, that it advances mostly by fits and starts, each paroxysm accompanied by febrile excitements of no very long duration; after the termination of which, a tolerably normal state of health follows, and lasts until interrupted by a fresh febrile exacerbation.

First, of the tuberculated kind, as witnessed in the Madras Presidency:

"Lepra tuberculata appears insidiously, without any or but ill-defined constitutional symptoms; burning and itching are complained of in the face and extremities, and the skin is often dry, bronze or fawn coloured: raised patches, patches of various shapes and dimensions, soon appear on the face and extremities; sometimes they present a glazed and shining appearance, or the reverse. These elevated patches are often hypersensitive in the first instance, but gradually become insensible, and continue so. In some the *distinct* tubercles are comparatively few; but the face is covered with livid, smooth, shining blotches; the nose, ears, brows, and chin are the parts chiefly affected; in severe cases, the face becomes one nodulated mass, producing a most revolting appearance; the moustaches, whiskers, the hair of the eyebrows and of the eyelids fall off; the nose becomes flattened, its alæ enlarged by tubercles. The staring effected, produced by the eyes in consequence of the want of lashes, and the sallow complexion, have led to the affix *leonine* being applied to this phase of the disease. The tongue and mucous membranes covering the hard palate become studded with tubercles; discharges of pus and blood from the nares are frequent; the voice becomes weak and altered, sometimes entirely lost, and the patient blear-eyed. The skin of the extremities is usually dry, shining, and thin—often fissured on the soles of the feet and on the hands; the ends of the fingers thick and clubbed; the nails horny, and raised by a deposit under them. There is not the same amount of distortion in the hands and feet that occurs in the anæsthetic form of the disease; indeed, it is not uncommon to see perfect hands in the advanced stages of the tubercular form of the disease. Even when stiffening of the joints occurs, there is not the same tendency to their flexion, nor is interstitial absorption of the phalanges so common: their destruction is generally by necrosis, the tubercles breaking down, and forming foul and painful circular sores on the soles of the feet, heels,

and palms, causing the destruction of the smaller joints. In this, the tubercular form, the loss of sensation is not so complete, but the patient is much more disfigured and loathsome in appearance than in the anæsthetic, the suffering is greater, and the disease runs a more rapid course, terminating in chronic diarrhœa or dropsy. The anæsthetic and tubercular forms are often combined in the same individual, constituting a mixed variety. In neither do any definite or well-marked constitutional symptoms precede the local development of the disease, but both are often complicated with other skin diseases, especially scabies, psoriasis, chronic eczema, and venereal eruptions."

Secondly, of the anæsthetic form, as witnessed in the same Presidency, where it is more common than the preceding:

"Anæsthesia of an extremity, or of a portion of an extremity, or of localised spots on the trunk, attended by a slight loss of colour in the anæsthetic part, is usually the first indication of the disease; it occurs without any constitutional symptoms, and so insidiously that its existence is often unsuspected; the spots are usually small, though of varying size, irregular or round in outline, and appear as if the colour had been partially discharged from them; they are usually dry, and present a peculiar glistening appearance, combined at times with wrinkling, or bullæ form over the extremities of the fingers, the toes, or over the back of one or more of the phalangeal joints: these latter soon lose their flexibility, the finger becomes swollen, the vesicle bursts and leaves a round glazed and intractable ulcer, which either heals slowly, to be followed by others of a similar character at longer or shorter intervals of time, or probably it destroys all the soft tendons and exposes dead and carious bone. While these ulcerations are in progress, changes of a destructive nature are also occurring in the bones and articulations; the latter become stiff and peculiarly distended, a process of interstitial absorption is going on in the phalanges, so that in a short time the last bone of the finger is entirely removed, and the altered nail and pulp is to be seen foreshortened on the second phalanx; or if that has also undergone absorption, the soft parts of the two terminal phalanges may be seen on the first phalanx. Although destruction of the bones of the hand by interstitial absorption is perhaps the most common, yet it is often effected by caries and necrosis. When the destruction of bone is considerable, the distorsion of the hand is often very peculiar; the fingers become so much distorted and stiffened as more to resemble the talons of a bird than the human hand; the first phalanges of the fingers are bent backwards, while the second and last are curved into a claw-like shape, in which position they become stiffened, and the whole hand becomes withered, wasted, and insensible. In like manner, the toes are absorbed and foreshortened, so that a toeless foot remains, accompanied by the destruction of one or more of the metacarpal bones. Ulcers often form on the soles, and corrode deeply towards the metacarpal or carpal bones; the edges of these

ulcers are hard, callous, and insensible, and appear as if they were cut out by a punch or some such instrument. These ulcers sooner or later communicate with dead or dying bone, owing to the hands and feet being generally anæsthetic. The commencement of the destructive processes just described may be mechanically caused by fire or abrasions; but, irrespective of these, vesicles often form on the insensible surfaces. The anæsthesia usually, in a few months, extends up to the knees and elbows, but often much further, cases occurring where it is so complete that in the tongue alone sensibility remained. Mutilation seldom extends beyond the fingers and toes. Epistaxis occasionally occurs, but more frequently there is a fœtid discharge from one or both nostrils, followed by destruction of the bones of the nose and palate, from which the voice becomes altered and hearing affected. The cornea often becomes hazy and ulcerated, the lower lid everted, the conjunctiva thickened, and consequent loss of vision. In this condition, more or less maimed and helpless, the leper drags out a weary existence, protracted over decades of years, till at last he succumbs under an intercurrent attack of diarrhœa, dysentery, dropsy, or bronchitis."

The development of the disease does not appear to be confined to any period of life. It has been known to occur in early infancy, but more frequently by far about the time of puberty up to maturity, rarely at an advanced age. Dr. Nicholson, writing from Antigua, says he has seen it manifest itself in children of five years, and also for the first time at the age of fifty. When it attacks early, he adds, there is sometimes a complete arrest of development; he makes mention of a youth in whom growth was thus stopped,—a fine youth, in whom the disease appeared at eight years, and who, though he lived to twenty-four, did not increase in stature, nor were the genital organs developed.

The frequency of the disease seems to be about the same in the two sexes; in some places the male, in others the female sex, is stated to be most subject to it.[1]

Though not confined to any race, it is unquestionably most

[1] The disease in its mixed form is well described in that interesting work, 'Narrative of an Expedition to the Zambesi,' by David and Charles Livingstone, when speaking of a chief who was a sufferer from it, and who attributed it to witchcraft. By the natives the disease is believed to be hereditary and not contagious. Yet the authors seem to hesitate in adopting this conclusion, from the remark made that "something like it was transplanted to the hands of Drs. Kirk and Livingstone, and was cured only by the liberal use of caustic." As the description of the malady is brief, we transcribe it. "The disease begins with slight discoloration of the surface and it first affects only the cuticle, the patches spreading in the manner and with somewhat of the appearance of lichens, as if it were a fungus; small vesicles rise at the outer edges of the patches, and a discharge from the vesicles forms scabs. The true skin next thickens and rises in nodules on the forehead, nose, and ears; and when the disease is far advanced, foul fissures appear on the toes and fingers. These eventually drop off, and sometimes the deformed patient recovers."

common amongst the coloured races, and of these amongst the poorest and those of lowest condition; but the poor, it should be kept in mind, in every country, more especially the countries where leprosy is common, constitute by far the majority of the population. Besides the exceptional instance afforded by Madagascar already quoted, we meet with others in the Report: thus, at Jhansi, in the north-west of India, it is stated that "the higher castes of natives, the Brahmins and Pundits, appear to be less subject to both forms of disease than the lower."

It is added—

"These classes or castes of natives seldom have any employment, except taking care of the numerous tombs and other places of religious worship; they are for the most part situated on the banks of the numerous tanks of stagnant water in and around the city. Their habits are extremely clean; their ordinary diet is confined to farinaceous food; their dwellings are, in general, more cleanly than those of other classes of natives."

In the same page it is stated that several rajahs and nawabs are known to be suffering from the malady.

There is a general persuasion that the disease is hereditary. In the Punjaub so strong is this belief amongst the natives, and so low their humanity, that they were, we are told, "in the habit" (since checked by Government interference) "of burying alive not only the leper himself, but also his relatives and friends, lest in multiplying their kind the disease should be communicated to distant generations." In some instances, a shortening of life was practised in India either by drowning or burying alive, with the assent of the individuals, from another motive—a belief of the incurability of the disease. Of the latter mode of suicide an instance is given, described as follows:

"About 1819, a woman of the cultivator caste, about forty-five years of age, in holiday attire, and with a garland of flowers hanging from her neck, was conducted out of the city [Nagpore], with music, to a spot near the bridge on the Paldi road. There she sat down till her relatives dug a pit six feet deep, with a recess on one side. The work finished, she blessed the spectators, and descending into the pit she took her seat in the recess, which was then closed with a bamboo mat; the earth was then filled in; the people on the surface gave a loud shout, which was said to be answered by a feeble cry from below, and the crowd dispersed."

It is the belief of some, that though hereditary, the disease wears itself out in two or three generations. That it commences *de novo* in many instances, appears to be a well-established fact, and often under conditions without any obvious predisposing or exciting cause. Thus from Loodiana, in the Punjaub, a reporter states—

"In the course of my inquiries I have not been able to come to any conclusion as to the predisponent or exciting causes of the disease, which, as we have seen here, singles out one member of a family, all of whom are living under precisely the same circumstances in every respect, occupying the same dwelling, subsisting upon the same kind of food, following the same employment, and, it may be presumed, all of somewhat similar constitutions. As a rule, one only is affected with the disease; the rest escape."

It is added—

"In almost all cases I have met with, the disease appeared to have had a spontaneous or accidental origin; neither the patients nor their friends are aware of any circumstance which would show a predisposition to the disease."

Though there is some difference of opinion regarding its infectious or contagious nature, the facts recorded appear to be strongly in favour of its not being communicable in any way from person to person, not even by sexual intercourse, and it is more than doubtful that it is communicable even by inoculation. Three instances are recorded of individuals wounded in the examination of bodies of lepers without any bad consequence, one only in which the disease was presumed to have owed its origin to the cause in question: this was a case of a European in Bengal, "who had become leprous from having been cut with a razor that had been used by a leper." The few who advocate its contagious character restrict it to the open ulcerative stage; and yet the instances on record are innumerable, of persons living amongst lepers, cohabiting with them, dressing their sores, and washing their foul linen, escaping the malady.

Of those who are disposed to believe that the disease may be contagious, though he would not absolutely assert that it is, is Dr. Tilbury Fox, who supposes it may be communicated "by long contact" and the medium of the secretions. This view he brought forward at a recent meeting of the Epidemiological Society, where the subject was freely discussed by the members. The most forcible argument he used was that resting on the outbreak of leprosy in Honolulu of the Sandwich Islands. He also argued in favour of it from the analogy between leprosy and pellagra, to which, however, we are not disposed to attach much weight, inasmuch as the contagiousness of the latter is as questionable as that of the former.[1]

[1] At the recent meeting of the Epidemiological Society alluded to, Dr. Tilbury Fox introduced the subject of leprosy, considering mainly (1) its cause, and (2) the influence of hereditary tendency and contagion (by long contact) in spreading or perpetuating the disease; urging that before we could *absolutely* deny the contagiousness of leprosy, we must explain away certain remarkable facts with regard to the production of the disease in clean persons and communities. As before

Considering the opposed facts as to the causes which have been assigned in answer to the interrogatories of the Committee relative to the origin of the disease, it seems hardly necessary to enter into details, showing, in a manner, the mystery that there is as to its etiology: it may be mentioned, that it is considered by the superstitious in India as a punishment for sin committed in a former stage of existence, especially for the crime of incest. The only very general conclusion founded on a wide induction that seems to be warranted is, that bad food, and perhaps some particular kinds of food, such as putrid fish, putrid meat, mildewed grain, filthy habits—in brief, whatever predisposes to an unsound state of health—may favour its production, and, *vice versâ*, a conclusion which seems tolerably to accord with what we learn from history, especially that of Europe, that as civilisation has advanced, as the sanitary state of a country has improved, the disease has been checked, if not altogether put an end to: at the same time, if adopting this conclusion as probable, we for our part cannot forget the exceptional cases in which this disease has shown itself under circumstances the most unfavorable, apparently, for its production.

Of the treatment of the disease and its cure, there is little that is satisfactorily known. No specific, unfortunately, has yet been found for it. The medicines which have seemed most useful in mitigating the symptoms have been chiefly preparations of arsenic and iodine, with attention to sanitary conditions and regard to the general health. In the anæsthetic form, counter-irritation has had a trial, with apparently considerable success. Mr. Kendal, writing from Midnapore, Bengal, states—

"In the cases mentioned by me, I have always allowed a liberal diet; and the remedies that I have found to be most useful are cod-liver oil, liquor arsenicalis, and chaooh-moynee oil. These are the only three remedies I place much confidence in, although I have used several others, but without much apparent benefit. . . . In lepra

said, he considered at large the circumstances of the case of Honolulu, where leprosy is stated to have arisen and increased rapidly during the last thirty years without being explicable by *hereditary transmission*, by *immigration*, or peculiarity of climate or diet, or the habits of the people, who have improved in every way socially and morally. In the discussion, Dr. Smart, R.N., who had seen the disease in Crete in 1853, and was adverse to the doctrine of contagion, recognised the influence of hereditary tendency and intermarriage. Dr. Nicholson, who practised in Antigua for forty years, gave some account of it as seen there; and Dr. Gavin Milroy, as showing that cases of the disease are not so very rare in this country as most medical men imagine, stated that within the last four years he had seen four distinct cases in Metropolitan hospitals—three in Guy's Hospital, and one in University College Hospital. He suspects that traces or vestiges of leprous diathesis are not very infrequent among us in the United Kingdom.

anæsthetica I have found counter-irritation along the course of the spine most useful. I usually apply it after the native fashion, viz., by application of a heated iron, and the sores resulting I either keep open for some time, or else renew them in an adjacent spot; and under this plan of treatment, combined with one or more of the remedies above mentioned, sensation very soon becomes restored, and the patient is comparatively cured. But I should hesitate to say that I have ever seen a perfect cure, as I believe the disease is very liable to recur."

In some instances, a very few, improvement has been experienced from a change of climate; and, in a small number of cases, a kind of spontaneous cure has taken place after the loss of some of the phalanges, in persons labouring under the anæsthetic form of the disease.

The morbid anatomy of the disease has hardly yet received the attention which it deserves. As a very important part of the inquiry, and as most likely to aid in throwing light on its nature, we shall quote *in extenso* the few post-mortem examinations given in the Report:

"Dr. Mongeri describes briefly an incomplete dissection he made of a leprous case at Canea, one of the principal towns in Crete. The patient was upwards of fifty years of age, had been in the lazaretto for thirty years, and had lost all his fingers and toes. The body was extremely emaciated, with the exception of the head, the scalp and face being enormously swollen from tuberculous enlargement. The integuments of the body were hard, coriaceous, and covered with brown prominent scales. When these were detached, numerous tubercular elevations, not visible during life, were made apparent. The larynx internally was twice its natural size; the *rima glottidis* was occupied with a mass of tubercles of various size; the mucous membrane of the larynx, trachea, and the bronchi was extremely pale. There was much bloody serum in the thoracic cavity; the right ribs were carious; those of the left side were not affected. The lungs were profoundly diseased. The stomach and intestines were very pale, and numerous tubercles were found in their tissues. The omentum, mesentery, and the abdominal parietes were so loaded with these deposits as to resemble the *ladrerie* in swine, a very common disease in Crete."

The next is of a Chinese who died in Australia, reported by Mr. Hutchinson, resident surgeon at Castlemaine Hospital:

"Body extremely emaciated; skin of a tawny colour, dry and corrugated, something like a dried fish's skin; nose flattened from absorption of cartilage; small abscess round the larynx; when the skin was cut into, matter welled out. Epiglottis and internal parts of larynx thickened. Mucous membrane denuded for some distance down the trachea. Aperture at the top nearly occluded. Heart empty; arterial system seemed healthy; lungs natural, with the

exception of a tumified deposit in upper part of each lung of a melanotic or tuberculous character. Liver healthy-looking, and about natural in size. Gall-bladder completely filled with gall-stones, 151 in number, smooth and polished, varying from a very small pea to a bean in size. Other organs normal. Brain not emaciated."

The next is of a man æt. 43 at the time of his death, who had laboured under the tuberculated form of the disease in his native country, Ceylon, fourteen years. The autopsy was made by Dr. John Davy, in 1816, and described as follows:

"The surface of the body was fissured and excoriated in a hundred different places. The left foot was in a state of gangrene. The heart was rather small and flaccid, and its parietes were thin; a thick layer of fat covered its outer surface. The liver was large, pale, and marked with white spots. The gall-bladder was distended, with greenish bile. Much fat was accumulated about the mesentery. A few red spots appeared on the mucous membrane of the intestine. A section of the slightly enlarged glands of the groin exhibited no decidedly marked diseased structure. The tuberculated parts of the skin were thickened, and each tubercle seemed to be produced chiefly by a thickening of the cutis. The integuments of the lower extremities, and especially of the knees, legs, and feet, were generally thickened. In most places the true skin was not less than a quarter of an inch thick. Under the thickened layers, a layer of fat presented itself, which was also diffused through the cellular membrane between the muscles. Most of the muscles of the leg seemed to be converted into adipose matter, so that very little muscular fibre remained. At both the knee-joints, the capsular membranes and bursæ were distended with an oily or fatty matter, which was yellow, semi-fluid and granular, and in appearance very like honey. No serous effusion was found in any part of the body."

Next, there is a brief notice of five post-mortem examinations, made at Mozufferpore, Bombay, by Mr. N. C. Macnamara, Civil Surgeon, who states that his attention was more particularly directed to the nervous system. He says—"Neither in the nerves themselves, nor in the brain and spinal chord, have I been able to detect any lesion either with the naked eye or by the aid of the microscope." The form of the disease in either of these five cases is not mentioned; it may be presumed to have been of the tuberculated kind.

In another short notice of some post-mortem examinations made by the Proto-Medico of Corfu, it is stated "that the tissues generally were attacked, and principally the venous system, more particularly in elephantiasis. In one case, where the death resulted from pneumonia, the cranial, femoral, and iliac veins exhibited knobby appearances, and, on being opened,

the deposit of a caseous substance resembling tubercular matter."

We may refer to Dr. Carter's paper, in the January number of our Review for 1863, for the interesting results of his microscopical researches, especially those relating to the diseased state of the nerves, so explanatory of some of the more remarkable symptoms of the anæsthetic form, and so in accordance with the observations of the Norwegian inquirers; and we would draw attention to the fact that, in fourteen instances in which he made a post-mortem examination, no specific morbid lesion was found in the following organs: the heart, lungs, intestinal canal, liver, spleen, pancreas, supra-renal capsules, brain, spinal chord, with their membranes, with the exception of slight congestion or adhesions of the lungs and dysenteric ulceration occasionally in the colon.

As bearing on the pathology of the disease, we shall give two or three additional extracts. Dr. Coates, writing from Chumparum, Bengal, thus describes the advanced stage of the disease when the bones become affected:

"The real commencement is in the periosteum, surrounding the shafts of the phalanges of fingers and toes. The periosteum becomes detached from the bone, and while doing so the tissues between it and the skin become infiltrated with exuded lymph, at first soft but ultimately hard and firm; and if more is thrown out at one side of the finger than at the other (according as the periosteum is affected at one part in preference to another), on the lymph hardening the finger is bent to the opposite side from the joint next to it, and firmly fixed in the new position. All this occurs without pain, and thus ends the attack for that time. Next time, the serum that is exuded between the periosteum and the bone, together with the surface of the bone that has exfoliated, find their way to the surface; the skin over the part being thick, becomes elevated into a sort of bulla, which at last bursts and an abscess is formed. The pus is of an extremely pale colour, and is accompanied by much serum, thin and clear; it has a peculiar and indescribable odour. The centre of the ulcer is of a pale, glossy colour, and the edges bright red. Attack after attack of this nature exactly succeed each other, and bone after bone becomes engaged; the ends soften, and shafts exfoliate; and while in this condition, if the sufferer knocks accidentally his finger or toe against any obstacle, the part breaks off. Some blood and grainy serum come away, and then the part dries up, leaving a thick scab over it until perfectly healed. In this manner joint after joint is lost. But if the affected finger is not broken off, the finger shortens at each attack from the entire shaft exfoliating, and the ends becoming absorbed, until the finger or toe-nail, or what may remain of it, is found as far back as the base of the second phalanx. Before the process has proceeded thus far, the tendons often take on

that softening and suppuration, which proceeds along up their palm and sole up to the forearm and leg as far as their muscular origin; and this suppuration then extends to the intermuscular cellular tissue, and diffuse suppuration amongst the muscles of the forearm and leg is the result. By this time, the radius, ulna, and tibia and fibula become engaged; softening of the ends and exfoliation of the shafts go on, but only for a very short time; for the extensive suppuration has so far exhausted the unhappy sufferer, that death from exhaustion rapidly supervenes, hectic accompanying of course."

Dr. Brodrick, writing from Indore, Central India, in the account he gives of the deformities produced by the disease, specially notices the absorption of muscle: thus, after describing the change which takes place in the hand, the claw-like form of the fingers from incurvation, he states—

"At the same time, the plump mass of muscle between the thumb and the forefinger (the abductor pollicis and abductor indicis muscles) become absorbed, and this muscular atrophy is followed by that of the 'interossic' and other muscles, until the member loses all plumpness and shapeliness, and becomes a very nightmare of a hand."

Dr. Broderick refers to a case, published by Mr. J. Hutchinson in the 'Medical Times and Gazette,' February 14th, 1863, in which changes of the hands with wasting of muscle, analogous to the preceding, followed the section of the ulnar nerve and a wound of the median nerve.

In a communication from Assistant-Surgeon A. C. Nisbet, from Akyal, British Burmah, is the following relating to the state of the blood-vessels:

"I may mention a case which I operated upon, and in which, on removing the affected part, the left lower extremity at the line of junction of the lower with the middle third of the tibia, no arterial hæmorrhage followed, and the stump healed kindly and rapidly without the application of a ligature. The arterial trunks divided anteriorly and posteriorly were, as blood-distributing agents of nutrition, to all intents and purposes obliterated; and the supply of nourishment having been cut off in this way, nature had removed toe after toe, and was engaged in removing the foot at the ankle-joint, when I assisted her with the knife by removing the useless and troublesome member a little higher up."

Dr. Finnimore, in his account of the tuberculated form of the disease at the Mauritius, remarks that amputations, of which he has performed many on lepers, "heal with a rapidity rarely met with in healthy persons;" adding, "perversion and loss of cutaneous sensibility are frequent in the course of the disease. Cutaneous secretion is always much diminished; fre-

quently almost entirely arrested. There is always more or less emaciation."

According to Dr. Browne's experience in Barbadoes, the loss of parts in the anæsthetic form of the disease may take place without ulceration, as described in the following passage:

"The disease commences with white spots on the skin of the body, hips, and arms, subsequently loss of feeling in the extremities, followed by gradual contraction of the flexor tendons, and afterwards by loss of the phalanges of the fingers and toes, and occasionally of the entire hands and of the greater portion of the feet by absorption without ulceration, the nails and toes being often found on the knuckles or remaining stumps."

This, we think, requires confirmation.

For what is known of the condition of the blood in leprosy, we are most indebted to Drs. Danielsen and Boeck. According to them, "the most marked abnormal change from the standard of health appears to consist in the excessive quantity it contains of albumen and fibrine;" these, they remark, are the principal elements in "the morbid effusion with which all the pathological alterations characteristic of the disease are connected." It is stated, they have found the same "sanguineous dyscrasia" in both forms of the malady. The only observation regarding this fluid we can find in Dr. Carter's paper is, "the blood loosely coagulated:" and this, we infer, found post-mortem. By Dr. Bowerbank it is stated that Dr. Scheida, from Bavaria, when engaged in researches in Jamaica on infusoria, "examined the blood of several patients suffering from both forms of elephantiasis, and found it deficient in red corpuscles."

We are not aware of any observations yet published on the temperature of the body in this disease. Having had an opportunity of consulting the notes of Dr. John Davy whilst he had the superintendence of the Leper Hospital in Ceylon, in 1816, we find that in most instances it rather exceeded the normal degree, ranging, as indicated by a thermometer under the tongue, from 101° to 103°; and this when there was no febrile paroxysm, and the skin generally was of natural coolness.

The only author we are acquainted with who has recently speculated on the etiology of the disease is Mr. Erasmus Wilson. In the paper already referred to, which abounds in ingenious remarks on the analogy between leprosy and syphilis, he suggests as probable that the former may be owing to a malaria, productive of a diathesis affecting the blood transmissible by generation; and on this hypothesis, and a review of the symptoms, he founds the treatment which he proposes.

Considering the imperfection of our knowledge of the disease,

it would be premature to criticise this view with any minuteness. The facts best ascertained seem to us hardly to support in anywise the humoral pathology of the malady: the lesions are so long local, the principal organs, especially the lymphatic glands so little, if at all affected, the intervals of health so considerable, and further, the rapidity of healing of parts after amputation or loss by ulceration so noticeable.

We have stated enough to give those of our readers who have never witnessed the disease an idea of the horrors to which the unfortunate subjects of it are exposed, and yet we have done this only partially; the topic is truly revolting, especially as regards the inhumanity so often exhibited. We wish we could qualify it by redeeming circumstances; but we look in vain for them, except in a few instances, and those almost exclusively in India and in a few of our colonies, where some inadequate provision has been made for the reception of the wretched patients, but hardly for that treatment their cases require, and such as they would have in a well-regulated hospital.

The cruelties which have been perpetrated on those labouring under or suspected of having this terrible disease afford, we may remark, a striking example of the evils resulting from error—the erroneous belief usually entertained that leprosy is contagious. The alarm thus created has too frequently mastered all regard to humanity. And do we not witness the same, though not in the same degree, even in civilised Europe, and in our own country—wherever there is a panic about infection, whether from cholera or yellow fever, leading to the enforcement of quarantine enactments always more or less harassing to individuals and injurious to commerce, and not always, if ever, affording security against the invasion of the diseases they are designed to ward off.

One subject of inquiry, and not the least interesting of those proposed by the Committee, viz., the hygienic condition of our colonies, has been but feebly responded to, and this chiefly incidentally. From such information as is thus afforded, it does not seem strange that leprosy should be found so widely scattered, if filthy habits of all kinds and all kinds of sanitary neglect conduce to its production and propagation. On the contrary, it seems surprising that it is not more prevalent, and that it should comparatively be so rarely met with in European countries, not excepting our own, in which the hygienic state of the poor is admitted for the most part to be of the lowest kind.

We must not conclude without expressing the hope that our Government will circulate widely the valuable 'Report on Leprosy' of the Royal College of Physicians, and that it may lead eventually to a more careful and extended inquiry into the

nature of the disease, and to a more humane treatment of those labouring under it. Knowledge has been called power: may it not with as much propriety be called humanity?

Review VII.

St. George's Hospital Reports, 1866. Vol. I, edited by John W. Ogle, M.D., F.R.C.P., and T. Holmes, F.R.C.S. London, pp. 444.

We gladly welcome this first volume of Reports published by the medical officers and former pupils of St. George's Hospital. Our readers will, we think, be best served by a brief analysis of its contents, such as we are wont to give of other hospital Reports.

The first paper is from the pen of Dr. Page, the senior physician. It is an interesting and pleasantly written sketch of the origin and progress of the hospital and its medical school. Dr. Page urges the necessity of instituting a better system of nursing, which is at present carried on by paid nurses. The heads of each ward are good and efficient, but the under nurses are not of so high a class as is desirable. He also points out the desirableness of the erection of a college with lodgings for the students, and a common hall for them and the officers of the hospital. Many important facts, which our space will not allow us to dilate upon, regarding the honoured names associated with the hospital will be found in this communication.

Mr. Prescott Hewett contributes a paper "On the Deviations of the Base of the Skull in Chronic Hydrocephalus." When the orbital plates are driven downwards, the dropsy is ventricular; when they retain their natural position, the dropsy, if ventricular, must have taken place after the bones could be acted upon, or it is in the cavity of the arachnoïd, and limited to the upper and lateral parts of the surface of the brain. We need hardly remind our readers that this theory of external hydrocephalus is denied by many, as by Watson,[1] who believes all the cranial dropsies to be ventricular. Mr. Hewett's observations, however, as to the effects produced on the bones of the base of the skull are of great interest. He says, we recognise the deviation in the orbital plates by the state of the eyeballs, which have a marked direction downwards. A great part of

[1] Watson, 'Pract. of Med.,' vol. i, p. 461.

the pupil is hidden beneath the lower lid, and the white of the eye is more uncovered than usual. The middle fossæ of the skull are sometimes displaced, as in the two examples 3489 A, 3487, in the museum of the Royal College of Surgeons, and especially in a fœtal head presented by Mr. Lomax, where on each side a large pouch bulges outwards and projects forwards into the region of the cheek as if this was blown out. The upper and back part of each pouch is formed by a saccular expansion of the bones; the lower and front part, by the temporal and the great wing of the sphenoid, separated from their articulations above and driven downwards—the gap in the centre of the pouch being filled up with membrane.

A similar case is represented in a plate by Vrolik, and also by Creutzwiezer, although he has wrongly explained it as external hydrocephalus; while Steinmetz's case (Gräfe und Walther, 'Jour. für Chir.,' Bd. 19, p. 119) is just parallel with that of Lomax. The fluid dilates the lateral ventricles, and so can be squeezed from the pouch in the cheek up to the vault of the skull. It may happen that the fluid is limited to the inferior horn of the lateral ventricle.

Mr. Holmes details a case of meningocele in a child five months old. Twice, at an interval of twenty days, the tumour was tapped and injected with iodine, but the child died of broncho-pneumonia. The protrusion was found to be through the occipital foramen; the arch of the atlas was incomplete, and the neck of the tumour was partly formed by the membrane occupying the place of this bone. On dissecting the tumour, it was found divided into loculi by septa; a bristle passed freely, through a single communication, into the fourth ventricle. The lateral ventricles were distended with clear fluid. Mr. Holmes mentions a case operated on in a similar manner by Mr. Paget, and a second by himself, where, though no cure was effected, the growth of the tumour was checked, and the iodine did not cause any dangerous symptoms.

Dr. Reginald Thompson contributes a carefully written paper on the typhus epidemic of 1864-65 as observed at St. George's Hospital. He gives evidence as to the greater fatality during the later period of life than the early, as to the evil effects of overcrowding, as to the admission of fever cases into the general wards of a hospital. He describes the course taken by the fever, the precurrent symptoms, the eruption, the state of the urine and stools; the delirium, cough, and pulse; the crisis accompanied by sweating, the thermometrical indications, and the use and abuse of alcohol.

Dr. Allbutt gives some notes on an epidemic of typhus at Leeds, chiefly having reference to the use of brandy, Graves'

opium and antimony mixture, camphor, and the advantage of open-air wards.

Mr. Lockhart Clarke's paper is a monograph "On Progressive Locomotor Ataxy," giving the diagnosis, pathology, and treatment of Duchenne's disease. The essential symptom is a peculiar unsteadiness in the performance of certain voluntary movements, arising from the loss of voluntary influence to control, combine, or co-ordinate the action of the muscles necessary for the steady performance of those movements. It usually begins in the lower extremities, and exhibits two forms: the loss of co-ordinating power, so that the patient cannot keep his equilibrium; the additional loss of the power to regulate the degree of muscular contractions, so that the limb is flexed or extended with a sudden uncontrollable jerk. The ataxy is always accompanied by a variable number of other nervous affections, as pains, anæsthesia, analgesia, paralysis of one or more of the cerebral nerves, derangement of the genital or urinary organs. The pains are sometimes dull, sometimes acute. There is frequently a sense of constriction around the waist and back. Anæsthesia is usually progressive; sometimes there is hyperæsthesia. Analgesia is exhibited as a slowness of perception; paralysis of the third or sixth cranial nerve occurs in more than one half the cases; the disorders of the urinary organs often intermit in their attacks.

The author gives seven cases in detail as examples of the different groups of symptoms and affections. Early diagnosis is very important. Three cases serve for examples of the differential points between ataxy pure, and when mixed with cerebral disease; and a fourth case, in which muscular atrophy might have been mistaken for ataxy. Syphilis and rheumatism also sometimes produce symptoms resembling the spinal disease. The exciting cause may be anything that depresses the nervous power. *Treatment* should be constitutional; oxides of silver seem to exert the most specific effect. *Pathological anatomy*:— The posterior columns, including the posterior nerve-roots, are the parts of the cord chiefly altered in structure. The nerve-fibres are atrophied or disintegrated.

Mr. Rouse's paper on rheumatic iritis is a brief summary of the symptoms, stages, diagnosis, sequelæ, and treatment of this disease. He urges the importance of mercury to prevent the organisation of the exuded lymph, and the use of atropine in the first and last stages. Recurrent iritis is caused by the existence of posterior synechia, and the only remedy is iridectomy. Five exemplar cases are given in detail.

Mr. James Toynbee communicates a paper on cerebral symptoms produced by morbid excitement of the contents of

the labyrinth of the internal ear. Pressure on the membrana tympani is communicated to the contents of the vestibule, producing giddiness, loss of vision, numbness, depression of spirits. The exciting cause may be a foreign body in the outer ear, closure of the Eustachian tube, sudden heat or cold conducted by the chain of tympanic bones. Five cases are detailed of morbid symptoms produced by cerumen, by an artificial drum, by a polypus, by closure of the Eustachian tube, by syringing with cold water.

A paper, by Mr. Warrington Haward, on strangulated hernia, exhibits the treatment adopted at St. George's Hospital. This is, briefly, the use of ice, especially in old scrotal hernia: if this is ineffective, chloroform and a very guarded attempt at reduction by taxis; if this does not succeed, operation without further delay.

The question of opening the sac or not is well discussed, the latter being the practice at St. George's. The grounds are, that the condition of the intestine is accurately determined, any constriction is detected, the fluid in the sac escapes externally, there is less danger of wounding the gut, and the operation is less fatal. The after treatment consists in perfect rest for the intestine, secured by full doses of opium. It is suggested that the radical cure for hernia is advisable for labouring men, and that Mr. Wood's mode is superior to that of Professor Wützer.

Mr. T. Holmes discusses the question of amputation at the hip-joint, firstly, for recurrent fibro-elastic tumour, and, secondly, for morbus coxarius. The mortality of the operation must not, he says, be judged of by the statistics usually put forward, as these are drawn up without due regard to other and co-existing causes of death. The chief danger is from hæmorrhage, and Lister's aortic tourniquet naturally lessens this risk. The doctrine of the constitutional origin of malignant and semi-malignant diseases is being questioned with some forcible arguments by Mr. Moore and others, who contend that the disease commences as a local affection, and is thence diffused through the body;[1] it is therefore eradicable if the primary cancer can be wholly removed.

Mr. Holmes details a case of recurrent fibroïd tumour of the thigh in a woman, which was apparently wholly extirpated by Mr. Tatum, but reappeared in about two months. An attempt to remove it a second time was unsuccessful, and the limb was amputated at the hip-joint. The patient recovered sufficiently to leave the hospital, and the stump healed soundly. Six months later, constitutional symptoms appeared, and a tumour

[1] See Article No. 1 of this number of the 'Review.'

was found in contact with the liver, and, later, another between the shoulders. The patient died. The post-mortem examination showed that the stump was healthy. Tumours were found in various parts of the body, which were determined to be fibroplastic, and not cancerous.

Amputation for extensive (so-called) strumous disease of the hip-joint is justifiable, because the disease when confined to this locality can be wholly removed. In children, amputation has the advantage over excision, that the state of the acetabulum can be exactly known and dealt with accordingly, and the aorta can be easily controlled by the thumb and finger.

A case of amputation by Mr. H. Lee is detailed, where the head of the femur was quite gone, and on gouging the acetabulum, which was much softened, an abscess in the pelvis was opened. The boy, æt. 14, recovered.

Another case is detailed, where Mr. Holmes first performed excision, and osteo-myelitis of the femur following, amputation was done. The child died of strumous abscess in the brain, the stump having healed.

In a third case, one of acute osteo-myelitis, the stump was doing well, when pyæmic deposit in the lungs proved fatal.

Dr. John W. Ogle, in a paper on disease of the brain as a result of diabetes mellitus, first refers to those complications which systematic writers mention, and observes that organic disease of the cerebro-spinal system is rarely dwelt upon either by them, or by later writers, as Bence Jones, Harley, Pavy. He then details a case, the close watching of which compelled him to the conclusion that there are cases in which brain lesion may follow in the train of diabetes, and grow out of it, being in no wise antecedent to or the cause of it. Dr. Ogle examines the case critically as to its general features, and then the points specially bearing on the question discussed. Diabetes was well marked for eighteen months or more; then the sight, which was at first defective, recovered itself, and under a starchless diet and tonic treatment the sugar disappeared from the urine, when, suddenly, thickness of speech came on, gradually followed by facial paralysis and left hemiplegia, the right side being also partially affected later. The cerebral disease increased, and terminated in coma and death. Post-mortem appearances:—The brain was wet; the anterior portion of the middle lobe on the right side was softened, and the arteries plugged with firmish fibrine. The corresponding portion of the left side of the brain was also affected in a similar but slighter degree.

Dr. Ogle considers that the softening of the brain was primary, and the occlusion of the vessels secondary, the stagna-

tion of the blood favouring the deposit of fibrine, as we find is the case in lung-tissue. The dilapidation of the brain originated in the diabetes. After detailing a case related by Mr. Foster, where death occurred with apoplectic symptoms, he suggests that the death was due to degeneration of the brain-tissue, and was not merely the result of a sudden yielding of a blood-vessel. Three modes may be conjectured by which the diabetic state tends to bring about such changes in the brain:—the lesions ordinarily termed complications may arise from the blood being poisoned by the sugar formed by some morbid action of the liver or other organ directly engaged in its formation; or, secondly, the sugar naturally formed in or taken into the stomach does not undergo the changes necessary for the organs dependent on starch-food, and so errs in its destruction; or, thirdly, the sugar may be the *débris* of the constituent parts of the frame, which, by some perversion of assimilation, have broken down. From this last point of view, all complications may be looked upon as correlative with the glycosuria, and the common result of a general defective secondary assimilation. Dr. Ogle draws attention to Dr. M'Donnell's conclusions as to the functions of the liver, and the formation of amyloïd substance or animal dextrine.

The paper concludes with a summarised detail of fifteen fatal cases.

Dr. Bence Jones contributes a paper on "Jaundice and Biliousness." Mechanical obstruction of the duct by a gall-stone causes diffusion of the bile acids into the blood: these possess highly energetic chemical properties, and have a paralysing action on the striped muscular fibre of the heart and its ganglionic nerves, and also on the nervous centres; they destroy the blood-globules, and so may induce fatty degeneration and convulsions.

The mild state of non-mechanical jaundice is called biliousness. When the gall-bladder is full, the bile diffuses itself through the mucous membrane into the blood-vessels; and the longer the distension continues, the greater is the amount of bile that passes into the blood. This is the case when food has not been taken for many hours, or when opium stops the action of the duodenum, or there is catarrhal inflammation of this viscus. The gall-bladder is emptied by the passage of solid food, or the action of irritating medicines or emetics. In ordinary health the diffused bile acids are oxidized; but if there is either suboxidation or an excess of bile, biliousness or jaundice results. This happens in some cases of pneumonia, and thus hot close air produces bilious headache, which fresh cold air removes. The excessive secretion of bile may probably be

caused by tetanization of the sympathetic nerve in the liver, as in fright. Jaundice from suppression of the secretion "almost certainly cannot occur, though the substances from which the bile would have been formed if the liver had continued to act might, without doubt, give rise to symptoms of poisoning, just as the substances out of which uric acid and urea are formed in the kidney become causes of death in cases of suppression of urine or extirpation of the kidneys: but these parent substances have yet to be formed, and the symptoms they produce have to be accurately determined."

The treatment in mechanical jaundice consists in stopping the pain; in catarrhal jaundice, in relieving the inflammation of the duodenum. In chemical and nervous jaundice, the gall-bladder should be frequently emptied, and oxidation promoted.

Dr. Fussell calls attention to puerperal paralysis; he quotes one case which he thinks was caused by direct uterine pressure, another which he refers to "central exhaustion of the cord," and reckons among causes of childbed paralysis, the altered condition of puerperal blood, albuminuria, embolism. The prognosis is usually favorable.

Mr. C. Hunter, in a paper on "the Modus Operandi of Hypodermic Injections," maintains the opinion he held in 1858, that "by injection into the cellular tissue of a part distant from that affected with the neuralgia, the relief that followed was quite as great as when they were performed locally;" he examines with a negative conclusion the writings of those who oppose this view, as Bahier, Rappaner, with other Americans. He urges that the drug injected into the cellular tissue is not subject to the changes which, like food, it might undergo in the stomach, and that the local action is secondary to the general effect, because the minute quantity injected is immediately dispersed. Neuralgia is, as a rule, a reflected and symptomatic affection, not a local disease, and is due more frequently—1, to a nervous diathesis acted upon by malaria, by cold or damp, or shock; 2, to a morbid state of the secretions; or, 3, to some exciting cause not at the part affected by the pain, which is induced by reflex irritation. There is proof, he continues, that the circulation is affected before the pain is relieved; there is no proof that more effect is produced upon the injected part than upon the more distant parts; there is proof that distant injections will cure neuralgia, cause sleep, and give power to the muscles of the one arm, although injected into the other. Localisation is, therefore, not necessary.

Mr. Brodhurst communicates two papers—one on congenital dislocation of the femur, the other on talipes varus.

The dislocation is upwards and outwards, and is usually double; it is produced at birth through downward force applied to the thigh in endeavouring to hasten the birth in breech presentations. The deformity is so much diminished in the recumbent posture, that it is seldom perceived until the child begins to walk alone. If recognised at birth, the dislocation is reducible by gently extending and flexing the limb. Later, the lip of the cotyloid cavity becomes absorbed, and it is difficult to fix the head of the femur. For this reason, Mr. Brodhurst performed subcutaneous section of all the muscles inserted about and into the trochanter, fixed the thigh with a splint, and afterwards with an apparatus, which was discontinued at the end of twelve months. The child then only required the thickening of the sole of the boot about one eighth of an inch to walk well. The operation should be performed after two years of age, and before a new joint is formed on the dorsum ilii.

Twenty-two cases of congenital varus were treated in St. George's Hospital during two years. In all, the tibialis anticus and posticus were divided, and when the punctures healed, in two or three days' time, the foot was gradually everted and brought into the condition of equinus; then the plantar fascia was, if necessary, divided, and subsequently the tendo-Achillis. The after-treatment, he particularly urges, is of the greatest importance: passive motion, shampooing, bathing, friction with oils, and galvanism are to be used freely until the joint moves easily, and the muscles gain power of action.

A very carefully written paper on talipes equinus, by Mr. Nayler, goes into the pathology of the affection with a minute examination of a preparation in the museum of St. Mary's Hospital. The cause is often difficult if not impossible to trace. In early life, it often follows dentition, eruptive fever, ascarides; later, it follows rheumatism, from the foot being kept in one position. Various traumatic causes are also referred to. The complications, and especially its modification by paralysis, are considered, and the prognosis, except in the latter case, said to be favorable. The necessary operations and after treatment are fully described.

Dr. William Ogle's paper on "the Diurnal Variations in the temperature of the Human Body in Health" aims at fixing the normal standard of temperature by which to judge of states of disease. Diagrams of diurnal fluctuations and tables of observations taken during twelve months are given. We can only here mention the conclusions arrived at, which are these: —1. The temperature of the internal organs is at its minimum about 6 a.m. After this is a rise, reaching its maximum late in the afternoon. Then a fall begins, which lasts till 6 a.m. again. The greater part of the rise is due to the combined influence

of exercise and food. Alcohol, or rather claret, causes an immediate rapid fall; but this effect is temporary, and a reaction ensues, by which the temperature is carried to as high a point or even higher than it would have reached had no alcohol been taken. There is a rise in the early morning while we are still asleep, and a fall in the evening while we are still awake. The range within which the ordinary diurnal fluctuations occur is not of greater extent than about 1½° Fahr.

Mr. Pollock, in a paper on "the Rupture of Arteries dependent on External Injury," details two cases. In one the femoral was torn across just where it is about to turn round the bone, the vein being uninjured; in the other, the popliteal artery and vein were both ruptured. "When," he continues, "the extravasation of blood is venous, it is circumscribed and does not continue to spread; when arterial, it is diffused through the subcutaneous and intermuscular cellular tissue. In the latter case the hæmorrhage continues until the surface of the limb becomes hard and tense; the extremity feels cold and is pulseless, and there is much immediate constitutional disturbance from loss of blood; gangrene usually, in the lower extremity always, commences sooner or later. *Treatment*:—If the femoral or popliteal be the seat of lesion, primary amputation, or as soon as the patient can bear it, should be performed. If the brachial or axillary, recovery may be possible without the loss of the limb, and we may wait for gangrene to commence.

Dr. Dickinson relates five cases in which, at the autopsies, he found coagula in the cerebral arteries which were certainly not of post-mortem origin. All had been seized with what is called apoplexy, and the paralysis was on the opposite side to the affected arteries. In four out of the five cases there was mitral valvular obstruction.

Dr. Dickinson thinks there are three conditions which lead to coagulation of blood in the living body: changes in the blood itself, retardation of its current, a rough or otherwise diseased surface which may have the power of collecting the fibrine from the blood.

Mr. Holmes examines the particulars of 300 amputations of the upper and lower limbs, chiefly with respect to the age of the patients, and to the proportion of those that died from the sequelæ of the operation in comparison with those who died from previous injury and disease. He finds that the percentage of deaths after amputations of all kinds grouped together rises gradually and uninterruptedly with advancing years, and at any given period after thirty years of age the risk is more than twice as great as it was at the same period after birth. Mr. Holmes then inquires into the causes of death after amputation, and classifies the 18 deaths which occurred

in the tables of the 300 amputations. 1. Death from causes unconnected with the operation, inevitable. 2. Death from other causes coinciding with the operation. 3. Death from the operation,—by pyæmia, sloughing and phagedæna, erysipelas and diffuse inflammation, secondary hæmorrhage, exhaustion —distinguishing, in each of these latter headings, those instances in which the viscera and blood-vessels were healthy, from those in which they were diseased.

The first class comprehends 14 cases; the second, 33; the third, 36. The particulars of each case is set out in elaborate tables. After discussing the influence of the plan of operation and the complications following, Mr. Holmes comes to the conclusion, that in ordinary hospital practice the influence on the rate of mortality, of secondary hæmorrhage, erysipelatous disease, and hospital phagedæna is very trifling, and that, *cæteris paribus*, the rate of mortality varies with the prevalence of pyæmia; and urges the necessity for some careful method of studying the condition of the wards, and of the atmosphere of the wards in various states of weather, and at various times of day and night.

Following the above paper, Mr. Vasey contributes "Statistical Tables from the Dental Case-Books" of the hospital; and the volume concludes with the Annual Reports (for 1865) on the Medical and Surgical Cases, by the medical and surgical registrars, Dr. Sturges and Mr. Pick respectively. These reports are written with much care, and show that considerable industry has been expended on their construction.

On reviewing generally this first volume of the 'St. George's Hospital Reports,' we think our readers will agree with us that it is composed of papers of considerable merit and of great practical interest, carefully and concisely written, and altogether of such a nature as to give much promise for the future. We cannot but congratulate the editors on the amount of co-operation which they have received in their undertaking, and in repayment of all the trouble and anxiety which are of necessity incidental to their duties. As we have before said on several occasions, a vast amount of useful and available material is being constantly lost in medical practice for the want of willingness, or of care, or of time, on the part of observers to utilise it; and especially has this been so in those fertile fields, our public hospitals. St. George's Hospital is not a whit too early in following the example which has already been set, and with such general approval, by Guy's, by the London, and by St. Bartholomew's Hospital.

REVIEW VIII.

Army Hygiene. By CHARLES ALEXANDER GORDON, M.D., C.B., Deputy Inspector-General of Hospitals, Army Medical Department; Member of the Sanitary Commission for Bengal, &c. &c. 1866. Calcutta and London.

THE subject of the work under our consideration and the mode of treating the subject adopted by the author claim the attention of the profession, more especially of those members of it who purpose undertaking the military branches of medicine.

Many details of the practical working of the army medical service are succinctly given, being taken from the records of some of the most trying actions and services in which our army has been engaged. Several facts are also given from the records of the French, American, and other armies.

Dr. Millingen, it appears, proposed in 1818 to establish an army medical school, suggesting that professors "of the following branches of the medical sciences should be selected from the most able medical officers: namely, theory and practice of medicine, theory and practice of surgery, military hygiene, morbid anatomy, and botany."

The sound judgment of Dr. Millingen, that showed itself in his suggestions, was not acted upon until very recent times; it is at variance with some reformers of medical education, who would recommend the reduction of portions of the curriculum of study now enjoined in the classes of the several educating bodies of the United Kingdom. It appears to us that it would be unwise to lessen the number of subjects now taught in the medical schools, especially when the public generally are daily becoming more liberally educated. To alter the mode of teaching, we conceive, might be a more correct method of proceeding.

Dr. Gordon speaks forcibly of the frequent fatal effects of exposure, especially in tropical climates, to sun or rain, and the emanations from graveyards in cholera seasons, as seen among troops forming funeral parties.

Comparative exemption from cholera among the Bedouin Arabs is attributed to their not congregating in masses.

Cholera has attacked workmen, with great violence, who came upon the remains of a number of persons who had died of cholera, as reported by Dr. Moore. The latter author also instances another body of workmen who, in cutting through an old graveyard, came upon a spring of apparently pure water:

many who drank of this were, within a few hours, seized with severe cholera.

Dr. Gordon alludes to the site selected at Benares that had to be abandoned from its notorious unhealthiness, and the question, how far its state was owing to the decomposing dead within its precincts, as it was found to have been in former times a Mahomedan burial-ground.

The poisoning of camp-ground by cholera excreta, and the dangerous effect of using water percolating such soil, as well as the injurious properties of emanations from such ground, are mentioned.

Epidemics from different foci.—" The history of the cholera epidemic of 1861 in India sufficiently shows that this disease may have several distinct and independent points of spontaneous origin, and yet spread by infection and personal communication. It has now come to be ascertained, also, that the epidemic influence once brought into activity, travels by definite directions; and that for a body of troops to proceed within the sphere or line of progress of that influence, is, to a certainty, to subject them to be attacked by the malady.

" The most recent example of this occurred at Mhow, in the Bombay Presidency of India, where a detachment of troops having been so marched into the line of progress of the epidemic, they were all but annihilated by it."

The above deserves the consideration of the few members of the profession who hold views opposed to the infectious power of cholera.

Of the geological features of districts, coal formations and " trap " rock, as well as clay and alluvial soils, are most generally found to be those predisposing to cholera; whereas the disease is rare where laterite forms the character of the country.

Infection.—" Some writers have of late years stated a theory that there is in reality no specific infection in existence; that when other favouring circumstances combine with neglect of sanitation to bring into activity the hitherto latent morbific influence, that influence may manifest itself in the production of cholera, yellow fever, smallpox, scarlatina, or in other forms of zymotic disease, according to accidental conditions of locality and climate. Nor does this theory appear so vague as it may at first seem, if we consider the phenomena of some diseases, more especially those of a non-eruptive nature. Thus, in India the coincidence of epidemics of cholera and intermittent fever is often observed; and at some stations in that country—as, for example, Agra and Peshawar—the cold stage of the latter disease was on many occasions attended by diarrhœa, vomiting, and

collapse: so much so, that medical officers have stated, in their reports, the apparent affinity that seemed to exist between the manifestation of these diseases."

We quote the above at length, and will add that allusion is made to the points of resemblance between yellow fever and cholera. He refers to the researches of Pettenkofer, of Alison, Monfalcon, and Barker, on fomites, pointing out the necessity for considering the "fomites of each different kind of epidemic" as "ever-existing entities," and to be guarded against accordingly: and he goes on to show the great value of preventive measures against epidemic disease.

Camping out, as at Bermuda in 1864, has been frequently sufficient to stay the fatal progress of yellow fever and other diseases.

The Indian Cholera Commission, 1861, ascertained that of the soldiers stationed in the North-Western Provinces, the temperate and the intemperate appeared equally liable to attacks of cholera; but the disease was much more fatal among the latter than the former.

Rest and Sleep.—"We should bear in mind, that in the case of soldiers as well as with other classes, excessive labour demands excessive rest. This was a maxim of Napoleon, and daily experience confirms it. On a campaign, it may be difficult or impossible to obtain for the soldier that amount of rest which sanitary requirements demand. In ordinary circumstances, however, it may be attained by diminishing night duties as far as is practicable.

"Duties that involve deprivation of the night's rest exert more than any others a pernicious effect upon the health: the physical powers having thus insufficient time to recover their tone after severe exertion or arduous duty, undergo decay, and thus the soldier, while yet a young man, becomes what is called 'worn out.'

"On home service a soldier ought not, as far as sanitary considerations are concerned, to be on night duty oftener than one night for four he is in bed; nor in India oftener than one night in seven at least."

Propagation of Infection.—"As remarked by Dr. Millingen, contagious diseases that have proved most fatal have frequently been traced to intercourse with prisoners of war. It is important, therefore, that they should not be put up with troops, or even march with large bodies of them."

Clothing has been known, as in China, &c., to have been the means of spreading smallpox and other diseases. Cholera has been often traced in India to troops meeting large bodies of pilgrims.

The comparative freedom of the Bedouin Arabs from cholera, attributable to their not congregating in masses, is mentioned.

This writer quotes Dr. Hammond, Surgeon-General of the United States Army, on the carriage of disease so readily conveyed by means of clothing, furniture, walls, and especially bedding. Hospital gangrene was only removed from the New York City Hospital by taking down the entire walls. The fact of the 'Dreadnought,' and other vessels, having to be abandoned on account of the tenacity with which the infection of erysipelas and other diseases clung to them, is stated by Dr. Gordon.

An outbreak of smallpox at Quebec occurred on the opening of a cemetery where a large number of victims of that disease had been buried years before.

In tropical countries, ferruginous soils are connected with deadly fevers, calcareous salts with goître, and swampy alluvial districts with enlarged spleen and general cachectic habit of body. Again, soil and water impregnated with salts of soda, as Aden, and the desert tract, including Mooltan, Delhi, and Lahore, where suitable vegetables are consequently wanting, produce scurvy in an endemic form. Hæmorrhagic dysentery seems to spring from this form of disease. A vegetable and meat diet does not ensure immunity from scurvy, if sameness of diet, bad cooking, arduous work, fatigue, want of sleep, dirt, crowding, mental despondency, &c. (so common to troops in a hard-fought campaign), concur. Dr. Gordon very properly states that the knowledge now possessed on these vital points should arm us against such dangers in future, and that we should modify the combination of circumstances so as to prevent or moderate the scorbutic taint in an army. The natives of India use the *Phyllanthus emblica*, and the Americans the *Chenopodium album*, the *Rumex acetosella*, pickles and sour-krout, vinegar and molasses; while the French benefited greatly in the Crimea by using a salad of the dandelion with vinegar. Though elevated ground is most generally highly advantageous in a sanitary point of view as a site for barracks, it is not always preferable to lower positions. The author mentions Gibraltar as an instance of an elevated position being unwholesome: in this statement we cannot wholly agree, inasmuch as the soldiers' quarters are not much elevated, at least if compared with the greatly higher parts of the rock, the *débris* of vegetation from which is so constantly washed down to the lower parts of the locality, on which stand the men's quarters, the town, &c.; on the contrary, when disease carried off great numbers in the barracks and town, the fatality ceased on the formation of a camp on Windmill Hill.

To the consideration of the great importance of hill stations in India, Dr. Gordon most properly devotes a long chapter, pointing out how the rarefied and damp air of the much-elevated hill stations frequently causes disease of the chest and alimentary canal in men debilitated by disease or exhaustion in the plains. The great value of these stations in a large class of cases is also well stated, and judicious suggestions are made relative to those most fitting for hill stations.

The whole subject requires the attention of the authorities, being one of importance in a national as well as in a sanitary point of view.

The opinions of Jackson (who wrote in 1804), of Chevers, Parkes, Jeffreys, Sutherland, Sir Ranald Martin, and of the Sanitary Commissioners, are quoted to show the great necessity existing in India for the proper construction of barracks.

"Entire isolation of the floors from the soil is essential to health in tropical climates.

"In India it is not safe for men to sleep within the sphere of night malaria proceeding from the ground within the limits of night fogs.

"The floors of barracks and hospitals should be raised several feet above the soil, on arches, the height varying with the nature of the locality. In low, flat localities, troops as well as the sick should always sleep in upper stories."

"According to the Royal Commission, the ordinary width of a barrack-room should be 20 to 24 feet; and under no circumstances whatever should more than two rows of beds be placed between opposite doors."

Verandahs are not recommended in India, as impeding both light and ventilation.

The walls should be of thin stone or brick masonry; if thick, they absorb much heat by day, and radiate it proportionately at night.

Old bricks, which are often saturated with organic matters, must on no account be used. (The recent local outbreak of cattle disease at Islington is a strong fact in proof of the great risk of employing old bricks, as Dr. Ballard considers he has traced the attack to some old bricks which belonged to the former cowshed in which so many fatal cases occurred in the previous epidemic.) Dr. Roscoe's experiments indicate the passage of carbonic acid gas through hard and well-baked bricks.

The Sanitary Commission for Bengal recommend a plan for barracks in India, a ground plan for which faces the title-page. According to it, the front occupies a line 1260 yards, the depth one of 631 yards; roads may be made so as to afford free means of communication between the different buildings. In another

part of the work the necessity for well-made raised footpaths is pointed out. Free perflation is allowed by the intermediate spaces between the several buildings; the quarters for the married are placed at the flanks, and in rear of those for single men; and all out-buildings, as plunge-baths, skittle-alleys, ball-courts, forge, &c., have suitable positions allotted them. When it is remembered that no less than £10,000,000 are to be devoted in the next ten years to the erection of suitable barracks and hospitals in India for the men by whom that great empire is held, it will be seen how necessary it is to execute the works on the best principles and in the most efficient manner.

Under the head of Clothing, the great disadvantage (amounting on one occasion even to disaster to an expedition) liable to arise from the use of too heavy clothing in a hot climate is mentioned: again, the injurious effects of cotton, or other too light material, in the rains or in a variable climate, is dwelt upon.

Civilians in such climates have long used light woollen clothes, as in Calcutta, Hong-Kong, West Coast of Africa, &c., during the rainy season.

The flannel waist-belt supplied to troops in India is a most valuable prophylactic.

The use of flannel under-clothing is as ancient as the armies of Rome. Although many most eminent physicians have in almost all ages advised the use of flannel under-clothing, there are those who consider it possesses the disadvantages of increasing the susceptibility to impression, and of retaining contagion.

The great value of the soldier having a piece of violet or green silk gauze to shade the eyes when necessary, will be appreciated by those who have encountered a tropical wind, and dust or sand.

Dr. Gordon regards the stock and the beard objectionable; he would, however, allow men on duty to please themselves in regard to the beard when on active service. He dwells upon the *cummerbund* or girdle of thick folds of cloth as a valuable preventive of illness.

Cotton socks, if good and often changed, are far more comfortable than woollen. This writer follows Dr. Parkes in speaking of the frequent defects in the boots supplied to troops, and the vast importance a suitable and well-fitting boot is to the soldier. The varying requirements of season and country should also enter into the regulations regarding the supply of soldiers' boots.

Hospital accommodation, so deficient in the army prior to

the Crimean war, cannot be laid to the account of the medical staff of the army, as representations were frequently made by it to the authorities without effect; it is but fair, therefore, observes our author, to allow credit to the service for exertions made by it in the right direction by Dr. Hennen and others years ago.

Pavilions are recommended by the Barrack and Hospital Commissioners in India. There should not be placed under one roof more than sixty or seventy sick, with their attendants. A convalescent ward should be provided, with accommodation for about 25 per cent. of the patients. An ophthalmic ward, with separate lavatory and separate basin and towel for each case, should also be provided. Wards for special cases should also be provided, as well as for infectious diseases.

The wards on the upper story should contain 2730 cubic feet, and on the lower 2340 cubic feet, per bed.

The dry system of conservancy should be carried out in the privies, of which latter there should be indoor and outdoor provided. The author laments the necessity of pointing out so many measures as requisite in Indian hospitals, which are still so sadly deficient in what we have touched on in this Review, and in many other details given in the work; and he quotes Dr. Parkes on the same subject.

The foundation of every hospital should be laid on dry ground, covered with concrete, and have an arched basement, so as to admit a free circulation of air underneath the ward-floors; the latter should be not less than four or five feet from the ground; and in malarious districts the sick should always be on upper floors.

Double verandahs are not allowed to barracks or hospitals by the Commissioners. They recommend twenty-six feet as the width for the wards, and from 1500 to 2500 cubic feet of air for each of the sick. A ward is recommended to hold twenty to twenty-four sick.

The example of some hospitals at St. Petersburg, wherein were wards without direct light, and which only yielded a fourth part of the recoveries when compared with patients treated in light rooms, should always be borne in mind when arranging the windows of a hospital. The great importance of officers' hospitals is mentioned, and can easily be conceived, especially in time of war; it has been often brought under the notice of the Government. The necessity of allowing a liberal diet and extras, of having trained hospital attendants, and of providing at Calcutta an establishment to answer some at least of the purposes that Netley does, is treated of at some length.

Potable water should be limpid, unaffected by the addition of

lime-water, chloride of barium, nitric acid, oxalate of ammonia, or hydrochloric acid. Beans and peas should be readily boiled in it, and soap readily dissolved.

Several other important matters connected with water supply and quality, with baths and bathing, and the connection between impure water and cholera, fever, dysentery, &c., are treated of. Several useful rules for finding water in any district are given; also some for the formation of filters, pumps, conduits, reservoirs, tanks, and for the general analysis of water.

Respiration—the impurities of the atmosphere from crowding, from combustion, organic matter, emanations from sewers, churchyards, brick-kilns, marshes, &c., are duly considered.

Dr. Jackson's statement in favour of ventilation is quoted; it was as follows, and was written in reference to the Peninsular War:—"It has often happened that in the history of the late war more human life was destroyed by accumulating sick men in low, ill-ventilated apartments, than by leaving them exposed in severe and inclement weather at the side of a hedge or common dyke."

Recent wars, as that between the French and Austrians in 1859, and the civil war in America still more recently, quite justify Dr. Jackson's statements, and show that judicious transport was safer than a crowded hospital.

The great value of thermantodotes and tatties in cooling the air in barracks, &c., in India is insisted upon; and such means well repay any expense incurred in their maintenance by the increased health and comfort of the men.

Overcrowding, or Ochlesis.—Captain Hall, in his work on the Esquimaux, describes the horrible fetor that exists within the close, ill-ventilated huts occupied by these people; and any person who has been in the Himalayas has, no doubt, experienced a feeling of nausea while passing the doors of the Hillmen's huts from a similar cause. The accounts of the progress of the Indian plague, or mahamurree, which devastated the hilly parts of India in 1851 and 1852, are replete with examples of the evils that occurred from the combined effects of overcrowding, want of ventilation, and inferior diet.

In concluding our notice of Dr. Gordon's work, we would add our commendation of it, as containing much most useful matter, and well worthy the attention of the authorities, as well as of that of the medical profession.

PART SECOND.

Bibliographical Record.

ART. I.—*Physiologie de la Voix et de la Parole.* Par Dr. EDOUARD FOURNIÉ. Avec Figures dans le texte. Paris, 1866, pp. 816.

Physiology of Voice and Speech. By Dr. EDWARD FOURNIÉ. With Illustrations. Paris, 1866.

THIS is a voluminous and exhaustive treatise upon one organ and function, written with the diffuseness characteristic of French authors. The production of such copious tomes demands of an author the pen of a ready writer; industry to search out what every one, at least of his own countrymen, has had to say about the topic undertaken, and to make his annotations thereon; a fertile imagination to discover its many aspects and relations; patience to agglomerate the results; a sufficient modicum of original thought, to produce, it may happen, a little kernel of added knowledge in the resultant mass; and withal, the self-conviction that the subject-matter is of huge importance to humanity and to science, and that he is the proper individual to elucidate it. The books so produced are often well put together, and the original matter well worth setting forward; but, "Life is short, but Art is long;" and as the latter is daily lengthening itself without any proportionate extension of the former, the reading of such books becomes an impossibility, and their production an infliction upon suffering humanity.

All real students would hail a change in the habit of book-making, especially on the Continent, and would be grateful to writers of other than text-books for assuming that they have mastered the elements of the subject treated of, have informed themselves of its history, and that what they require is the development of the new doctrine or the enunciation of the new fact which the book before them professes to contain.

The author of the treatise under review duly prefaces it by asserting the importance of its subject, which he places first among the branches of human knowledge. "Of all the divi-

sions of the science of man (he writes) that which concerns the study of the voice and speech is assuredly the most important and the most useful." Wherefore we are bound to accept the inference that the treatise on this most important subject must partake of like importance, and that we cannot have too much of so good a thing.

One principal problem Dr. Fournié undertakes to solve is the mode of formation of speech—not uttered speech, but speech as present in the mind—"parole pensée," or thought-speech, as he designates it. To accomplish his purpose he has been compelled, he informs us, to discuss some of the most delicate and obscure problems in psychology. In the absence of positive conceptions regarding the formation of specch, he is convinced that the operations of the human mind are incomprehensible, but that by means of a knowledge of the physiology of speech we possess a key to the interpretation of the problems of the human understanding which, in the hands of the pure psychologist, must ever continue inexplicable.

But before embarking on this physiologico-psychological exploit, the author lingers long upon the solid ground of anatomical facts regarding the vocal instrument, and of direct physiological research respecting the movements of its parts. He likewise indulges in a long disquisition concerning the opinions of Greek and Roman philosophers, and of the learned of a later date in several countries, though chiefly in his own, on the formation of the voice in the larynx; and in order to give his work that exhaustive character so much aimed at by Frenchmen, he furnishes the reader with five chapters on acoustics and acoustic instruments. His account of the anatomy of the vocal instrument is very complete, and comprises numerous investigations conducted by himself on collateral topics not considered in ordinary anatomical works, and which alone are likely to be referred to by the reader possessed of the ordinary amount of anatomical and physiological information. Among such collateral subjects are, the development of the larynx at different ages, its modifications as connected with sex or with individual or special characters, the varying conditions being exhibited by accurate measurements carefully tabulated.

During the first three years of infantile existence the larynx is rudely developed, and no very distinctive feature appears in this organ in a child three years old that is not to be found in a child three months old. Its development and growth remain much in abeyance until the twelfth or thirteenth year; but after this period the several parts are rapidly evolved, and their lining membrane undergoes a peculiar change, whereby sonorous vibrations are perfected. Development, however, is

not completed until about the twenty-fifth year, and in the case of females never proceeds so far as in males. In the former likewise the larynx is smaller in every dimension; its outline is more rounded, its angles less prominent, and both its cartilaginous and its soft tissues have less consistence. To the slighter consistence of the vocal membrane the difference of diapason in the female as compared with the male is attributable.

In the preceding remarks we have alluded to the subjects comprised in the first three books, viz., in Book I, on acoustics; in Book II, on the anatomy of the vocal instrument; and in Book III, on the critical history of the different theories of the voice. Book IV, the next in order, is occupied with the physiology of the voice, and is prefaced by an introductory chapter on the construction and modes of using the laryngoscope. The laryngeal mirror used by Fournié is the quadrangular one introduced by Czermak, and the reflector he advocates is one made by Charrière, of Paris, which is fixed across the bridge of the nose, resting on a padded saddle, and is thus brought nearly on a level with the plane of vision. The instrument is kept *in sitú* by an elastic band of steel, extended over the head to the occiput. After citing the definitions of the voice as given by numerous physiologists, he propounds, as more complete and correct, the following one:—"The voice is the product of a special reed-instrument, possessing walls capable of modification, and subjected to the influence of muscular action, its vibrating portion being formed by the mucous fold that limits the border of the glottis. The vibrations are excited by the passage of air through the glottis."

For the principal facts embodied in this definition the author claims originality, and proceeds to unfold a new theory of the voice. In the first place, he asserts that sounds are not produced by the vibration, from the passage of air, of the vocal cords in their entire thickness, but only by that of the mucous membrane which overlies the white fibrous tissue, and is so loosely attached on its inner aspect, that the current of air uplifts and distends it like a fringe, and in so doing throws it into vibrations—a result impossible in the case of the rigidly extended fibrous cords. This assertion M. Fournié justifies by an appeal to physical principles, to physiological experiment, and to pathological observation, and applies the term *vocal membrane* to this mucous lamina.

He goes on to examine the physiological action of each several portion and tissue of the larynx and of its complementary parts, and illustrates his views by reference to a very ingenious and effective artificial larynx which he has contrived and had engraved, and in the ensuing chapter discusses the mechanism

of the formation of the voice. Three types of voice are recognised—the chest, the throat, and the mixed—each and all the result of the vibration of the same mucous lamina, their difference being attributable to the muscular forces brought to bear upon this lamina, and affecting its length, its thickness, and its tension in connection with alterations of the parts associated in structure and function with it.

The next chapter is occupied with the subject of the *timbre* of the voice, and is followed by others, of less extent however, on the intensity of vocal sounds; on vocal illusions (ventriloquism, &c.); on the influence of sex and on that of age upon the voice, and on individual varieties of the voice. Chapters XI and XII are devoted to a consideration of the application of the theory of the voice promulgated, to the purposes of instruction in singing, and to the pathology of laryngeal maladies.

The author now arrives at the pyschological section of this exhaustive treatise, and proceeds to inquire into the physiology of speech in relation to mental action. Speech, as he rightly points out, is a complex phenomenon, consisting of an intellectual act and a sonorous mechanism; the former being indissolubly connected with the thinking principle (pensée) or mind. Further, Dr. Fournié believes he has succeeded in demonstrating physiologically the participation of sensibility in the silent operations of the mind, and thus made good in all its terms the Aristotelian maxim, "*Nihil est in intellectu quin prius fuerit in sensu*,"—a feat hitherto unaccomplished. As a necessary prelude to the discussion undertaken, he proceeds to define the meaning severally attached to the terms sensibility, sensation, and memory of the senses (des sens).

Sensibility is represented as a threefold phenomenon, comprising—1. Impression; 2. Conduction or transmission, and 3. Perception, and as non-existing in the absence of any one of these constituent elements. Besides being provoked by the external and internal agents usually recognised by physiologists, it is also called forth in a special manner by speech, and then presents itself to notice as "special sensibility." Again, sensibility is no property of matter, though excitability, at times confounded with it, is. Neither is it a property of spirit. If viewed in connection with matter, it appears as impression and transmission; with spirit, it constitutes perception. It subsists in the union of matter and spirit. The application of the term to the phenomena of organic life is not justifiable, and the expression unconscious sensibility, used with regard to those phenomena, is indefensible.

Sensation is described as "sensibility in exercise." The term

can be rightly employed only where the three phenomena, impression, transmission, and perception, are concurrent.

Memory of the Senses is the result of a special action of the intellect upon the organs of sense, so that impressions once perceived through the agency of the latter are revived in the mind in the absence of sensible objects. The mental act here constitutes the initiative; it is under the control of the will, and it operates upon the organs of sense, *i. e.* upon matter, so as to call forth a *subjective* (as opposed to a real or *objective*) sensation, which constitutes memory: for memory is no fundamental faculty of the mind, there being as many memories as there are sensations, or objects to remember. Sensations are of three classes: 1, those derived from the relations of the individual with the external world by means of the special senses; 2, those derived from the involuntary, and, 3, those from the voluntary exercise of our organs—acts of relative life. The "memory of speech" appertains to this last class, and is compounded of the memory of ideas and of the memory of sonorous impressions. It is an instance of the memory of the acts of relative life.

A faculty of co-ordination has, in M. Fournié's belief, no existence as such. What has been regarded as such, is nothing more than a phenomenon of memory. Every object, sound, or image is subjectively placed before the mind in concert with the movements associated with it; and this representation of it suffices to arouse the will (volition) to effect the combination of movements already mentally conceived, and previously learnt by usage.

The "sense of thought" is a psychological novelty introduced by the author to express a group of phenomena which, though well known, have never been viewed from the same stand-point. According to his interpretation, the "intelligence" (understanding) is the faculty of perceiving impressions, and, consequently, of acquiring cognition of objects. Sensations give no more than an image of material objects to the understanding, and do not of themselves constitute ideas; for in simple perceptions no act of thought is concerned. The understanding is passive in the reception of images, and the most elementary operation of the human mind presupposes the establishment of a relation between two perceptions; and an "*idea*" is something more than a sensation—it is, in short, a sensation transformed by the intelligence into voluntary and definite movement. This movement constitutes the element of language. To think is to reproduce subjectively these different movements, and to determine the relations between them. Hence an idea cannot be transmitted and perceived by the agency of the ordinary

organs of sensation. To designate the perception by itself of the acts of the intelligence (understanding) is what M. Fournié intends by the term "sense of thought," which indeed he regards as a special sense, although possessing no special organ for its exercise.

Having laid this groundwork, the author proceeds to erect his hypothesis of speech. Speech (he says) stands in the same position relatively to the sense of thought that the image of objects does to the sense of sight. This diversity, however, obtains between the impressions in the two cases—viz., that the verbal impression is always a consequence of voluntary activity on the part of our organs, whilst the visual impression may be independent of any such activity. In the formation of a word the following phenomena occur:—1, a determination of the will; 2, a transmission of the will through the medium of motor nerves; 3, a measured amount of muscular excitation by the aid of the muscular sense; and, 4, an association of the movements in a definite result, to which moreover the sense of hearing is indispensable. In a word, the sense of hearing is the educator of speech.

M. Fournié sums up his considerations respecting the physiology of speech in the three propositions following:—1. Education of the movements of speech by imitation and with the aid of hearing, as the initiating, educating, and exciting sense. 2. An act of volition whereby the signification of a word is associated with the movements concerned in its production. The meaning of the word and the movements are so intimately blended together, that they are viewed as one and the same phenomenon. 3. Transmission of this act of volition by the intellect to the intellect itself, under a sonorous form, by the medium of the sense of hearing.

For the further development and illustration of the author's views of the physiology of speech, of the cerebral seat of its faculty, and of the possible applications of his hypotheses in practical medicine, in the interpretation of disordered mental function, and in the method of instructing the deaf and dumb, we must refer the reader to the work itself, with the persuasion that, if conviction does not follow the study of its arguments, much instruction may be gained from the mode of argument pursued and the illustrations brought forward.

As we commenced by indicating, M. Fournié's book is much too voluminous and diffuse for ordinary readers engaged in the active duties of life; but physiological and psychological students who desire to inform themselves of all that can be written about the voice and speech may advantageously study those chapters

in the work which bear upon the particular subject of their inquiries.

ART. II.—*A Manual of Practical Hygiene, prepared especially for the Use of the Medical Service of the Army.* By EDMUND A. PARKES, M.D., F.R.S., Professor of Military Hygiene in the Army Medical School, &c. &c. Second Edition. *London:* John Churchill and Sons. 1866.

THE demand for this elaborate and comprehensive work, in its first edition, affords an excellent criterion of its value. In the preface to the present edition, the author states, "Without greatly increasing the size of this work, much new material has been added, and the old matter has been carefully revised, though the rapid sale of the first edition has rendered any great changes unnecessary."

The value of hygiene has come prominently out in the recent visitations of cattle plague and cholera. London, warned by previous experience, has been found, in the main, not to have uselessly lavished her millions on the grand system of sewerage and the improvement of water supply. Cholera was not slow in seizing upon those localities still most deficient in sanitary arrangements. Thus, the east end of London suffered greatly in excess of those portions of the city better circumstanced in these particulars.

It is still, however, our duty to observe the severity with which the neighbourhood of Woolwich was visited with cholera late last autumn. From this, and much experience in other places, the great importance of the question of water supply and drainage comes prominently into view; moreover, it would be idle to say that the removal of sewage by water is satisfactorily established as the best method for adoption. The rapid growth of London and of other great cities, the comparative limited amount of water supply, and the question of the pollution of so many watercourses, as well as the occasional visitations in our own climate from severe frosts, tend to increase the necessity for a searching investigation into the merits of the systems pursued in the sewerage of cities.

The value of early measures in dealing with cattle plague, and the importance of disinfection in this disease and in cholera, has been well seen of late, as also the necessity for improvements in the housing of our working classes and in our cattle-sheds. Again, inquiries that have been carried on in the East justify the statements of so many of our fellow-labourers

in medicine, who, at home and abroad, in civil and in military life, have for generations striven to convince the authorities of the paramount importance of sanitary measures long before, as Dr. Gordon observes, this term was used.

Unwise indeed is that government that would neglect to use the valuable light shed by such men as the authors under our immediate notice, and by so many like them who, with much less encouragement, have striven in the same course.

If there is one matter more than another that needs legislation, it is some means of being able to separate persons affected with, say cholera, from the bulk of the population, and of compelling such attendance upon all affected persons as may effectually guard the community at large against the introduction of fatal disease. It is not exactly an international quarantine that would meet the case, as cholera is not bound by quarantine.

The preservation of Ireland from cattle disease may be adduced as an instance of the value of measures of prevention, and as well worthy, as far as applicable, of adoption against cholera, &c.

Greatly increased powers have been granted to the Executive by the Diseases Prevention Act of last year; and it would be a matter well worthy of the care of the Imperial Government to undertake, through means of those who (such as the authors, and others of similar attainments) may be said to be experts in such matters, an investigation into the local circumstances under which cholera arises in India, and into the means by which it appears to spread thence, carrying death throughout the world.

The previous edition of Dr. Parkes's work has been already fully noticed in the 'British and Foreign Medico-Chirurgical Review.'[1] The present edition contains much new matter, as well as several additional illustrations. In fine, we have the utmost confidence in recommending 'Practical Hygiene,' not only to the military and naval medical officer, but also to our brethren in civil life.

ART. III.—*On Addison's Disease. Clinical Lectures on Addison's Disease, and a Report on the Diseases of the Suprarenal Capsules.* By EDWARD HEADLAM GREENHOW, M.D., &c. *London,* 1867. Pp. 64.

To the pathologist and medical inquirer this little work cannot fail of being interesting. The main object of its author is to prove that the disease which bears the name of Addison is a

[1] July, 1865, vol. xxxvi.

reality, and, subordinate to that, to contribute to its further elucidation. In both, we think, he has been succcssful.

The work consists of two parts,—one comprising lectures and reports on the disease, in which carefully detailed cases are given, and the subject is generally discussed; the other is "*an abstract of one hundred and ninety-six cases*" of the reported malady, collected by the author from all available sources, given on a uniform plan and arranged in a tabular form.

The more important results of this analysis, as stated by Dr. Greenhow, are briefly the following:

1. That there is a decided relation between a certain train of constitutional symptoms, attended mostly by discoloration of skin of a distinctive kind, and a particular organic lesion of the supra-renal capsules.

2. That there is some intimate relation between this lesion of the capsules and the tubercular diathesis.

3. That the affected capsules are generally enlarged, hard and nodulated, &c.

4. That in a large number of the cases there is evidence of inflammation of the cellular envelope of the capsules.

5. That in the majority of cases both capsules are diseased, but rarely in like degree.

6. That the intimate condition of the supra-renal nerves is doubtful, and, in consequence, a proper subject for further inquiry.

7. That the seat of the pigment, the cause of the discoloration, is chiefly the rete mucosum, the epidermis and the superficial layers of the true skin being generally uncoloured.

8. That little certain is known respecting the blood in the disease—another fit subject for inquiry.

9. That the disease is essentially chronic, of uncertain duration, insidious in its beginning, and subject to remissions. This is, we would remark, particularly deserving of attention as regards its treatment.

10. As regards influence of sex, age, occupation, on its occurrence, that males have been found the subjects of it in a larger proportion than females; that children under ten years of age are exempt from it, and also persons of very advanced age; and that it is mostly limited to those engaged in active manual labour.

Finally, from the facts generally considered, the author concludes—"That the origin of Addison's disease of the supra-renal capsules is due, in a hitherto unsuspected degree, to the extension of inflammation to these organs from diseased or injured adjacent parts in persons of a tubercular diathesis."

That the exact nature of this obscure disease is yet undis-

covered can hardly be considered surprising, keeping in mind that the function of the organs affected is a problem which has hitherto baffled research.

Our thanks are due to Dr. Greenhow for what he has accomplished. Our brief and imperfect notice of his labours, we hope, may induce many of our readers to peruse his book. Besides what we have adverted to, they will find in it an excellent account of the characteristic symptoms of the malady and hopeful views of its treatment.

ART. IV.—*De la Foudre; de ses Formes, de ses Effets sur l'Homme, les Animaux et les Corps bruts, &c.* Par le Docteur F. SESTIER; rédigé et complété par le Docteur C. MÉHU. *Paris,* 1866. 2 tomes. Pp. 480 and pp. 632.

On Lightning; its Forms, and its Effects on Man, on Animals, and on inanimate Objects, &c. By Dr. F. SESTIER; edited and completed by Dr. MÉHU. 2 vols.

Two volumes octavo, containing above 1000 pages of print, occupied entirely with an account of lightning, its characters and consequences, are calculated to produce something like an electric shock upon the hard-worked student of medicine, who is expected to have his head stored with all the "*-ologies*" which the inventive faculty of examining boards has suggested to possess a theoretical or a practical bearing upon medicine: for let the medical student clearly understand that these thousand and odd pages about lightning are not only written by doctors, but are especially addressed to the profession, and that, sooner or later, some bookworm examiner will stealthily gather from this ample repertorium some facts, and frame questions thereon, to the terrible discomfiture of the ignorami.

The editor, Dr. Méhu, justly claims for this treatise a completeness above every other work yet produced on the subject. By far the greater portion of the information contained in its pages is the result of the researches of Dr. Sestier, carried on with wonderful assiduity during the active pursuit of his profession for a period of ten years. M. Louis, who has written a brief notice, and at whose suggestion, it would seem, the materials collected by Sestier were edited and placed before the world, characterises the collection of facts as immense. At the recommendation of Gavarret to M. Louis, the documents of Sestier were placed in charge of M. Méhu for editing and publishing. Their choice of an editor has been justified by his devotion to the work, by the use made of the materials placed

in his hands, and by the additions and emendations introduced by him.

The work is divided into five principal parts:—1, on the different forms of lightning; 2, on the general effects of lightning on metallic objects—on houses, ships, and powder magazines, and on telegraphs and railways; 3, on the effects of lightning on vegetation and on the soil; 4, on its effects upon man and animals; 5, on the history of lightning conductors, and on the means of personal preservation.

The second volume, which opens with the fourth section, will recommend itself most to the study of medical men, as dealing with subjects more cognate with their profession; yet the first contains much matter that will instruct, and, we may add, entertain the reader, whether medical or otherwise. Authors of all kinds among the principal countries in Europe have been ransacked for illustration of the opinions and statements advanced, and large contributions have also been levied upon the periodical press in further aid of the same purpose. The numerous notices of the effects of lightning are curious and interesting, and demonstrate how mighty an agent of destruction lightning may be, and at the same time how *bizarre* it often is in its action. We need, however, restrict our notice to the more strictly medical contents of this exhaustive work, to those which in a great measure would claim a place in a treatise on medical jurisprudence.

The chapter on the "general lesions" produced by lightning portrays a much larger collection of such lesions than could be predicated theoretically. The injuries inflicted vary between simple ecchymosis, erythema, and scorching of the surface, on the one hand; and wounds resembling those from gunshot, deep lacerations, fractures and dislocations, and the tearing away of more or less of the body, on the other. In some instances of death a visible injury is scarcely discoverable, and then only, perhaps, a discoloured patch on the skin; in others, again, no mark whatever is discernible of electric action. The sudden incineration of individuals is questioned by the author, who, however, believes that those who are suddenly struck and rendered unconscious fall to the ground without any previous perception of light or sound accompanying the shock of zigzag lightning, although such persons may discern the electric discharge when this takes place in the form of a globe or rocket (fusee). The fall to the ground is instantaneous, and the limbs double under the injured individual as though relaxed or paralysed at the moment. Sometimes people are forced along or upwards by the electric current, and now and then are instantaneously rendered rigid.

The nervous system suffers in various ways, the injured individuals complaining of pricking in the limbs, or of tremors and commotion more or less general, or of sundry neuralgic pains. The mind is often affected, and stupor, delirium, and unconsciousness induced either temporarily, or, it may be, for a considerable period. The sufferers are frequently stunned for a time, and the memory afterwards rendered defective. Chronic convulsions often ensue upon recovery from a state of stupor or of apparent death. Rigidity, more or less general, is a common phenomenon of injury by lightning, but it mostly disappears speedily; in about one half of the cases collected by M. Sestier it did not last more than a quarter of an hour. Paralysis from the same cause, moreover, is, as a rule, transitory; it may present itself in a very limited extent, or may seize the limbs, and when it so happens it is oftener the inferior extremities that suffer. The paralysis exhibits this peculiarity, that it is attended with much pain. The eyes are frequently involved in accidents from lightning, and vision lost temporarily or permanently. The ocular muscles may be paralysed or contracted; or photophobia or opththalmia, or amaurosis, hemiopia, myopia, or Daltonism, or cataract may be produced.

Dulness of hearing and sundry noises in the ears are not unusual consequences of lightning, but complete deafness is uncommon. The return of the sense of hearing is gradual. The faculty of articulation is occasionally lost for a time, and with it, in rare cases, the power of deglutition.

Hæmorrhage from the nose, mouth, and ears is often noticed, and also at times takes place from the lungs and stomach, and from the intestinal canal and urinary passages. At the moment of the accident, the principal phenomenon is that of collapse, with coldness of surface, and a small, scarcely perceptible pulse, generally remarkable by its slowness, and but rarely accelerated. This state of collapse is succeeded by active reaction, which by-and-by gives place to sleep. Vomiting of glairy fluid, and of the contents of the stomach, commonly follows upon recovery from the apoplectic state, and also when general convulsions supervene; but it, as a rule, soon ceases.

Passing over the recorded effects of lightning upon the secretions, the next chapter is devoted to the discussion of some singular phenomena of lightning, of which the first noticed is the production of poisonous effects by that agent. This remarkable consequence was noted from the earliest times. It was alluded to by Seneca, and afterwards by Fabricius; nevertheless, not many distinct notices of it are found upon record. A still more extraordinary result of lightning on rare occasions, indeed, is the production of salutary changes in the individuals

struck by it. Cases are reported of cure or relief from paralysis, from spasmodic affections, from deafness and blindness, and from sundry other affections.

There seems to be no constant rule respecting the time of occurrence and the duration of the post-mortem rigidity of the ordinary type; but there is, at times, a peculiar form of rigidity, of a convulsive or tetanic character, following immediately upon the electric shock, and not progressive, but at once complete.

Material appears wanting, ample and sufficiently satisfactory, to supply an answer to the inquiry how lightning kills. Dr. Sestier advances the following conclusion, which to our mind, however, simply leaves the question unanswered, or rather veils it by words that may hide our real ignorance of the matter. He says—"Sudden death appears to depend on the instantaneous abolition of the dynamical forces of the economy, without any necessary visible lesion; lightning would seem to strike the vital principle itself."

The next section is occupied with a discussion of the external appearances of the bodies of those killed by lightning. The fact of persons so destroyed being afterwards standing or sitting, and rigid, as if petrified at the moment of attack, is one of the most curious in the history of the subject before us, and appears to be well substantiated.

Decomposition mostly proceeds more rapidly in persons killed by lightning than in others dying from other causes, and the authors concur in the general belief that the blood of such individuals is deprived of the power of coagulation. The lesions of the several organs of the body remarked in this form of sudden death are passed in review, and the authors proceed next to discuss the prognosis and the statistics of cases of injury by lightning. They go much in detail in examining the question of statistics, and discuss the influence of sex, of stature, of physical constitution, and of a perspiring skin, and that of the relative position which several persons may occupy during a storm, &c. As might be conjectured, the number of men struck greatly exceeds that of women. This is attributable largely to the greater exposure of men in the open air to storms. Thus, of 206 persons engaged out-doors, victims to lightning, 169 were males, and only 37 females. On the contrary, among those struck whilst within houses, the proportion of the two sexes is equal. Again, statistics go to show that it is a misfortune in a thunderstorm *to be a tall, bulky individual,* inasmuch as your stature is a predisposing cause to the descent of the lightning current through your body.

The effects of lightning on the clothes of those struck is the

subject of the next chapter. It is curious to read the notices of cases on record, and to mark how oddly the effects of the lightning have been exhibited on the clothes of the injured: at one time, these have escaped unharmed; at another, have been perforated, as if by bullet-holes; at another, have been burnt or rent in various fashion and degree. M. Sestier is at a loss to explain the curious and contradictory-looking results, and is reduced simply to the position of a reporter.

The following chapter is occupied with the treatment of the accidents resulting from lighting, and is succeeded by the fifth and concluding part of the treatise, devoted to the history and description of lightning conductors and protectors. M. Méhu takes credit for much inquiry into the history of lightning conductors, and he certainly shows great perseverance in endeavouring to discover that such protectors in storms were known ages before Franklin. The ancient Temple of Jerusalem is dragged at large into the argument, and the wisdom of Solomon exalted as fruitful in the discovery of the conducting power of metals, and of the value of metallic pinnacles in protecting buildings against lightning. Such argumentation is ingenious, but we cannot consent to it as satisfactory. Such elucidation as M. Méhu has been able to throw upon the inventive tendencies of the ancients in the matter in question lessens not by one jot the merit of Franklin as an inventor; for the real inventor is not the man who fumbles obscurely with the facts falling within his grasp, and aborts in displaying them to the world, but rather the man who seizes securely upon them and holds them up to be realised distinctly by his fellow-men.

Though, as we started by remarking, the two bulky volumes on lightning might well be terrifying to the student on first taking them up, yet we believe the short notice of their contents now given will encourage some to undertake their perusal. They contain not only an extraordinary and unparalleled collection of facts regarding the subject treated of, but they are likewise well written and easily read; and, what is unusual in French books, the names of persons and places in England, though very numerous, are, for the most part, correctly spelt, and always intelligible.

ART. V.—*Clubfoot. Its Causes, Pathology, and Treatment.* Being the Essay to which the Jacksonian Prize for 1864, given by the Royal College of Surgeon, was awarded. By WILLIAM ADAMS, F.R.L.S., &c. Churchill, 1866. Pp. 414.

WE cannot pretend to review *in extensó* this treatise of Mr.

Adams on the subject which forms, to use the common term, his "speciality," and in which he is known as so distinguished an authority. The very distinction which he has obtained in it, and the success of his numerous works, are a better guarantee to the reader for the goodness of the book than any formal praise which we could give. We may, however, say that the book is well written, well got up and well illustrated, and will, no doubt, serve the turn of any one who wishes for a complete manual on the subject.

We will select one or two matters of practical importance as to which difference of opinion may still prevail. First, as to Mr. Barwell's attack on tenotomy and the tenotomists, and as to the "new method" which he proposes. Mr. Adams writes as follows:

"In the so-called method brought forward by Mr. Barwell, the feet are placed in an improved position, and so retained by means of tin splints, placed lengthwise down the leg, and a series of elastic india-rubber cords attached to the tin splints, and passing in different directions corresponding to the paralysed muscles. The foot and leg are previously covered with adhesive plaster, which doubtless adds to the general support afforded by the apparatus, and to some extent prevents excoriations and blisters from excessive pressure, which, however, is still described as occurring in some cases. By the use of these elastic cords, the author speaks of 'supplying an *anterior tibial muscle*' in one case, and a '*posterior tibial muscle*' in another, or a '*tendo Achillis*;' and in the same way many other muscles are supplied, so that the foot and leg are rigged like a ship. And, no doubt, a very useful compensation for paralysed muscles is often thus afforded; but that this can in any way be regarded as a *curative means* for the paralytic affection which Mr. Barwell describes as being 'the head and front of the offending,' has yet to be demonstrated. If it should prove to be a means of cure for paralysis, it would be a valuable addition to those at present so frequently employed with but very limited success; but if this cannot be proved, the new method seems to be as little deserving the unbounded praise bestowed upon it by Mr. Barwell as it can be shown to have any claim for novelty.

"On the latter point, I can state that the plan was certainly brought under my notice about ten years ago by Mr. Bigg, of Leicester Square, who had a great variety of ingeniously constructed instruments upon this plan, with vulcanized india-rubber cords attached by hooks, and passing in various directions, according to the deformity for which the apparatus was made—all distortions of the feet, knee-joint instruments, spinal instruments, &c. Some of these instruments for the treatment of clubfoot were adapted by Mr. Bigg to cases in St. George's Hospital, and he told me the chief difficulty was to regulate the pressure without producing sores. The constant pressure from the elastic force could not be borne by

the patient, and the plan of treatment was given up, only to reappear as '*my new method*' ten years later."—Pp. 41, 42.

This is a matter of some professional importance. Mr. Barwell has reissued his views in a second edition of a work designated 'Cure of Clubfoot without cutting Tendons,'* in which he expressly asserts that it is "a method of treatment which I have never found to fail, if proper perseverance be used." On the other hand, Mr. Adams, and other surgeons of much experience, assert that the method is very troublesome and uncertain, and, as a rule, can only succeed in the very slightest cases—cases in which cures without tenotomy had always been regarded as probable. The experience of the writer of this notice, though very limited, leads him to agree with the latter view. Having failed in several attempts to cure clubfoot by Mr. Barwell's method, he invited that gentleman's assistance in two cases. In one the deformity was temporarily removed, so that if a photograph had been then taken, it might have passed for cured; but it rapidly recurred, after the treatment had, with Mr. Barwell's consent, been suspended. In the other, no effect was produced except a succession of troublesome excoriations on the skin of the leg and foot. The conclusion to which the present writer was brought by his experience in the matter, was that Mr. Barwell's method was one which was too laborious, and too difficult for him to practise, even if successful in the hands of Mr. Barwell. With respect to the question of priority of invention, the charge of plagiarism so distinctly made by Mr. Adams (if we understand him aright, as inferring not only that Mr. Bigg made such instruments, but that Mr. Barwell knew it) will, no doubt, receive an answer.

Mr. Adams has occasion also to protest (p. 33) against the teaching of a more eminent surgical authority—Professor Syme, who has strongly advocated a system of treatment of clubfoot by tenotomy, and immediate restoration of the foot, even with active exercise of the limb in walking, within three days of the operation. Mr. Adams observes, and we have no doubt justly, that—

"In severe cases tenotomy alone will not enable us to restore the form of the foot, in consequence of the adapted shortening of the ligaments; and in slight cases the danger of non-union, or excessive elongation of the new connecting material, should deter any surgeon from adopting Mr. Syme's recommendation."

In a subsequent part of the volume (p. 345) Mr. Adams alleges

* See a notice of this work in our number for July last, p. 23.

that in paralytic equinas this method of treatment after division of the tendo Achillis is very liable to produce talipes calcaneus.

We may conclude these extracts from Mr. Adams's work by a reference to some very interesting cases and observations (pp. 228—232) referring to wound of the posterior tibial artery in the operation for dividing the neighbouring tendon, and to the consequences of such a wound. These consequences are generally unimportant, but in some cases a traumatic aneurysm will form. In one such case, where there had been considerable arterial bleeding from sloughing of the skin over the aneurysmal swelling, Mr. Adams effected a cure of the aneurysm by injecting from 5 to 10 drops of the concentrated solution of perchloride of iron into its centre—compression being maintained on the femoral artery. Mr. Adams believes this to be the only instance of cure of aneurysm by injection of perchloride of iron in this country up to the present time. His observations on the use of this agent, so as to obtain its coagulating without its caustic effects, are well worthy of attention.

ART. VI.—*On Surgical Diseases of Women.* By I. BAKER BROWN, &c. Third Edition. Hardwick, 1866. Pp. 336.

THIS third edition of a well-known book does not call for any long notice at our hands. The profession is already well enough acquainted with Mr. Baker Brown's opinions and actions to render any notice from us superfluous. The "clitoridectomy" controversy has thrown such an atmosphere of disrepute, not to say disgust, on the subject, that many readers might perhaps be inclined to regard Mr. Brown's book with somewhat the same feelings as are generally entertained towards the doings which are believed to go on in the London Surgical Home. This, however, would, we think, be a mistake. There are few persons now living to whom the operative treatment of diseases of the female organs owes more than to Mr. Baker Brown; and this volume is full of instructive details on the subject of these operations. The drawings, executed by Mr. Brown's son, are in general very clear and correct; and those persons interested in the subject who have not already done so, can hardly fail to derive benefit from studying Mr. Brown's work.

ART. VII.—*Lessons in Elementary Chemistry.* By H. E. ROSCOE, B.A., F.R.S., Professor of Chemistry in Owen's College, Manchester. *London:* Macmillan and Co. 1866. Pp. viii, 398.

WE can speak in terms of unqualified praise of this little book. It is written carefully; the language is simple, and yet accurately conveys the intended meaning; the data afforded are precise, without being too elaborately wrought. In fact, Dr. Roscoe has found out what to leave unsaid as well as what to say: moreover, what he says is said well. The book is of small size, well printed and illustrated, and is issued at a moderate price. A beautiful and correct chromo-lithograph of the spectra of various metallic elements and of the sun forms an appropriate frontispiece to this compact introductory book on modern chemistry; while at the end of the volume numerous useful questions are inserted on the forty-one lessons in which are detailed the various facts of the science. A full index concludes the book.

A few words as to the plan of Dr. Roscoe's lessons. The student is first instructed as to the nature of chemical science; such words as "element," "compound," are explained. Then he learns what is meant by chemical actions, included under the terms "composition," "decomposition," "reaction." With the chief facts about the number and the distribution of the elements in the mass of the earth, the first lesson concludes. The second lesson is devoted to oxygen, ozone, and hydrogen; the history of these gases, with their properties and functions, being clearly explained and illustrated. One of the most important lessons in the whole work is the third, on the "Physical Properties of Gases." To this subject Dr. Roscoe has devoted much attention. He very properly lays great stress on the necessity for a clear apprehension of the chief laws regarding the expansion of gases by heat, and the relation of the volume of gases to heat and pressure. The observations on the thermometric scales and the measurement of temperature are clear, and will prove most useful to the beginner. It is unnecessary to refer in detail to each one of the remaining lessons. We will merely indicate, here and there, a lucid explanation, or an important or effective illustration.

No student reading pp. 36—41 need fail to learn what is meant by the terms "latent heat," "ebullition," "tension of aqueous vapour," "thermal unit," &c. Similarly, in Lesson V the constitution and properties of the atmosphere are explained in a concise and most satisfactory way. When speaking of

ammonia, in Lesson VII, one of the characters of the aqueous solution of this gas is well shown by a brief description of M. Carré's machine for freezing water. Other important pieces of apparatus—the Davy lamp and the Bunsen gas-burner, for example—are also duly illustrated and explained in suitable parts of the work under review.

Occasionally we find an important process or material of the useful arts noticed and made to serve as an illustrative example. The bleaching action of sulphurous acid, the manufacture of steel, the production of copper from copper pyrites, and the manufacture of malleable platinum, may be cited as instances in point.

Lessons on organic compounds, animal and vegetable nutrition, organic and spectrum analysis, conclude the volume.

It is scarcely necessary to add that the new notation, which may now be said to have acquired a considerable degree of certainty even on some long-contested points, is adopted by Dr. Roscoe. This book of his will greatly advance the accurate knowledge of modern chemistry, and it has already established for itself a wide reputation as the best text-book of chemistry for the beginner.

ART. VIII.—*Om Pathogenesen af Asthma hos Voxne.* Af Dr. J. C. LEHMANN. Særskilt Aftryk af 'Bibliothek for Læger,' 5 R. xiii. 1866.

On the Pathogenesis of Asthma in the Adult. By Dr. J. C. LEHMANN. Reprint from the 'Bibliothek for Læger,' 5th Series, xiii. 1866. 8vo. Pp. 66.

THE author, from a comparison of the results of clinical experience with those of physiological experiments, finds reason to adopt the opinion commonly entertained, that at least many cases of nervous asthma are due to an abnormal state of irritation in the nervus vagus. But it is not so easy to point out the source of this irritation. It is possible that future researches may lead to the discovery of changes in the substance of the nerve itself.

"In other cases the abnormal function must be assumed to depend on a change external to the nerve, acting as an irritant upon the latter, either upon its peripheric terminal expansions, or upon the trunk directly from the place where it emerges from the nervous centre, and until it is resolved into its terminal branches: but even in this case it must be supposed that the nerve itself is the seat of

a physical or chemical change which makes it unusually susceptible of the external influence; an hypothesis the necessity of which follows from the fact above alluded to, that the same irritant is by no means always followed by the change in question in the function of the nerve, as well as from the circumstance that these two elements, properly speaking, never stand in any definite proportion to one another."—P. 48.

Duchenne has shown, by experiments both upon animals and men, that irritation of the phrenic nerves produces effects similar to those caused by irritation of the vagus.

Dr. Lehmann concludes his work, in which he has in a brief space concentrated the results of much careful research, in the following words, which may be taken as presenting a summary of his views:

"We have thus, in the preceding pages, seen that the idea 'nervous asthma' handed down to us from ancient times may vary extremely, as asthma in most instances depends upon a spastic affection of the respiratory muscles, in rarer cases upon paralysis of the same, while it is doubtful whether it is ever produced by spasmodic contraction of the bronchi; and as we moreover cannot assume that the intermittent dyspnœa so produced, or any other nervous phenomenon, ever occurs as an 'idiopathic or essential' affection, but must, on the contrary, always be connected with an organic change in the nervous system itself or external to the latter, so neither can we consider asthma as a disease proper, but only as a symptom which may have very different starting-points. It is very interesting that the great advance which practical medicine has of late years made under the influence of physiology and histology compels us, on this question at least, in one respect to revert to the same standpoint at which we had already many decades before arrived, at a time when science, under the vigorous development of microscopic pathological anatomy and the discovery of auscultation, had made a progress which, for its magnitude, may be compared to that in the course of which we at present find ourselves."—P. 66.

In these observations we cannot quite go with the author. It is difficult to conceive that the phenomena of asthma should in any case be due to *paralysis* of the great respiratory muscles; on the other hand, the physical signs accompanying a paroxysm of the affection appear to us to be strongly in favour of the existence of spasmodic contraction of the bronchi.

ART. IX.—*De l'Ovariotomie.* Par E. KŒBERLÉ. *Paris,* 1865.

On Ovariotomy. By E. KŒBERLÉ. With Illustrations.

Opérations d'Ovariotomie. Par E. KŒBERLÉ. Avec six Planches lithographiées. *Paris,* 1865.

PROFESSOR KŒBERLÉ is one of the apostles of ovariotomy in France: he has not only preached, but practised it, and that with great success, so that no wonder he feels some pride in detailing his experiences in a country where this operation has been so much opposed and so tardily recognised. No doubt, too, the Strasbourg Professor feels a secret gratification that, while in Paris the cures have been 1 in 6, in the provinces half the operations have been successful, and that his own cases furnish three fourths of the recoveries.[1] Indeed, these results have been attributed by the faculty at Paris and elsewhere as due to "the favorable air of Strasbourg," and M. Kœberlé feels bound, in consequence, to observe in his preface, that his operations have been performed in an establishment very far from hygienic perfection in its arrangements, and with very unhealthy surroundings, having a prison, a public slaughter-house, and some damp barracks within 100 yards, and being situated in the midst of a thickly populated district.

The volumes whose titles we have given above consist of a treatise on ovariotomy, giving its history, statistics, and the arguments for and against, and of a narration of operations, with remarks. The cases are detailed at some length, and have the advantage of some good lithographs, showing the method of operation, the instruments used, and the appearances of the tumours when removed.

M. Kœberlé remarks—

"Ovariotomy is a conquest of modern surgery. Some years back, the extirpation of ovarian tumours was regarded as a frightful operation. Discarded for a long time, it has nevertheless ended by establishing itself gradually in France since the impulse given by the success of my operations. Whilst this operation has been finally accepted and commonly practised in England and America, Continental practitioners still hesitate to adopt or advise it."

According to this author's statistics, but 54 ovariotomies had been performed in France up to June, 1864, of which 32 died.

"Two of those operated on, and who were on the way to recovery, died from tetanus; and in another case the disease returned in the opposite ovary. In three cases the diagnosis was erroneous (*i. e.* in

[1] 'De l'Ovariotomie,' p. 33.

6 per cent.), and in one operation only an exploratory incision was made. Deducting my operations, there remain 42 cases, of which 12 were cured, making the cures 1 in 4. Of 20 operations performed in Paris and the suburbs, only 3 succeeded, or 1 in 6. Of 34 operations in the provinces, 16 failed and 17 recovered; while the patient subjected only to an exploration also recovered.

"At Strasbourg there have been 18 operations, 12 of which were performed by myself, of which 9 recovered."

In enumerating the causes of death, the author considers that in cases which sink from the immediate consequences of the operation, death generally occurs on the third or fourth day, the usual cause being peritonitis or purulent absorption. After the fourth or fifth day the liability to death is much diminished, such a result being then due to some complication occurring in the course of the healing of the wound. As regards the age of the patients, M. Kœberlé says—

"It has varied from 16 to 68 years, but that the results have been more successful in cases under 35, as after that age the tumours are often cancerous, and the patients are less able to resist the shock of the operation and its consequences."

In his opinion, also, the manner of disposing of the pedicle has had a great influence on the result.

"When the pedicle has been left out, union by first intention has been obtained when there were no ligatures in the adhesions, and peritonitis is then rare. Peritonitis has been most frequent when the pedicle has been left in, although considerable success has followed the adoption of Dr. Tyler Smith's plan of abandoning the ligatures and leaving the pedicle in the abdominal cavity."

The application of perchloride of iron to the parts cut off by ligatures is also recommended as having a favorable influence.

In speaking of errors in diagnosis, M. Kœberlé observes that fibrous tumours of the uterus are most commonly the cause of mistake, and that he has known forty-five cases in which this has occurred. It appears that the greater success of capital operations in England has been explained by some foreign writers on the theory that the English enjoy on the score of race a sort of physiological immunity from the accidents attending such proceedings; and M. Chauffard hazarded ('Gaz. Hebd. de Méd. et de Chirurg.,' 1863, p. 714) the suggestion—

"That the English operated on in France would recover with the same good luck as they do at home, and that the French operated on in England would meet with the same accidents which occur with us and spoil the success of the best-conducted operations."

But, says our author, the success met with latterly in the

provinces in France is enough to refute these mischievous assertions, and we are inclined to agree with him in thinking that many of the operations have been attended by fatal results from the rash proceedings of the operators; such, for instance, as that of M. Heyfelder, "who lost three patients from hæmorrhage because he contented himself with torsion of the arteries."

The ordinary arguments *pro* and *con.* are very fairly stated, and the author discusses the English statistics in a way that shows he is well acquainted with all the details of the subject. As these points, however, have been so lately referred to in this Review, we need not dwell longer on them here.

In France, it appears that so long ago as 1753 Delaporte proposed this operation to the Academy of Surgery, and that Morand also supported him. De Haen, however, writing in 1761, opposed it, dwelling on the difficulties and on the uncertainties of diagnosis.

Lassus mentions having seen a girl, æt. 18, in whom a surgeon had removed, *by mistake,* an ovary protruded through the inguinal ring, and who recovered perfectly.

In 1781, Laumonier, of Rouen, performed his "ovariotomy," since so celebrated, though not properly deserving the title; but this was classed as one of the curiosities of surgery, and was not followed up.

Some years later (1798), Chambon openly declared himself in favour of the operation, and made this remarkable prediction, which time has not failed to fulfil:

> "We are persuaded that the time will come when this operation will be extended to a far greater number of cases than those for which we have proposed it, and that no difficulty will be made in putting it in practice."

Escher, in 1808, in a thesis published at Montpellier, also proposed a plan for the operation. These proposals were, however, considered too rash and dangerous for adoption; and, though in England and America many cases had proved successful, it was not till 1844 that ovariotomy reappeared in France, "by a sort of accident;" a country doctor, named Woyeikowski, having complied with the request of a patient to remove a tumour, the nature of which was uncertain.

This case was followed by another successful one in 1847, in the practice of M. Vaullegeard, of Condé-sur-Noireau.

Between 1852 and 1862, eight more cases, all *un*successful, are recorded; and, till the tide turned in favour of M. Kœberlé's operations in 1862, ovariotomy had fallen into discredit, and was, indeed, proscribed by the French Academy of Medicine only in 1858. Since then, ovariotomy has been per-

formed with success in India (1860) and in Russia (1863), as well as most other parts of the world, and may be said now to deserve the title given it by M. Kœberlé of "un des plus beaux titres de gloire de notre époque chirurgicale."

Of M. Kœberlé's twelve cases—the total till June, 1864—nine recovered, and three died. Several presented some grave complications. In five cases both ovaries were removed simultaneously, and once the uterus as well, with success. The most remarkable case is related at p. 79, in his 'Opérations d'Ovariotomie,' and is headed "Double Ovariotomy: Extirpation of a Fibrous Tumour of the Uterus weighing upwards of fourteen pounds, and Amputation of the Sub-vaginal part of the Uterus."

The following is an abridged account of the case:

Madame S—, æt. 30, married for six years, had one miscarriage, but after that no pregnancy. A year after her marriage, she felt a hard, round tumour in the left of the pelvis, which was diagnosed by M. Stolz to be a fibrous tumour of the uterus. It was solid, but at points seemed to have a doubtful fluctuation, and turned freely on its axis under pressure. It was uncertain whether it was ovarian or uterine. On operating, the tumour was found to be connected with the uterus by a narrow pedicle, and was removed easily; but another small one was found in the substance of the uterus, and the left ovary was diseased and enlarged. The operator did not hesitate to remove the body of the uterus with both ovaries and appendages, the proceedings occupying an hour and a half. The patient recovered in a short time, and continued in good health and free from suffering. The large tumour weighed fourteen pounds, had the ordinary fibrous structure, and measured about 13 inches across, 6½ in thickness and 9½ in length.

M. Kœberlé has collected all the published cases up to the dates of his *brochures*, in which the abdomen has been opened for the removal of uterine tumours with or without extirpation of the ovaries, and they amount to 50 in all. Of these the operation was completed in 36, of which 12 recovered and 24 died; while in 14, of which 9 lived and 5 died, the operation was left incomplete.

Such a mortality as two thirds of the cases shows that removal of uterine tumours by abdominal section is a graver operation than pure ovariotomy. M. Kœberlé has arrived at the following conclusions in connection with this subject:[1]

"1. Fibrous tumours of the uterus, when troublesome from their

[1] See 'Documents pour servir à l'Histoire de l'Extirpation des Tumeurs fibreuses de la Matrice par la méthode sus-pubienne.' Strasbourg, 1864.

size, position, or the complications they cause, or when, either as pediculated or interstitial, they threaten to shorten life, may be extirpated through the walls of the abdomen.

"2. When they have a narrow pedicle, they may be excised after ligature of the same. The operation, when well performed, is as serious as ovariotomy.

"3. When the ligature of the pedicle involves the body of the uterus, or when the latter is the seat of other fibrous tubercles, it should be amputated at the neck, or completely extirpated.

"The ovaries and tubes should be removed at the same time, because (1) they are no longer of any use; (2) their preservation, being a cause of periodical congestion, may give rise to complications; and (3) because the operation becomes easier and less dangerous.

"Tumours which are of considerable size and not very vascular may be reduced in volume by the partial enucleation of their contents. When the broad ligaments are not the seat of fibroid disease, the tumours may be removed with the uterus, after ligature of the vessels of the ligaments.

"4. The extirpation of fibrous tumours of the uterus is particularly indicated in young women, in whom the tumours form rapidly, seriously interfere with health, and tend to shorten life, or render it insupportable to the patients by the inconveniences and complications they produce."

All the cases are admirably narrated and criticised by the author, and we cannot but think that M. Kœberlé's experiences will stimulate others of his countrymen to imitate his operations, and raise ovariotomy in France to the same level of success it now enjoys in England and elsewhere.[1]

ART. X.—*Lessons in Elementary Physiology*. By THOMAS H. HUXLEY, LL.D., F.R.S. *London*, 1866, pp. 329.

THIS is a masterly little work, such as we have a right to expect from its author.

Though designed for school-teaching, it is well adapted for college-teaching, and for those who have left school and college who may be desirous of acquiring an exact elementary knowledge of the human frame and of its living functions, for it happily blends anatomy and physiology.

Its great peculiarity is that which is enunciated in the preface—the separation of the well-established and the essential from the doubtful and the unimportant portion of the vast mass of knowledge and opinion called human physiology, thereby

[1] A brief notice of Kœberlé's operations, quoted from the 'Gazette des Hôpitaux,' will be found in the "Quarterly Report on Surgery," by Mr. Chatto, for January last (see p. 266).

endeavouring, as Mr. Huxley remarks, to perform simply the part of a sieve, or, as we would add, the part of a winnow, to rid the grain of the chaff.

What enhances the value of the book, is the clear, concise, and easy style in which it is written; and what, we think, may recommend it even to the advanced physiologist, is, besides its logical arrangement, the many happily expressed views and illustrations of some of the more important and interesting phenomena of the vital economy.

Though it is not usually a practice to offer quotations from a work professedly elementary, we are tempted to make an exception to the rule in this instance, partly in proof of what we have stated above, but more for the sake of the information conveyed and impressed. The portion we select is that which in part is given under the head of "Subjective Sensations."

After relating some experiments in which single objects appear double, the author proceeds:

"Phenomena of this kind are not uncommonly called *delusions of the senses;* but there is no such thing as a fictitious or delusive sensation. A sensation must exist, to be a sensation; and if it exists, it is real, and not delusive. But the judgment we form respecting the causes and conditions of the sensations of which we are aware, are very often erroneous and delusive enough; and such judgments may be brought about in the domain of every sense, either by artificially contrived combinations of sensations, or by the influence of unusual conditions of the body itself. The latter give rise to what are called *subjective sensations.* Mankind would be subject to fewer delusions than they are, if they constantly bore in mind their liability to false judgments, due to unusual combinations, either artificial or natural, of true sensations. Men say, 'I felt,' 'I heard,' 'I saw,' such and such a thing, when, in ninety-nine cases out of a hundred, what they really mean is, that they judge that certain sensations of touch, hearing, or sight, of which they were conscious, were caused by such and such things.

"Many persons are liable to what may be called *auditory spectra* —music of various degrees of complexity sounding in their ears, without any external cause, while they are wide awake. I know not if other persons are similarly troubled, but in reading books written by persons with whom I am acquainted, I am sometimes tormented by hearing the words pronounced in the exact way in which those persons would utter them, any trick or peculiarity of voice or gesture being also very accurately reproduced. And I suppose that every one must have been startled at times by the extreme distinctness with which his thoughts have embodied themselves in apparent voices.

"The most wonderful exemplifications of the subjective sensation, however, are afforded by the organ of sight. Any one who has witnessed the sufferings of a man labouring under *delirium tremens*, from

the marvellous distinctness of his visions, which sometimes take the form of devils, sometimes of creeping animals, but almost always of something fearful or loathsome, will not doubt the intensity of subjective sensations in the domain of vision.

"But that illusive visions of great distinctness should appear, it is not necessary for the nervous system to be thus obviously deranged. People in the full possession of their faculties, and of high intelligence, may be subject to such appearances, for which no distinct cause can be assigned. The best illustration of this is the famous case of Mrs. A—, given by Sir David Brewster, in his 'Natural Magic.'"

Mr. Huxley, after describing its chief points, remarks—"What the senses testify is neither more nor less than the fact of their own affection;" well insisting that,

"As to the cause of that affection, they really say nothing, but leave the mind to form its own judgment on the matter. A hasty or superstitious person in Mrs. A—'s place would have formed a wrong judgment, and would have stood by it on the plea that 'she must believe her senses,' and 'would thus become a mine of ghost stories of the most excellently authenticated kind.'"

"Mrs. A—," he points out, "undoubtedly saw what she said she saw. The evidence of her eyes as to the existence of the apparitions, and of her ears to those of the voices, was in itself as perfectly trustworthy as their evidence would have been had the objects really existed; for there can be no doubt that exactly those parts of her retina which would have been affected by the image of a cat, and those parts of the auditory organ which would have been set vibrating by her husband's voice, were thrown into the same condition by some internal cause."

This curious subject reminds us of what our author says of education, concluding with the remark, worthy, as we think, of being ever kept in mind by parents and teachers, that

"The object of intellectual education is to create indissoluble associations of our ideas of things in the order and relation in which they occur in nature: that of moral education is to unite, as fixedly, the ideas of evil deeds with those of pain and degradation, and those of good actions with those of pleasure and nobleness."

ART. XI. *Hysteria. Remote Causes of Disease in general. Treatment of Disease by Tonic Agency. Local or Surgical Forms of Hysteria, &c.* Six Lectures delivered to the Students of St. Bartholomew's Hospital, 1866, by F. C. SKEY, F.R.S., &c. London: Longmans, 1867. Pp. 107.

WE cannot speak of this volume in terms of praise. Considered as a course of lectures to students, there is no doubt

that it would impress some useful truths on their minds; but we should have thought that Mr. Skey's position and reputation had been so high, that the publication of the lectures in a volume could hardly have added to them. The main points on which Mr. Skey lays stress are the numerous diseases which hysterical affections simulate, the way to distinguish the simulated from the real affection, and the uselessness of lowering treatment in non-inflammatory disease, which, on the contrary, must be treated by tonic remedies. Mr. Skey's method of enforcing these facts (facts which, in the present day, we must really be excused for believing to be rather elementary) consists in an exaggerated representation of the ignorance of others with respect to the theoretical part of the matter; in an account of his own share in altering the practice and theory of the profession on this head, which appears to us to be equally exaggerated; and in exaggerating the benefits of the method of treatment which he calls tonic, and which others would denominate stimulant. Now, we would not altogether deny that exaggeration has its uses in impressing the minds of persons in the position of students—young, often not very well educated, or habituated to close attention—and who require to be impressed by some striking statement, in the same way as the uneducated part of the public require the exaggerated style, which it is now the fashion to call "sensational," in their penny papers; so that we should not quarrel with Mr. Skey for having addressed his class in the style of these lectures, though we fear his practical directions would lead to a stimulant routine quite as hurtful, if not more so, than the routine of depletion which he denounces. But as the deliberate publication of a hospital surgeon, on retiring from hospital practice after an experience extending over all the period of an active life, we cannot help perusing this work with some feelings of shame. Mr. Skey appears to wish to persuade the profession that he is the originator of the revolution in medical opinion on the subject of depressing and stimulating treatment which has occurred in this generation—or rather, he represents himself as upholding the correct doctrines on this subject, in the face of a hostile majority of English practitioners. Expressions like the following occur too frequently in this book:—"I often marvel at the wonderful faith exhibited by medical men in the influence of these 'secretions'" (p. 8).—"Now, gentlemen, if you could see this picture as I have sketched it, you would shrink from the folly that prevails so largely in the profession on this matter" (p. 10).—"I have a very limited faith in the cathartic system. In this I know I am un-English; but I am content to live under this obloquy." (P. 14.)

Expressions such as these, which might be multiplied *usque ad nauseam*, imply a claim on Mr. Skey's part which, as far as our knowledge extends, is quite unfounded. Mr. Skey, like most practitioners of his age, has seen reason to modify the opinions of his youth. He has changed with the world, and the world with him: but the profession would certainly not have been aware of any special obligation to him in the matter, had he not been at the pains to tell us of it. Nor can we allow that Mr. Skey occupies in any sense the position of a witness against prevailing errors. He teaches, as nearly every one also now does, that there are many forms of local inflammation which are due to general weakness, and that these must be treated, not by general depletion, but by support and stimulants. To describe himself as "exposed to obloquy," as protesting against "the folly of the profession," and so forth, for such very common-place doctrine as this, may go down well enough with a class of students, but is hardly fit for publication to persons better informed as to the real opinions which are taught outside the walls of St. Bartholomew's. But Mr. Skey's object throughout seems to be to impress his hearers by startling statements. Thus we find that the success of Mr. Jones, of Jersey, as an operator, depended on the principle (which, of course, Mr. Skey says, was learnt from his own practice) of giving "every patient on whom he operated at least a pint of port wine on each of the two days following the operation" (p. 5). We are told that the liver, which medical men believe to be the centre and focus of all the maladies of the body, is, on the contrary, "the most inoffensive organ in the whole body" (p. 8)—more so than its neighbours the pancreas and spleen, we presume. Again, that "there is one disease only of the stomach in which pain follows the admission of food into it—and that occasionally only—viz. cancer" (p. 93). Many more examples of the originality of Mr. Skey's *medical* doctrines might, no doubt, be cited; but as to his *surgical* teaching, it seems to us much less novel. It consists in descriptions of the ordinary hysterical affections of the joints, spine, &c., and of the diagnostic marks which distinguish these affections from the real organic diseases of the same parts. These are the only valuable parts of the book, and they, although forcible and often very well expressed, have no claim whatever to originality; and the only difficulty lies in applying the rules. Mr. Skey gives many examples of consultations on the subject, in which, of course, he was right, and the other consultants wrong; *e. g.* a young lady with disease of the elbow (p. 100). At the consultation with "four surgeons of well-known eminence," Mr. Skey pointed out "the absence of local pain, the normal appearance of the joint,

the hysteric character of the patient, and the fact that she had not long before been the subject of a true hysteric paroxysm," in justification of his contending, in opposition to the majority of these eminent surgeons, that the affection was hysterical. But allowing the accuracy of the representation (and we must be excused for hinting that the most truthful of men do not always represent the opinions of others with perfect accuracy), it evidently shows that Mr. Skey had examined his patient with more care than the others; for, surely, there is no surgeon of eminence who requires to be taught that the above mentioned are the signs of hysteric affection of a joint?

As a whole, we must say that we regret the publication of this volume, as injurious to Mr. Skey's reputation. The benefit to the students cannot have been very great, if the lecturers on surgery at St. Bartholomew's do their duty, as we doubt not they do; while the damage to Mr. Skey's character from the open exhibition of arrogant and unfounded contempt of others, and equally unfounded exaltation of himself, can hardly fail to be considerable.

ART. XII.—*On the Congelation of Animals.* By JOHN DAVY, M.D., F.R.S., &c. [Reprint from the 'Proceedings of the Royal Society,' No. 86, 1863.]

DR. DAVY stated many years ago that the leech may be frozen without loss of life. Puget came recently to a different conclusion, and consequently Dr. Davy was led to repeat his observations. After making an extensive series of experiments, which we have only space to allude to, he finally agrees with Puget that complete congelation is fatal to animal life, but is inclined to differ with him as to the cause of this death from congelation. Puget attributes it to a change in the blood, and chiefly in the corpuscles. Davy, on the contrary, attributes it rather to the freezing of the organs, not excluding the blood, than to the freezing of the blood alone. For this view he states his reasons, asking why, if the crystalline lens and blood-corpuscles suffer and undergo appreciable change from congelation, as Puget allows, the brain and nerves are not to be supposed capable of like change. Dr. Davy found that muscular fibre, after being frozen, lost its striated character, and after a few hours became friable, and was deficient in coherence to a remarkable degree.

PART THIRD.

Original Communications.

Art. I.

Cases of Lithotomy in the Practice of the late Benjamin Henry Norgate, Esq. Communicated by E. Copeman, Esq., M.D., M.R.C.P., F.R.C.S.; Senior Physician to the Norfolk and Norwich Hospital.

At first sight it might seem foreign to the province of a physician to publish a paper on such a completely surgical subject as lithotomy; and some excuse may be expected in explanation of such an attempt. I hope, therefore, to be allowed to enter upon such reasons as may justify me in laying before the profession the experience of a locally well-known and successful lithotomist, although a man of retired habits, and whose professional deeds would never have been known more extensively unless a friend had interfered to perpetuate them.

In the first place I plead friendship for the deceased, and a desire to exhibit the practical value of his surgical life. Another reason for bringing his success before the public is, that he has faithfully recorded his cases so that they cannot be read and studied without benefit to those who have the opportunity of perusing them. Then, again, I have sincerely at heart the reputation of the noble hospital of our county; and Mr. Norgate, during many years, did full justice to the responsible position he held as one of the surgeons to that institution. If to these reasons be added the fact that I have been more or less connected with the Norfolk and Norwich Hospital nearly forty years, and have witnessed, perhaps, as many operations for lithotomy as almost any medical man living, sufficient excuse may be allowed me for writing upon a subject which, although full of interest to myself, might seem inappropriate in the hands of a physician.

I purposely avoid making remarks about the particular mode of operating by different individuals; but I may state that the operation generally performed at the Norfolk and Norwich Hospital has been the *lateral*, and until within a few years the partially blunt gorget of Martineau was in general use. Occasionally of late the *median* operation has been adopted; but the success of this in the hospital has not been equal to the old method. If I may be permitted to venture an opinion, which I do with great diffidence, I think the median operation admirably adapted for a *small* stone, and *for no other;* for I believe that, anatomically, there is greater

difficulty in extracting a stone, owing to the forceps being so immediately under the narrowest part of the arch of the pubes; and the result of some of the post-mortem examinations has been (what I scarcely ever remember to have seen in consequence of the *lateral* operation) infiltration of urine and purulent cellulitis *anterior* to the bladder, and immediately behind the pubic symphysis.

I trust these few introductory remarks will shield me from severe criticism for the work I have undertaken, and that the cases now published, speaking as they do for themselves, will add somewhat to the benefit of surgery in the particular department for which our district is, unfortunately, so notorious.

Case.

Case 1.—O. H—, a maltster, 71 years of age, a strong, hale old man, about 5 ft. 5 in. in height, complained of incessant incontinence of urine, unaccompanied by pain. He had had these symptoms about two years; and suspecting a calculus in the bladder, I sounded him and found one. Three weeks after admission I determined on operating, as he was willing and appeared to be in good health. The bladder was tolerably free from mucous secretion, and the prostate was not more enlarged than usual in a subject advanced in years. The operation was performed on the 3rd of May, 1832. I introduced a large deep-grooved staff, and in making the incisions, being perhaps *too* careful of wounding the rectum or the bulb, I was hardly free enough in cutting downwards, as I afterwards discovered the edge of the levator ani muscle obstructed the passage of the stone, which was about the size of a pigeon's egg, and weighed seven drachms and a half. This operation, I understand, from the beginning to the end, occupied nearly a quarter of an hour; however, there was no undue force applied, although the stone passed with some difficulty. I made uniform but constant traction, allowing time for the parts to dilate. There was no hæmorrhage. Three hours after the operation he had a slight rigor, but soon after passed a quantity of urine through the wound. At 4 p.m., five hours after the operation, reaction had taken place; pulse 108, and full; skin getting hot. At nine in the evening he was in a very satisfactory state; no pain; pulse 84; water freely escaping through the wound, but little tinged with blood; gentle moisture on the skin.

May 4th, 11 a.m.—Had three hours' sleep during the night without interruption; urine passing freely; pulse about 80; no tenderness. 9 p.m.—Has taken plenty of milk-broth and tea, and is in a most satisfactory state.

5th, 11 a.m.—Continues free from any unpleasant symptoms; mind cheerful; with full expectation of getting well.

6th.—Bowels relieved by castor-oil; perfectly free from pain; to have solid food.

He continued daily to improve. On the fourteenth day after the operation I allowed him to get up, as he then passed the greater part of his urine through the natural passage, and the wound was nearly cicatrized. On the 24th he was quite well, and was discharged cured.

Case 2.—R. E—, aged seven years, from the neighbourhood of Wisbeach, was sent to the hospital with symptoms of calculus. I ascertained the presence of a stone, and operated on him on Thursday, the 16th of August, 1832, the boy being in good health, and only suffering from the effects of the irritation in his bladder. The operation passed off well. Stone weighed two drachms. At 9 p.m. I found he had passed urine freely through the wound, and had some comfortable sleep during the day.

Friday, a.m.—Had a good night; no tenderness in abdomen; urine free. Evening—is cheerful, and cannot possibly be in a better condition.

Saturday.—Has passed the greater part of his urine through the penis; wound nearly closed.

Sunday.—All the urine through the natural passage, and wound closed. Patient, in truth, well. Discharged one week after the operation.

Case 3.—J. C—, aged six years, from the parish of Gunthorpe, was admitted into the hospital on the 16th of September, 1832, with stone in the bladder. He was a stout, ruddy-faced boy; had his symptoms three years. On sounding him I discovered a large and rough stone, but his symptoms were not excessive. I kept him a fortnight, to accustom him to the regimen and the new faces, and operated on the 4th of October. I used my usual apparatus and Martineau's smallest gorget. The external incision should have been freer; the bladder was capacious, and did not contract readily; the stone fell into a pouch behind the neck, and I passed the forceps over it for some time without being able to grasp it; it was, however, extracted without any dilatation, by passing one finger into the rectum and raising it. (*Mem.*—Good plan.) There was a gush of blood at the time of making the incision, which rather appalled me; but it ceased before the calculus was extracted, and I had no trouble with it. At 7 p.m. he was perfectly easy; had no bleeding; but small quantity of urine passed. Next morning it flowed much more freely; a small coagulum passed from the bladder, which gave him a little pain.

6th.—On the third day water flowing clear and copiously through the wound; altogether in a most satisfactory state. Calculus weighed two drachms, and was rough; principally uric acid. He did not pass urine through the penis until the eleventh day, when he also voided

a lumbricus from the bowels, which had been rather confined, and his abdomen had been somewhat tympanitic. I now kept him under an alterative treatment with Hyd. c. Cret. and Pulv. Rhei for a few days. He passed no more worms; the wound continued to heal slowly; water came through it until the 19th, a fortnight after the operation. He was discharged quite well on the 26th, three weeks after the operation.

Case 4.—J. R—, a stout, ruddy yeoman, 54 years of age, from the neighbourhood of Aylsham, was admitted into the hospital with symptoms of stone in the bladder. He had first complained of sudden difficulty in making water two years ago; and on sounding him I felt a stone, which appeared to me to be small. On the 6th of February I performed the operation in the usual method, and removed four rounded, compact calculi, one of which was broken in the extraction. I commenced the incision rather lower than on previous occasions, and found the perineum deep, my finger but just entering the prostate after its division. By cutting low I avoided the artery of the bulb, and at the same time took the precaution to leave the rectum untouched. There was no hæmorrhage. The patient bore the operation well. In the evening he had been asleep, and water was passing through the wound. Pulse 72, and good; scarcely any constitutional disturbance.

Feb. 7, 11 a.m.—Had a comfortable night, pulse 68; water passing freely through the wound; no pain nor fever.

8th.—Improving, very cool; pulse good, urine free.

9th.—Had two motions from some castor-oil; wound healthy.

18th.—Twelve days after the operation, passed the greater part of his urine through the penis, the wound being nearly healed. On the 19th he passed all his urine right, and got up. Sixteen days after the operation he was well.

Case 5.—J. W—, æt. 64, an apparently healthy countryman, was admitted into the hospital on the 21st of February, 1833, from Elmham, where he had resided the greater part of his life. His symptoms of stone dated about three years, his pain was at the neck of the bladder, and his urine, though acid, was loaded with mucus mixed with pus. I kept him three weeks under regimen, giving the tincture of buchu, which appears to allay chronic inflammation of the mucous membranes. He took the common diet, and occasional doses of castor-oil. From the irritation he suffered, and his being frequently obliged to pass water, I was led to expect a large calculus.

On the 14th of March I performed the operation; I made a freer external incision than I had hitherto done, but think I ought not to have *limited* myself so much at the prostatic section; I introduced large and long forceps, grasped the stone, but unluckily in

the long axis; I was unwilling to let it go, consequently was longer in the dilatation and extraction than usual. (*Mem.*—Always divide freely the lower edge of the *levator ani* muscle; it forms an insurmountable barrier to the free extraction of a stone if it be of any bulk.) In this instance, I was obliged to use the straight-beaked knife to divide this muscle, upon doing which the stone easily passed; it weighed three drachms, and was composed of uric acid externally. There was no hæmorrhage. At 9 p.m. I found he had passed urine freely through the wound, and was free from pain; pulse about 98.

Friday, 11 a.m.—Pulse 100, skin moderately warm; expresses himself as being easy. No tenderness in the abdomen. 9 p.m.—Rather feverish, tongue dryish, pulse fuller than in the morning, but *wets* profusely, and continues perfectly easy.

Saturday, 11 a.m.—Slight pain at times in the pubic region; pulse 100; skin rather hot; suffering from the usual symptomatic fever attendant on a capital operation; takes freely of milk broth, tea, and diluents. 9 p.m.—Feverish, but water continues to pass in considerable quantities.

Sunday, 11 a.m.—This morning early took ℥ss Ol. Ricini, which had not acted. An enema given, which produced two scanty evacuations; pulse not so good, intermitting *once* in 100 beats; no pain, but rather languid. 9 p.m.—Flushed, water free, body slightly tympanitic.

Monday, 11 a.m.—Two large motions from another dose of castor-oil; pulse better, but languid; complains of a cold and hoarseness. 1 p.m.—Three more motions, which reduced him much; pulse intermittent; took ʒj Tr. Kino. 6 p.m.—Bowels now quiet, pulse still irregular, but he complains of no pain; has rather an anxious countenance; to have pudding and broth. (Begins to feel fidgety.)

Tuesday, 11 a.m., 6th day.—Had a cup of broth, and feels better; pulse more regular; to have meat for dinner. 9 p.m.—Skin cool; pulse intermitting once in a minute; urine free, feels himself comfortable. From this time he continued to improve by allowing him *meat every day*, with a cup of broth morning and evening. He still passed urine through the wound, but on the thirteenth and fourteenth day he began to have the greater portion pass naturally, and on the seventeenth it all came through the penis. I then allowed him to sit up, and from this time considered him well. I feel satisfied that the intermission of the pulse arose not from the fever or inflammatory symptoms produced by the operation, but from the too powerfully depressing effect of the second dose of castor-oil, which was repeated too soon; luckily he had lost no blood at the operation, otherwise the result might have been fatal. Elderly men after a severe operation *will not* bear depletion; they have not the rallying power of youth, and if allowed to sink too

low are rarely capable of being restored. I discharged this man exactly *twenty-eight* days after the operation.

Case 6.—J. B—, a dark-haired, tall lad, æt. 17, from the parish of Marham, near Swaffham, was admitted into the hospital on the 24th of August, 1833, with stone in the bladder. A tailor by business; had complained of pain and difficulty in making water for twelve months past, which had gradually increased until the present time. I easily discovered the calculus, which appeared large and rough, lying towards the left side of the fundus of the bladder. I kept him on hospital diet, giving a dose of castor-oil twice a week; aud on Thursday, September 5th, performed the operation. I introduced my largest staff; no vessel of size was wounded; had a little difficulty in extracting the stone, as it proved of a round shape, of the oxalate of lime or mulberry species, and once or twice nearly slipped from the forceps; however, altogether the operation passed off much to my satisfaction. 9 p.m.—Urine free; slight fever; pulse about 98.

Friday, 11 a.m.—Urine passing, pulse about 108; rather more fever. 8 p.m.—Skin hot, pulse 116; no tenderness in the body.

Saturday.—Pulse 108; comfortable, and in no pain.

Monday.—Continues improving; on which day he presented me with a brace of partridges. God knows where he got them.

Wednesday, seventh day.—Feels very comfortable, bowels free; to have pudding and meat.

Thursday.—One week after the operation passed his water entirely through the penis; wound appears nearly closed. He was discharged cured on the 28th.

Case 7.—J. S—, a small, blear-eyed, rather feeble old man, æt. 66, has been living at Harleston for the last forty years. For rather more than two years he has had symptoms of stone, but was not sounded till lately. Admitted into the hospital October 25th, 1833, in consequence of the difficulty he experienced in passing his urine, the catheter being necessary; he has considerable pain in the bladder, the urine depositing a copious secretion of mucus; prostate rather large, even for an old man. Urine neutral. For the first fortnight it was necessary to use the catheter twice a day; the stone could always be felt, apparently large, of hard texture, and flattened. After having by medicine and rest quieted the irritability of the bladder, and otherwise prepared him, on Thursday, November 14th, I proceeded to perform the lateral operation, *commencing* my external incision one and a half inch anterior to the anus, a little to the left of the raphé, continuing it downwards between the ischium and and anus, *deep* into the perineum; again, by pressing on the rectum with my finger in the wound, cutting deeper through the transversus

and edge of levator ani, and laying open the urethra; and, lateralising the knife, cut on the groove through the prostate; introduced a blunt gorget as a director. An immense gush of urine followed this. I immediately introduced my finger; felt the prostate cut, and the stone in the bladder; introduced forceps, grasped the stone, and then came the difficulty: although obtained in the right axis, it was large, and required continued traction for some time, and the assistance of the probe-pointed knife to dilate the internal wound, before it could be extracted. It weighed two ounces, of hard compact *uric acid* texture externally. This operation must have lasted eight or ten minutes. He bore it most heroically. I perceived no bleeding at the time, but after he returned to bed there was considerable flow from the deep wound; this lasted some time, and I was obliged to *wind lint round a hollow elastic canula* and pass it through the wound into the bladder. For many hours I was very anxious about him; but the bleeding ceased, and at 6 p.m. I found him rallied, and water was flowing through the tube. Pulse 96, and steady; he had an opiate.

Friday, 8 a.m.—Passed a quiet night; urine free, but little pain, countenance cheerful. 3 p.m.—Slight pain in left groin. 9 p.m.—The pain remains, but all other symptoms satisfactory. Has taken broth, and sop in his tea.

Saturday, 11 a.m.—Comfortable night. Water free; complains of wind in the bowels; pulse under 100. I withdrew the lint and canula from the wound. Cheerful; tongue clean. 7 p.m.—Bowels rather tympanitic. To have an injection in the morning.

Sunday, 11 a.m.—Bowels not yet moved. To have pudding with his broth for dinner; pulse stronger, and altogether in a satisfactory state. 9 p.m.—Bowels not yet relieved; to have three drachms of castor-oil in the morning.

Monday, 11 a.m.—Cannot yet get the bowels open; face rather flushed; slight pain on left side of the body; pulse quicker, 108 and irregular. 8 p.m.—Has had four motions from injections and castor-oil, too much; rather hurried in mind, and restless.

Tuesday, 11 a.m.—Symptoms unfavorable: tongue dry; more delirious. To have some port wine and water, quinine in decoction of bark, and meat for dinner. 8 p.m.—A sensible difference from the effect of better diet; intellect clear, tongue moist, and pulse more regular. From this time I gave him more generous diet every day, which I believe made an immense improvement in his condition. *Mem.*—Old men will certainly not bear starving, or even much evacuations of any sort; they require good support nearly from the first.

Friday, ninth day.—Has had one motion spontaneously. Wound remains open, but looks well. To continue nourishing diet and quinine.

Thursday, 27th, a fortnight after the operation, continues hearty, slowly mending, urine still all through the wound. This old gentleman did not begin to pass his urine *per urethram* until the 19th of December, five weeks after the operation; during the following fortnight it all came through the natural passage, and he was discharged on the 4th of January, 1834.

Case 8.—H. M—, æt. 3½, was admitted into the hospital on the 28th of December, 1833, with well-marked symptoms of calculus in the bladder; they had existed for sixteen months, and latterly he had suffered much; the bowels had been attended to previous to admission. On the 2nd of January, 1834, five days after admission, I operated on him, removing a compact uric acid calculus, weighing two scruples fifteen grains. It was easily obtained, notwithstanding a troublesome prolapsus of the rectum, in about four minutes. In the evening he was comfortable, but courage gone; he had been very spirited.

Friday, 11 a.m.—All satisfactory. In the evening some urine passed through the penis; slight fever.

Saturday.—Has passed much urine the right way, and in the evening the entire quantity; wound healing by first intention. From this day, two days after the operation, it continued to pass *per penem*. Wound healthy and cicatrizing fast. Discharged cured, a fortnight after the operation.

Case 9.—J. A—, æt. 5, was admitted November 3rd, 1834, for stone in the bladder. He lived at Thornage; his symptoms had lasted about twelve months; he was small of his age, but healthy and in good condition; suffered from prolapsus ani when he strained much. I kept him a fortnight in the house to accustom him to new faces, occasionally giving opening medicine; and on Thursday, November 17th, operated: gained easy access to the bladder, but could not immediately seize the stone with the forceps, owing, I suppose, to the contraction of the bladder upon it. I, however, removed it with the scoop, with a finger in the rectum; it was composed of uric acid, and weighed half a drachm. In a few days the greater part of his urine flowed through the natural passage; opening medicine was administered once or twice during the convalescence; ten days after the operation every drop of water passed *per penem*, and on the following Saturday he was discharged.

Case 10.—W. C—, æt. 3, admitted November 29th, 1834, with stone in the bladder. He came from Wilts; was a ruddy-faced boy, and apparently in good health. His symptoms were not urgent, and had existed rather more than six months. On Thursday, December 10th, after having well purged him and given him an

injection, I proceeded to operate. I believe that I began my incision too low, for I missed the groove of the staff *at first*, and was consequently longer in making the proper opening into the neck of the bladder than was desirable. He lost some blood at the time, but not enough to alarm me. A stone weighing fifteen grains was extracted, and he was sent to bed. In the afternoon a bleeding was observed, which continued some hours. Mr. Copeman sat by his side, with his finger pressing on the ramus of the ischium, which, after some time, had the effect of stopping it. He altogether lost a considerable quantity for a child, and was much reduced by it; however, it was completely stopped, and he began to rally, when, unfortunately, another bad symptom arose. He had milk and tea given, but everything was rejected from his stomach for many hours, and in the course of the day after the operation he vomited two large lumbrici. His belly was tumid; he remained constantly vomiting; he was restless, and got no sleep during the night: these symptoms continued without any abatement, and he gradually sank, and died on the Saturday evening, fifty-eight hours after the operation. I verily believe that this child would have recovered, if he had not had the irritation in the system caused by the worms; for he had recovered somewhat from the hæmorrhage and shock of the operation, and his urine flowed abundantly through the wound. He had not evacuated a single worm previous to the operation, although he was copiously purged two or three times. I have known children languish and die in a similar way, after a most rapid operation, where there was an accumulation of lumbrici in the intestines.

Case 11.—W. S—, æt. 26, a stout, ruddy-faced yeoman, from Snetterton, near Quiddenham, admitted March 14, 1835, with stone in his bladder; he had occasionally experienced pain, more or less, for six years, but, to use his own expression, was not "hampered" until the preceding August, about six months previous to admission. On the first sounding, I found a large and very rough calculus, which I was surprised had not occasioned more urgent symptoms, his urine depositing but little mucus, and he having but little pain except on motion. He was in perfect health otherwise. I kept him a fortnight, occasionally passing an instrument, and relieving his bowels by castor-oil. On the 26th of March I performed the operation: found a fair-sized perineum, not very deep; prostate in a healthy state. Used my largest staff, and easily cut upon it, but was scarcely free enough in my incision through the muscular parts for so large a stone: I introduced the forceps, but could not immediately seize it in its right axis; it was lodged *on* the pubes towards the right side, in the position that I had always felt it; probably in some measure fixed by the muscular coat of the bladder, and rendered incapable of moving in that viscus, which might have

been the cause of its not producing more pain. It was, however, extracted with the assistance of a little dilatation of the edge of the levator ani muscle. A vessel at this time sprouted out rather freely, but it was easily secured. The stone weighed an ounce and seven and a half drachms—a mulberry, oxalate of lime calculus. He had an opiate, and in the evening I found him tolerably easy, complaining only of soreness from the urine, which was considerably coloured with blood, passing through the wound. Next morning, had an abundant secretion of water, many coagula passing from the bladder, which continued all the day, the sheet gradually becoming less stained. In the evening, his pulse got up and his face flushed, but still he was free from pain. On the following morning I found him in a most comfortable state; very large flow of urine, no pain, pulse about 90. On the third day he had castor-oil, and his bowels were relieved thereby: urine still abundant, body free from pain, pulse 90. On the fourth day, on examining the wound, I found fæces in it, which surprised me much, not having been in the least aware that the rectum had been wounded: I imagine, by this not appearing before, that the bowel must have suffered in the extraction of the stone, which pressed for a long period in the wound, and was exceedingly rough and nodulated on its surface. The discharge of fæces through the wound occasionally continued; but in all other respects he gained ground—his appetite increased, and all danger from the immediate consequences of the operation were at an end.

April 9th.—Thirteen days after the operation, at 3 o'clock p.m., I was summoned to my patient, who was reported to be bleeding. I was absent from home; Mr. Dalrymple saw him at 4 o'clock, and applied compresses. Notwithstanding, he lost at least 2lb. of blood; at one time he was supposed to be dying; wine and milk broth were administered. I saw him at 7, and introduced a canula surrounded with lint; this plugged the wound and made pressure on the surrounding parts. The urine flowed through, and I left him for the night, but felt considerable solicitude about him. No hæmorrhage supervened; I kept the canula in the wound for three days, and then withdrew it. Gave him some castor-oil, which purged him considerably; no bad symptom came on, and the wound looked clean and healthy. He passed some water *per urethram* on the twenty-first day. He continued to mend daily, passing more water naturally. His bowels acted of their own accord, and after the 21st of April I saw no feculent matter in the wound or in the urine. He was allowed to get up daily at the end of five weeks from the operation, his strength daily increasing. On the 2nd of May the wound was healed, and he was discharged the following week, just six weeks after the operation. This patient was a remarkably courageous man—never daunted even at the anticipation of the operation, nor at the danger he was in from bleeding. Being

naturally of a good constitution he recovered in a much shorter time than I had any reason to expect.

Case 12.—H. G—, æt. 8 years, from Sporle, near Swaffham. Had been suffering from symptoms of stone for four years, and latterly very severely. He was brought to the hospital on the 20th of July. I was obliged to keep him a month, *i. e.*, to the 20th of August, until I dare operate, as he suffered so much from paroxysms of pain and irritation at times. The first incision was, in this instance, begun too low in the perineum, and by mistake my assistant gave me a wrong knife, one an inch and a half shorter than that I am accustomed to operate with, which I was not aware of at the time. By beginning too low, all my incisions were unnecessarily deep, and I wounded the *rectum for the first time in my life.* The stone proved nearly globular, but very rough and large, weighing more than half an ounce, and remarkably nodulated; fully explaining the cause of the excessive irritation this poor boy had suffered. The opening in the bowel was extensive. An anodyne was given after putting him to bed; motion passed freely through the wound, mixed with urine, for the first four or five days; after which time he was in the habit of passing the greater part of his evacuation *per anum;* at the end of a week he passed a small quantity of urine naturally, but whenever it passed into the bowel it caused a looseness. He took astringents with anodynes, and at the end of the week good nourishing broth with eggs. This boy had suffered so much for the last four years from the irritation of the stone, that on its removal he was more capable of bearing the effects of this accidental wound in the rectum than if he had suffered less previously. His spirits were good, and at the end of ten days the wound granulated and looked very healthy. At the end of a fortnight his appetite was remarkably good. There were many changes in the case of this boy: at the end of three weeks he entirely lost his appetite; the wound had constantly fæcal matter passing through it. I allowed him three ounces of white wine with bark and cretaceous mixture, as his bowels were always loose; and under this regimen in ten days he entirely recruited, got up every day, could retain his urine three or four hours together; the wound in the perineum very nearly closed, he went out into the air every day, and at the expiration of *five weeks* after the operation he had lost that scowl indicative of long suffering, became quite fat, and I discharged him on the 14th of October. He still passed some urine through the rectum; the fistulous opening was anterior to the prostate. Altogether this was an interesting case, although not so satisfactory to my own mind as I could have wished.

Case 13.—W. A—, æt. 75, from Fakenham, was admitted

February 13th, 1836, suffering acutely from stone; his frame was emaciated, his features sharpened, pulse intermitting, and he was evidently in the last stage: however, he always refused to come to the hospital before. I sounded him in two days, and discovered two stones of considerable size. He told me that during the last twenty years he had voided fourteen calculi; his urine was sub-acid and tolerably clear, but generally running from him. He moaned dreadfully. I had him put to bed, and gave him anodynes; but in the course of seven days he died. On inspection I found both kidneys in an ulcerated state, particularly *on the cortical surface;* the capsules remarkably thickened, and containing pus in different places: the right pelvis contained a calculus of half a drachm; one or two cysts containing serum on the cortical structure; the lining membrane of the pelvis of each kidney highly vascular, and the right pelvis considerably enlarged. Ureter of same side enlarged; the bladder, which was firmly contracted, contained two calculi, each weighing three and a half drachms; they were white on the exterior, about the size of pigeons' eggs, one rather larger than the other, exteriorly of a uric acid character. The prostate was enlarged in its two lateral lobes; also the third lobe was large; the veins around the neck of the bladder and the prostate were very much distended; urethra otherwise in a healthy state. (No operation.)

Case 14.—A. W—, æt. 23, was admitted May 29th, 1836, with a large and rough calculus in the bladder. His symptoms were urgent, and of long duration—he says the greater part of his life; but he continued in farmer's service at Yaxham, near Dereham, until about a year ago, when he had an attack of inflammation of the chest, for which he was actively treated. This young man had a severe attack of inflammation of the bladder soon after coming to the hospital, for which I leeched him above the pubes, used a hip-bath, and gave him anodynes every night: his pulse was very easily excited. I did not consider him a favorable subject, but believed that all his suffering arose from the foreign body. After keeping him nine weeks in the hospital, the bladder became much less irritable; his pulse, however, was very excitable, arising in some measure from his excessive nervousness, frequently ranging to 120, and even more.

July 25th.—As his bladder was now capable of retaining urine for several hours, I determined on the operation, which was performed on Tuesday, July 26th. I made my incisions as usual, but had some difficulty in hitting the groove of the staff, owing in great measure to the careless manner in which the staff was held; several times I was obliged to rectify the position. I extracted a stone weighing fifteen drachms, oxalate of lime, and very ponderous in its structure. I considered it a favorable operation,

considering the size of the stone; an anodyne was given, and the patient sent to bed. At 4 p.m. I saw him, and finding he had not voided any water through the wound, I introduced an elastic canula, and eight ounces of bloody urine escaped. The tube was left in for two days. I consider it a good precaution always in adults to introduce a tube; it prevents infiltration about the neck of the bladder, should there be any obstruction from coagula or other causes. He gradually improved, and said he slept better after the operation than he had for weeks.

On the fifth day I found his left testis rather inflamed, and a slight flush on his cheek; however, the urine passed freely through the wound. The next day he had slight pain in the left groin; testis kept wet with lotion, and improving; no abdominal pain; bowels daily relieved, and passes about half his water through the penis. At the end of a week he had slight erythematous eruption on the nates and buttock, caused, I suppose, by the irritation of the urine; this, however, gave way to cold lotion, &c. At the expiration of sixteen days all his urine passed through the penis, and the wound was nearly healed. I discharged him cured five weeks after the operation; but his general health was feeble, his appetite very variable, and I had an impression on my mind that there was some lurking disease in the chest which would eventually prove fatal.

I heard afterwards that this man died of consumption in November.

Case 15.—J. A—, æt. 64. Had driven a team for years at Fransham, near Dereham; came to the hospital August 1st, 1836. Was a fine, healthy, hard-featured man, of very quiet temperament, complaining of stone in the bladder; had only suffered much for seven months, but the calculus must, from its size, have been forming for a much longer period. I gave him some gentle anodyne, with Liq. Potass., and allowed him the diet of the house for ten days. Operated on him August 11th: made my external incision rather more oblique and freer; found the prostate larger than I expected, but not to be of any consequence; introduced my second-sized forceps, but did not at first feel the stone with them; introduced a longer pair, and grasped the stone, which was of a flat oval shape, about the size of half-a-crown, lithic acid, and weighed half an ounce. He bore the operation manfully, and I was quite satisfied. No hæmorrhage. Immediately on his return to bed I introduced a canula, and left it in four days. In the afternoon I found his urine flowing freely, and the patient very little shocked by the operation. On the following day I found he had slept well, and he was in a most satisfactory state: pulse sometimes intermitting, but it was naturally irregular; and I have often experienced this state of pulse after a

severe operation in old men. He continued free from abdominal pain and sympathetic fever; I therefore desired the nurse to give him good broth, and by no means to let him down.

On the third day he had castor-oil, as he had had no relief, and it acted well.

Fifth day.—Entirely free from pain, and urine copious; takes his food well; pulse much more regular. The canula was removed from the wound when his bowels were relieved, and not replaced, as the water came so well. I never had a patient who seemed to suffer less from an operation of this sort. At the end of the week he was attacked with erysipelas on the left buttock, which extended considerably, and produced a great deal of irritation; his tongue became dry and brown; his pulse flagged, and I was alarmed: however, I applied tepid fomentations, gave him broths, and, at first, fever medicines, but very shortly afterwards Ammon. Carb. and Inf. Gent. c.; and, as the erysipelas became more dusky in its hue, half a pint of London porter in the day. He improved under this regimen, and at the end of a fortnight was considerably better. About this period one of his testicles swelled. I have often observed this circumstance in patients when the urine is about to flow through the natural passage. This symptom abated with the common treatment, and at the expiration of one month he got up; wound cicatrized, with the exception of one small fistulous opening that gave issue to a few drops of urine. I discharged him cured six weeks after the operation.

Case 16.—C. K—, æt. 69, admitted October 22, 1836, with stone in the bladder. He had lived at Barton Turf, and been a carpenter. He was a thin, feeble man, with considerable palsy of his limbs; had a rupture in the right groin, and varicocele of the left testis. His symptoms were of about one year's duration, and latterly urgent, causing him to rise eight or ten times in the night to make water. On sounding him, I found a capacious urethra and a movable calculus, the prostate gland slightly enlarged; the urine was free from mucus, and neutral. I performed lithotomy on the 10th of November, Mr. Crosse holding the staff, which circumstance much expedited my incisions; they were made in the usual way, and the opening into the bladder was small, but sufficiently large to introduce my longest forceps. I did not readily seize the stone, which fell into the pouch behind the prostate, the third lobe of which was enlarged; if I had used bent forceps, I should have found it at once. A finger was introduced into the rectum, which assisted me in getting hold of the stone; but a portion of the third lobe of the gland also got entangled in the forceps, and was brought away: however, I did not think this a matter of any consequence, having often seen it before. Stone weighed two and a half drachms, uric

acid externally. I was perfectly satisfied with this operation, knowing that there was no violence or bruising of parts; but between two and three hours after he had been put to bed, Mr. Goodwin was summoned to him, as a large coagulum had been expelled from the bladder, and active hæmorrhage was coming from its cavity. No blood was lost in this case at the time of the operation; if a large vessel had been wounded, it must have shown itself before. I conceived the hæmorrhage must have arisen from the ruptured surface of the prostate gland, or from some of the large veins which we always see in an old man's gland. I introduced a canula and stuffed lint by the sides; but the bleeding continued into the bladder, and once in about half an hour a coagulum was expelled. This continued for *five* hours, when I injected some *cold water* through the canula into the bladder. At this time the patient was cold and faint, and his pulse was very feeble. The injection appeared to have a good effect, as from this time, 7 o'clock p.m., there was no hæmorrhage; but another bad symptom came on, viz., obstinate vomiting; he could keep nothing on his stomach for forty-eight hours. I tried every means I could think of—solid opium, brandy, wine, effervescing draughts, &c. He was becoming at last so weak, that on Saturday I injected some strong broth, with Liq. Opii Sed., into the bowel two or three times, which was retained. His tongue was now brown and dry, pulse 100; he suffered no pain, the urine passed freely, and there had been no return of bleeding. On Saturday evening he retained a little on his stomach, and rather more on Sunday and Monday, but I still entertained great doubts of his recovery. Tuesday, fifth day, pulse 84, very feeble; tongue dry and rough. No abdominal pain, and urine free through the wound. His sickness had ceased, but he could take so very little that I was in constant fear of his sinking from want of nourishment. I now allowed him beer, three ounces of wine, and he tried a small piece of meat. His bowels were relieved slightly by an injection on the Thursday, just *one week* after the operation; but as I knew he could not bear the least depletion, I was fearful of giving him opening medicine.

Friday, eighth day.—Rather better: to have two grains of quinine three times a day; took four ounces of port wine and a pint of good strong broth.

Sunday.—Eats a piece of meat every day; enjoys his broth, and wine and beer. Wound looks very sloughy and open; plentiful urinary secretion.

Thursday, fourteenth day.—To have a pint of porter instead of beer in the day; edges of the wound beginning to separate, and healthy granulating surface appearing.

Tuesday, 19th—Passes a little urine through the penis; wound granulating.

December 14th.—Wound filling up gradually; his strength improving. He was discharged on the 27th in as good a state of bodily health as when admitted, and without a stone in his bladder.

Case 17.—John N—, æt. 72, from Southrepps, was admitted May 20th, 1837, with stone in the bladder; he was a small man, five feet three inches, but healthy. After having prepared him in the usual manner by castor-oil the evening before, and an injection in the morning, I proceeded on the 25th to operate. He was so rheumatic in his joints, that we had great difficulty in tying him up, not being able to separate his legs; this proved a cause of great inconvenience to me, as his feet were continually over my hand. Having cut upon the staff, I continued the incision along the membranous parts, but, as I found, not far enough; for on introducing my finger on the gorget, I found there was not room enough for me to introduce the forceps into the bladder. As I had removed the staff, I had some difficulty in reintroducing it, as the urethra had been laid open; however, after a little manipulation, the orifice of the bladder was found, and I then continued my incision freely through the prostate, cutting laterally. I found the gland much more resisting and gristly than I anticipated, although I was aware that it was enlarged. The calculus, weighing two drachms and a half, was easily gained and extracted. There was active hæmorrhage from some small vessel for a little time, but it was entirely stopped by a canula wound round with lint introduced into the bladder. In two hours the urine began to flow through the tube; he was slightly sick, which I attribute to the pressure of the tube on the neck of the bladder. Eight hours after the operation he was in a comfortable state: pulse 96; no bleeding.

Friday, 26th.—In a good state: abundant micturition; stomach settled.

Saturday, 27th.—In a very satisfactory state. I removed the tube; urinates freely, and bowels relieved. At the end of ten days he began to pass some of his water through the penis, and on the 13th it all came so. He was discharged cured exactly one month from the operation.

Case 18.—H. H—, agricultural labourer, æt. 66, a strong, hale, light-complexioned man, from Matlock, was admitted on the 20th of May with stone in the bladder. He had passed gravel for several years, but during the last three he had seen none, and his pain in voiding urine had increased from that date. His sufferings lately had been very acute, and on sounding him I found a large stone. Prostate of natural size; bladder not very irritable; urine slightly acid, and depositing very little mucus. I operated on the 1st of June; introduced my largest staff, made an ample incision, and con-

tinued cutting low in the wound until I laid bare the groove of the staff in the membranous part of the urethra. In making these incisions the transverse artery of the perineum was divided, and spouted out most furiously; however, I did not stop, but continued my cut through the prostate, introduced gorget and forceps, and with little difficulty extracted a large, oblong heavy calculus, about two inches long by one and a half broad, and weighing two ounces six drachms. I immediately proceeded to put a ligature on the bleeding vessel, which was easily done, and the man conveyed to bed. He bore the operation remarkably well. After he was placed in bed, I introduced a gum elastic tube through the wound into the bladder, through which his water continued to flow in great abundance. Next day, Friday, urine plentiful; he complained of flatulence, and I gave him an opiate in mint-water at night.

Saturday.—Going on well. I removed the canula.

Sunday.—Gave him castor-oil and an injection, but neither operated, so I was obliged on Monday to give him more oil and injections, and at last they acted rather too much, which exhausted him; therefore the next day I gave him good meat broth, pudding, and an egg. I am always fearful of reducing old men after this operation, as they have much to contend against, and if they once sink below a certain point it is very difficult to restore the strength.

On Thursday, the eighth day, the integuments of the sacrum became slightly excoriated from pressure, and being constantly wet with urine; but by placing him on his side and oiling the part, the redness disappeared. At the end of three weeks all the urine passed *per urethram*. He was allowed meat and a pint of porter every day, and was discharged cured at the end of the month.

Case 19.—G. M—, æt. 7, from Sculthorpe, was admitted July 23rd, with stone in the bladder; he was small of his age, of ruddy complexion, and muscular. His father told me he had suffered more or less ever since he was an infant, but that he had never been examined. I kept him until Thursday, the 3rd of August, and then operated, having previously kept his bowels in good order. I operated as usual, and removed three roundish stones, about the size of the common nut, flattened where they were in contact. On Sunday, I observed a small clot of blood in the wound, and another on the sheet, tinging the urine; face flushed, but he was not restless. At noon the nurse observed more blood, which now began to flow freely, and in my absence Mr. Johnson introduced a canula armed with lint; at this time he had lost seven or eight ounces of blood, was blanched, pulse 150. I saw him at 6 p.m.; found him very faint, but the bleeding ceased from this time. He was quiet all night; but on Monday morning I found the canula had been pro-

pelled from the bladder in his efforts to make water; as there was no hæmorrhage, I did not again introduce it; he now took tea and milk, and the flow of urine was clear and abundant through the wound. All went on well, and on the fourteenth day he began to pass the urine through the urethra; it did not all come the right way until one month after the operation, soon after which he was discharged cured.

Case 20.—J. W—, æt. 3, a stout, ruddy, muscular boy, admitted October 12th, 1837, with stone in the bladder. He had suffered more or less ever since he was weaned. He came from Burgh, near Yarmouth, and neither father nor mother suffered from gravel. He made water almost every quarter of an hour, when he always cried, with much apparent suffering. I always felt the stone in the fore part of the bladder, and I thought generally on the concave side of the instrument. He passed one lumbricus a few days after admission; I purged him three or four times with compound scammony powder, but he voided no more. On Thursday, October 26th, I operated: used my smallest staff, which I had some difficulty in introducing, owing to the great spasm on the bladder trying to expel it: the stone lay at the posterior fundus, and I was some time in feeling it; but it was readily extracted when seized, and weighed one drachm, lithic acid. I never saw a boy who appeared to care less about an operation; when I went into the ward afterwards he was laughing. In the evening I found he had slept quietly, and passed a considerable quantity of urine, very little stained with blood. On Tuesday, November 7th, ten days after the operation, he passed all the water through the urethra, and the wound healed. Discharged three weeks after the operation.

Case 21.—M. B—, æt. 57, from Dilham, admitted March 27th, 1838, with stone in the bladder. He was a healthy, spare man, with plenty of resolution. His symptoms had been of about a year's duration. He had voided some calculi twelve months previously. On sounding him I thought I felt more than one, but I believed them to be too large to pass *per urethram*. There was a question raised whether lithotrity should not be performed in this case; but both my patient and myself in this instance preferred lithotomy, which was performed on the 12th of April. Having introduced my large staff, I had some difficulty in detecting the calculus, the bladder being full: I began my incision an inch above the anus, and continued it downwards, midway between the raphé and ramus of the pubes and ischium; I cut deep here, and then felt for the staff under the symphysis, entered the point of my knife into the membranous part of the urethra, and continued it laterally through the prostate; introduced gorget and forceps. I observed

the transverse artery of the perineum to bleed furiously at the time of its division, but did not stop to secure it. I removed a small stone, smooth on its surface; tried again, and removed another, also a third and a fourth, which last was fractured; and then tried flat forceps, and removed many portions of a stone which was crushed to pieces. I could not fish out any more pieces, although Mr. Johnson's long finger detected portions just beyond the prostate; I syringed the bladder and tried with the scoop, but none appeared. The patient was unbound, and we then secured the bleeding vessels, two in number, and sent him to bed. The operation was necessarily prolonged, but he was not much exhausted. Three hours after the operation, I found that no water had passed through the wound. I introduced my finger, broke up a coagulum, and then about six ounces of bloody urine gushed out. I therefore put an elastic canula into the wound, and secured it by tapes; in the evening he was free from pain, and comfortable; pulse 90. On Saturday I removed the tube, being anxious to leave the opening into the bladder as open as possible, to allow of the escape of broken pieces, of which I thought there might be some. Going on well.

Monday, fourth day.—As he was free from pain, and the wound had a tendency to close, I introduced my finger, and upon it the bent urethra forceps, and fished out another calculus from behind the prostate, but could feel no more. He daily improved; no more portions of calculus passed, and at the end of ten days he passed the greater part of his water the right way. At the end of four weeks, when the wound was quite healed, he one day had difficulty in making water, which was caused by a small calculus getting into the urethra, and passing as far as the glans penis. He got it out with a pair of tweezers of his own; it weighed about fifteen grains. From this date he was well, and discharged on the 1st of May.

This man was afterwards operated upon again in the autumn, by another medical man, and another calculus removed.

Case 22.—S. B—, æt. 43, admitted September 29th, 1838. A healthy, ruddy-faced man, by trade a carpenter, and living at Walsingham. Had several times passed small calculi for some years, but for the last nine months had voided none. The symptoms had increased during that time, and were sometimes severe. I found a stone in the bladder, and operated upon him on Wednesday, October 17th, making my incisions as usual, and removing easily a flat stone, one inch by five eighths, of uric acid character. On Friday he passed all the water through the penis; at the end of a fortnight he was quite well.

Case 23.—R. G—, æt. 57, a gardener, residing at Walsingham, admitted March 4th, 1841, with stone in the bladder. He was a

fine man, very steady, resolved, five feet eight inches high, and rather robust. His symptoms were of one and a half year's duration, previous to which time he had been in the habit of voiding red gravel. His bladder was healthy, prostate of the usual size, and urine limpid. I operated upon him on Thursday, March 18th: my largest staff entered readily into the urethra; but, owing to there being a large quantity of urine in the bladder and the stone small, I could not detect it; therefore, after a little time I withdrew the staff and introduced the common sound, and with that heard and felt the stone. I then proceeded with the operation: the perineum was deep, and, after making the requisite incisions, I introduced the forceps and removed first one stone and then a second, both flattened and oval, weighing three drachms one scruple. There was an active vessel which bled rather profusely. I introduced a canula into the bladder, and stuffed in pieces of lint, so as to compress the vessel, and he was removed to bed; there was no return of bleeding. On the second day after the operation I removed the tube and lint; and from day to day, without a single bad symptom, he rapidly recovered. On the tenth day all his urine passed by the urethra, and in three weeks I discharged him cured.

Case 24.—J. M—, æt. 61, a stout, ruddy agricultural labourer, from Morningthorpe, had suffered from symptoms of stone for two years and a half. Some years ago he voided some small stones; latterly he has had great pain, and for twelve months past he has risen ten or twelve times in the night to empty his bladder. He was admitted into the hospital in July, 1841. I kept him three weeks; gave him some anodyne and alkaline medicines, with occasional purgatives. His bladder became far less irritable, and on the 5th of August I performed the usual operation. There was an ample but very deep perineum. I made a large external incision, gradually diminishing as I went deeper; the prostate was larger than usual, and very firm. On introducing the forceps, I immediately felt and grasped the stone, or, I should say, *stones*, for they lay together, and I extracted them both at once. There was but little hæmorrhage, and he was speedily removed to bed, after a very satisfactory operation; *i. e.* I never felt more satisfied with myself after an operation. An anodyne was given, and an elastic canula introduced. Next day he had a little pain just over the pubic region, but had a good night: pulse 80; plenty of urine. The canula was removed on Sunday, and on the tenth day the wound was nearly healed, and he passed almost all the water *per penem*. Discharged cured just three weeks after the operation.

Case 25.—K. H—, æt. 70. Agricultural labourer from Saham. A small, healthy man. Has had pain in passing water for about a

year, and his symptoms have been much aggravated for the last six months. Admitted February 4th, 1842. On sounding him, I found a compact, hard stone lying on the left side of the bladder; left lobe of the prostate much enlarged. After quieting the irritability of the bladder with Henbane and Liq. Potass., I operated upon him on Thursday, February 17th: used my largest staff; had to make a considerable opening through the left lobe of the prostate gland, and used the bent forceps to remove the calculus, which I was obliged to do slowly, as I constantly felt the projecting third lobe of the prostate getting between the blades of the forceps, and was afraid of laceration; however, after a while I succeeded in extracting a lithic acid stone weighing about half an ounce. A canula was introduced; he lost but little blood, and I believe no part was unneeessarily wounded. He had an anodyne immediately after the operation. I removed the canula on Sunday, and gave him ʒss. Ol. Ricini, which caused four motions. His pulse has never been more than 65 since the operation, and I never had a patient who suffered less from constitutional disturbance. At 2 o'clock on Monday morning he had a shivering fit, and complained afterwards of pain and distension in the body, which, when I saw him at 11 a.m. on Tuesday, had accelerated his pulse to 108 and made him very desponding. Ordered fomentations to the abdomen, and the following powder every four hours:—Pulv. Ipec. c. et Hyd. c. Cret. ana gr. iij. In the evening, pulse under 100, and less pain in abdomen; urine abundant, and wound healthy.

Wednesday, 11 a.m.—Pulse 92, tongue dry, restless night, and thirsty; body tympanitic, but no acute pain. Bowels acted well at 12 o'clock. Medicine omitted. 8 p.m.—has taken during the day two eggs in tea, with some broth and about an ounce of white wine in water; but he is still desponding and restless; pulse 110 and irregular; less abdominal tenderness, but a good deal of general disquietude.

Thursday, 11 a.m.—Passed a quiet night, but says he has not slept. Abdominal tenderness gone, and very little flatulence remains; pulse 92, tongue cleaner, and wound looking very healthy. *Vespere.*—Passed a good day, and has taken freely. He is better, but I cannot make him acknowledge it. Gave 20 drops Liq. Opii Sedat.

Friday, 11 a.m.—Is restless, and tongue dry; pulse slightly irregular, and became more so in the evening.

Saturday.—Has certainly lost ground, and is desponding; pulse irregular; free from pain. To have ʒiij. of wine, eggs and good broth.

Sunday.—Tongue very brown and furred, pulse bad. Has taken a table-spoonful of castor-oil, which produced two motions. Is evidently sinking.

Monday.—Sacrum red, and, I am afraid, will slough.

Tuesday.—Has lost more ground; pulse 106, and intermitting. Urine free through the wound, tongue parched. He died the next day.

Post-mortem examination.—Found no appearance of inflammation of the peritoneum; the wound went through the left side of the prostate, but not beyond its proper limit, and looked rather sloughy; third lobe slightly torn. No urinary infiltration; bulb of urethra and rectum untouched, and as far as the operation was concerned everything as it should be. Kidneys healthy; but the bladder was contracted and small, the prostate large. I consider him to have died from want of power in the system to repair so great a shock as the operation produced.

Case 26.—R. B—, æt. 73, admitted September 20th, 1842, from Ketteringham; a healthy but rather feeble man; had pain in making water for two years, which had increased very greatly during the last month; his countenance was florid, his pulse often irregular. The stone did not seem to be large, but very rough, and movable. Urine slightly acid, but loaded with puriform mucus. Ordered him a grain of opium every night, and some henbane, Liq. Potass. and Dec. Pareir. three times a day. By some mistake, after a few days, some spirit of turpentine got into his medicine, and aggravated his suffering; but it was soon discovered, after he had taken it twice, and the original mixture given. The operation was delayed a week in consequence of this error; but I performed it on Thursday, October 14th, when I found a *very large* prostate, which was very tough, and occasioned some difficulty to the introduction of the forceps. I could not feel the stone readily, as, when the urine flowed out, the calculus rolled forwards behind the prostate, and my forceps repeatedly passed over it; nor could I, until Mr. Johnson raised it by his finger in the rectum, seize the stone. I then had to dilate the wound through the gland a little before I could extract it. There was a good deal of bleeding from the enlarged state of vessels around the neck of the bladder, as we often see in old men whose prostates are enlarged. The calculus was oxalate of lime, weighed five and a half drachms, and was the most nodulated I ever saw. It must have produced much torture in his bladder.

I introduced a canula, and stuffed lint around it with my finger, after he was taken to bed. There was more bleeding than I liked, but this means prevented any continuance of it.

Friday, 11th.—The sheet far less stained with blood, and he is free from pain; only complains of flatulence; takes tea, milk broth, and some meat broth. Soreness about the wound. 7 p.m.—Countenance flushed, pulse full, skin hot.

Saturday, a.m.—Pulse about 98, slightly irregular; urine plenti-

ful, and tongue dry. 8 p.m.—Passed a quiet day; had an egg in his tea, and some cordial mixture composed of wine and gruel.

Sunday, 11 a.m.—Pulse irregular every four or five beats; tongue glazed and dry; countenance much sunk; urine turbid; body tympanitic, but much flatus escapes, which relieves his tenderness. I thought very ill of him, but still worse in the evening, when his pulse was at least 140. However, on the following morning his pulse had got steady again, and about 110. He took plentifully in the day, and at night I thought him rather better; but he soon got worse again, and died on Tuesday morning. I consider this man to have died from a prolonged operation on a constitution considerably enfeebled from the harassing disease under which he suffered. There was no post-mortem examination.

Case 27.—Thomas R—, æt. 31, from Felthorpe, a strong, ruddy, muscular countryman, admitted April 1st, 1843, with stone in the bladder, from which he had suffered but little until the last six months. On the 20th of April he was operated on. I used my largest staff, made an ample external incision, and the deep one rather lower than usual, laying open the membranous part of the urethra near the prostate. I did not so readily hit upon the staff by this means, but got a more ample opening into the bladder, and at the same time avoided wounding the artery of the bulb. The stone was easily seized and extracted, but proved to be much larger than I anticipated, and of the oxalate of lime species, with a recent deposit of uric acid on the surface; which change of deposit, I imagine, produced the more urgent symptoms which he endured for the last few months. A canula was introduced, as usual; urine flowed plentifully; and although his recovery was rather protracted, he got well, and was discharged in June, 1843.

Case 28.—G. L—, æt. 61, from Harleston. Had symptoms of stone about a year; admitted in March, 1844; a tall man, with a set colour in his cheeks. On sounding him, I found a healthy state of bladder and prostate, and discovered one large stone, and another or more of a much smaller size; urine clear, and not albuminous. After being in the hospital a few days, he had a rigor, succeeded by a sharp attack of fever. This took off his appetite and greatly reduced his strength. He was anxious for the operation, but I was obliged to defer it until the 18th of April. I used my usual apparatus, Mr. Crosse holding the staff. Immediately on introducing the forceps, I felt the stone and seized it; there was also a smaller one, caught near the joint of the forceps, which was crushed in the extraction. I then felt another small one just within the prostate, and easily obtained it. Of course I was uncertain as to how many more there might be, which rendered the introduction of the forceps and afterwards the scoop necessary, as the finger was not long enough

to enter far into the bladder. This prolonged the operation; and as one of the calculi had been crushed, I injected a syringeful of warm water into the bladder, which immediately returned. There was no hæmorrhage, and I believe the incisions were in the right place. I felt satisfied with the operation. In the evening he was comfortable; pulse about 96, and but little feverish.

Friday, 11 a.m.—Had a slight rigor, but free from pain, and there is abundant flow of water. In the evening there had been no return of rigor, and he expressed himself as comfortable.

Saturday.—Going on well.

Sunday.—Has lost his appetite; pulse not so good, and he is desponding; abundance of water through the tube.

Monday.—Not improving; pulse quick, and somewhat irregular. He went on badly, and died at the end of six days. I am afraid there was some urinary infiltration about the neck of the bladder.

Case 29.—J. R—, æt. 53, from Morningthorpe, admitted July 20th, 1844, with stone in the bladder. He was a labourer, tall and thin, but not strong-looking. His symptoms were not urgent, but decided, of about a year's duration; urine very clear. On sounding him, I found a flat-shaped calculus lying across the neck of the bladder. On the 15th of August I operated on him, but did not make so long an external incision as usual, or as I should have done for a large stone. The stone was easily removed, but was broken in the extraction; weight half an ounce, of uric acid composition, flattened, about one and a half inch long, and not quite so wide. He lost very little blood, and the tube was introduced. The tube was withdrawn on the third day, and from that time all the water came through the penis. I never had this occur so early before in an adult. At the end of a week he was well.

Case 30.—W. J—, æt. 73, from Burnham, admitted November 9th, 1844, with stone in the bladder; had symptoms for two years. A stout, healthy man, with bladder healthy; urine sub-acid; prostate moderately large; pulse slightly irregular; symptoms not severe. On Thursday, November 28th, I operated as usual. The perineum was deep, and I made my section of the prostate hardly large enough, although sufficiently so for the easy introduction of my largest forceps. I almost instantly grasped the stone, but caught it in a transverse direction, consequently could not extract it without further dilatation by the bistoury. The calculus weighed nine drachms, nearly oval, and of uric acid externally. There was no hæmorrhage at the time of the operation; but, after he got warm in bed, a good deal of bleeding appeared from the deep wound; but as it did not come with a jet, and I saw nothing of it when on the table, I conceived it must be from the large vessels about the prostate,

and not from any particular artery. However, it made him very faint; and I introduced a canula, and with my finger stuffed lint quite up to the neck of the bladder, close round the tube. This effectually stopped it, and it did not recur. Gave him some wine and hot tea. He progressed favorably; the tube was removed on Sunday; and although his recovery was rather slow, he was discharged quite well, at the end of five weeks after the operation.

Case 31.—J. P—, æt. 9, from Dickleburgh, a stout little boy; had suffered under symptoms of stone for six years, but was never examined until recently. Admitted November 1st, 1845. His sufferings were urgent, always having a discharge from the bowels when the water passed, which was very frequently. I ordered him Liq. Potass. Syr. Papav., and Mist. Camph., which materially relieved him; and I operated on Thursday, the 13th. All went on well. The calculus was quite black. He was soon returned to bed, and progressed favorably, although it was nearly three weeks before all the water came the right way again. He required a good deal of opening medicine during his cure, and was discharged quite well about six weeks after the operation.

Case 32.—J. B—, æt. 60, a Norwich weaver, not by any means robust, admitted April 29th with stone in the bladder. Had symptoms about two years and a half. Found, on sounding, a rough flat calculus in an apparently healthy bladder. On the 18th of May I proceeded to operate. Unfortunately, he could not hold his urine up to the time of operation, so that on making the opening through the prostate there was no gush. The incisions were readily made, but the straight forceps passed over the stone repeatedly, and it was not grasped until it was raised from behind the prostate by a finger *in ano*. After a little traction, the stone was removed; a tube was introduced, and he was sent to bed. The calculus weighed half an ounce. This patient had never a bad symptom of any kind after his operation, and was discharged cured at the end of five weeks.

Case 33.—F. M—, æt. 64, a resident in Norwich, a very fat and large man, who had been formerly a Peninsular campaigner, admitted in 1847, suffering much from calculus. The mucous membrane of the bladder was in a very high state of inflammation, with much purulent secretion and blood. He was obliged to make water every ten minutes night and day. Urine sub-acid. I gave him tincture of buchu, and dilute nitric acid in decoction of pareira; and in about three weeks I so far relieved his inflammatory symptoms as to recommend the operation of lithotomy, which was done on Thursday, the 16th of September. The perineum was of an unusual depth. I made an ample external incision through a great layer of

fat, cut upon the staff, but had to go very deep before I could reach the bladder. I introduced Mr. Martineau's largest gorget to guide into the bladder, as my finger would not reach it; and after a little manipulation and enlargement with my finger, got sufficient room to introduce my longest forceps. I speedily grasped the stone and extracted it. It was composed of uric acid, weighed three drachms, was very friable, so as to break in the grasp, but was all extracted. There was no bleeding. I introduced the gum elastic canula, which was withdrawn in forty-eight hours. This man's prostate gland was enlarged and forced the bladder back, so that I had a wound of at least four inches from the surface. He went on most favorably afterwards; in three weeks all the water came *per penem*, and he was discharged cured on the 21st of October.

Case 34.—A. B—, æt. 6, from Norwich, admitted with symptoms of stone of about four years' duration. He was a very robust lad, although short of his age. I kept him about a fortnight, and operated on him on the 21st of October, 1847. I almost immediately cut on the staff and entered the bladder; used my small forceps, and easily extracted an oval, flat calculus, weighing three and a half drachms, of uric acid externally. I did not in his case introduce any canula, the depth being so shallow there could be no obstruction to the flow of urine. He went on well from day to day. On the 12th day all the water came through the penis, and he was allowed to go home, cured, on the 10th of November.

Case 35.—T. R—, of Fersfield, near Diss, æt. 63, a man with a curved spine, about five feet high, admitted in October, 1848, with stone in his bladder; symptoms of about three and a half years' duration, and latterly very urgent. Bladder very sensitive, and secreting a large quantity of ropy mucus. I kept him in the hospital about three weeks, giving him decoction of Buchu with Sp. Æth. Nit. and Liq. Potass., and occasionally a warm bath, with castor oil twice a week. He soon improved, and I performed the operation of lithotomy upon him on Thursday, the 1st of November. Owing to his deformity, he could not separate his thighs, and his pelvis was drawn back so as to make his sacrum project much behind. He could not be tied up in the usual way; I therefore first introduced my large staff, and then had his legs at full length raised above my head, just leaving room for Mr. Crosse to hold the staff between his thighs. This created, *to me*, some difficulty, as the perineum was lax instead of giving way readily under the knife, and by the pelvis being at a more obtuse angle with the spine; the pubes looked downwards, and I believe I had more difficulty therefrom in cutting on the staff, as I think I opened the urethra nearer the bladder than usual, and not very readily. There was no hæmorrhage or wound

of the rectum. The stone broke under the grasp of the forceps, but it all came away at once. I introduced a canula after he was in bed, through which I found, in the evening, he had passed plenty of water. I removed the tube at the end of forty-eight hours; in a fortnight his wound was healed, and he was discharged cured in a month.

Case 36.—J. M—, æt. 7 years, from Beeston, admitted January 21, 1849, with symptoms of stone of three years' duration. The stone was large and rough, and generally lying at the neck of the bladder. I gave him opening medicine twice, and an anodyne at night. On Thursday, February 1, I operated upon him. He was put under *chloroform*,[1] and afterwards placed on the table and tied up. I very speedily removed a large, globular, rough calculus, weighing five drachms. There was a little bleeding; I introduced, therefore, a canula, and stuffed three pieces of lint along the side of it.

Friday.—Removed the tube and two pieces of lint, as the water flowed freely, but I could not feel the other piece.

Saturday.—Pulse 130, with some flatulence. I gave him some castor oil, and pushed my finger through the wound quite into the bladder, as I was afraid the sides were agglutinating too soon, but still the third piece of lint did not come away.

Sunday.—Pulse still 126; however, he had but little pain. As the bowels had not acted, I gave him two drachms of oil, and on Monday the nurse found the lint presenting at the orifice of the wound and removed it.

Tuesday.—Evidently better, but pulse still 120.

On Thursday the water all came through the wound; all the following week he continued very slowly improving; it was nearly a month before he was up, and then he passed pus from his bladder; but he gradually got better, and after a slow recovery was discharged cured.

Case 37.—R. W—, a ruddy old man, æt. 63, from St. Faith's Union House, admitted with stone in February, 1849. His stone was readily felt; prostate not large, and urine healthy. On the 8th of March I lithotomized him under chloroform. The calculus weighed an ounce, very firm, and oval-shaped. He bled from the upper angle of the wound; I introduced a canula and put three pieces of lint around it, and he was conveyed to his bed. In about half an hour I was summoned to him, and found he had a *pool* of arterial blood between his legs, and a feeble pulse. I immediately gave him four ounces of wine, and pushed in three more pieces of lint at the upper part of the wound between the tube and the pubes;

[1] This is the first mention of chloroform having been used in this list of cases.

this effectually stopped the bleeding, and he rallied. I believe the hæmorrhage was from the artery of the bulb, and I ought to have stuffed more lint in at first. In the evening he had quite rallied.

Saturday.—No more bleeding; pulse 80; large secretion of urine escaping through the canula.

Friday.—Removed the tube and lint; water very free; gave him one ounce of castor oil, which acted too freely and made him low. These old men are soon reduced, and do not bear it well. He went on well; at the end of three weeks he passed most of the water *per penem,* and he was discharged six weeks after the operation, cured.

Case 38.—R. E—, æt. 6, from Shotesham, admitted August 18th, 1849, with symptoms of stone in the bladder. Shortly after admission I sounded him, but did not distinctly feel a stone. He then had an attack of inflammation of the bladder, passing pus with his water, and his health became disordered, so that I could do nothing with him for many weeks. However, in October I sounded him again; felt a small stone, and operated on the 23rd. There were two stones (lithate of ammonia) easily extracted, but one broke in the extraction. There was a little bleeding, but the introduction of a canula surrounded with lint entirely checked it. The calculi weighed about a drachm. He progressed quite favorably, and was discharged at the end of the month.

Case 39.—R. M—, from Burgh, near Aylsham, æt. 4 years, was admitted July 20th with calculus; a healthy boy in other respects; symptoms of about two years' duration. Has constant stillicidium. I kept him ten days in the hospital, and operated on the 1st of August. Used a small staff, *and no gorget;* easily removed a small uric acid calculus, very rough on the surface. He had chloroform, and was perfectly unconscious of any operation. This patient went on every day improving; at the expiration of ten days he passed all his water *per penem,* and was discharged cured on the 24th of August.

Case 40.—H. J—, æt. 75, from Upwell, an old hale country labourer, rather deformed in the shoulders; had symptoms of stone between five and six years, but they are not very urgent. He had a large movable stone, very readily felt; urine rather acid, with slight mucous deposit; he is anxious to have it removed, although his age is great. Urethra capacious, prostate not bigger than usual at his age, perineum not deep. He was admitted November 2nd, 1850, and I operated upon him on Thursday, the 14th, under chloroform. I removed a large flat stone weighing an ounce and a quarter. After I had seized the stone, I could not get it through the wound without enlarging it with a bistoury: the surface of the prostate gland bled rather freely; I introduced a canula and stuffed

it round with pieces of lint, but hardly deep enough at the time, and was therefore obliged in the afternoon to introduce some more lint firmly into the deep wound; by this means all hæmorrhage ceased, the water came through the tube, and he progressed favorably.

Tuesday.—Removed three pieces of lint with forceps, but could not reach the other, which came away the following day, when I immediately removed the canula. I gave him wine and good supporting diet, and he went on most favorably; but it was not until three weeks had expired that any water passed through the penis; most likely the stuffing the wound occasioned it to be more patent than usual. On the 10th of December I perceived his scrotum a little œdematous, and his legs were slightly swollen. I did not like these symptoms, fearing they indicated some defect of circulation. I ordered him, in addition to nourishing diet, three ounces of gin in gruel every day; this increased the urinary secretion, and, I think, lessened the œdema. Exactly on the last day of the sixth week from the operation he passed all his water right, and it never returned by the wound. A fortnight after, he was discharged cured.

Case 41.—E. C—, æt. 61, farming-man, from Bradfield, admitted July 18th, 1851, with calculus in the bladder. Tall and thin, but healthy, and during the last year had suffered much. Four years ago he voided a stone the size of a bean; urine not loaded, and he is very desirous for the operation. He had a son operated upon by Mr. Dalrymple eleven years since, who quite recovered; the character of his stone was oxalate of lime with lithate of ammonia. On Thursday, July 24th, I operated upon him; easily cut upon the staff, introduced forceps, but grasped the stone in a wrong axis, so that I was obliged to use the beaked knife to enlarge the incision before extracting it. There was no bleeding at the time. I removed two calculi at one grasp, one an oblong spindle-shaped stone, the other a large oval one, weighing together six and a half drachms. There was a little hæmorrhage two hours afterwards, and I then introduced three pieces of lint around a hollow canula, which entirely checked it, and I removed the canula and lint in forty-eight hours. He went on so favorably that he was discharged cured in a month.

Case 42.—J. B—, æt. 16, from North Walsham, admitted November 1st, 1851. Had symptoms of stone as long as he could recollect. Small of his age and half starved, of sickly appearance. Urine very muco-purulent, and he suffers much from irritation. He bore the sounding remarkably well. After he had been in the hospital a fortnight, he had sharp rigor, succeeded by a paroxysm of irritation; his water was slightly alkaline, with a large deposit of

apparent purulent secretion. This prevented me from operating for some time; however, I determined that the only relief for him was the removal of the foreign substance; and on Thursday, November 18th, he was brought into the operating-room and put under chloroform. I passed a large staff, but believe it passed under the stone, as I only felt its presence by chucking it on withdrawing; he unfortunately was not put fully under the influence of the chloroform, and was badly held by a young dresser, so that he wriggled a good deal and got his bandage loose. I was some time before I satisfactorily felt the staff, in great measure owing to the patient's instability. However, I opened the membranous part of the urethra and carried my knife well along, and made, I fear, a large opening into the bladder; then introduced the forceps, which continually passed over the flat surface of the stone, which, from its weight, was sunk deep into the posterior part of the bladder; indeed, I almost despaired of seizing it; at last I did, and got it into the wound with considerable traction; then I thought I might get it in a more favorable axis and let it fall again into the bladder, and then had the same difficulty in seizing it. After sundry efforts on my part, Mr. Nichols tried and luckily caught it lengthwise, and I then readily extracted it. There was no hæmorrhage, nor was the rectum touched, although the opening was extensive. The operation was too prolonged. After he was in bed, he vomited incessantly large quantities of green secretion from his stomach. I gave morphia, effervescing draughts, cold water, and everything I could think of; it continued until he was quite exhausted. On Friday he had pain in the abdomen on the left side, which increased all over; I fomented him constantly. In the evening he was still sick, and body tympanitic.

Saturday, 11 a.m.—Pain increased; ten leeches to the abdomen, and poultice; pulse 120; incessant vomiting of green viscid bile; there was evident peritonitis.

Sunday.—Much the same; gave six grains of calomel, turpentine poultice to the abdomen, and a blister to the stomach. The turpentine created much heat and uneasiness; he was still sick, and much exhausted. In the evening I gave him four grains more of calomel, as there had been no relief, and I thought very likely it had been rejected in the morning.

Monday, 11 a.m.—I began to give up all hope, as all his symptoms continued and he could hardly open his eyes. All this time there was a free flow of urine through the wound. In the evening I gave him five drops of dilute prussic acid every hour for five or six hours; this medicine alone appeared to check the sickness: his tongue next morning was dry, but he had no sickness; he bore a very small quantity of brandy and tea, and in the course of the day a piece of bread in some tea. His whole abdomen was now very

sore from the turpentine; I gave him an enema of soap and water, but it passed away again with much flatus; urine abundant, pulse 130 and very feeble.

Wednesday.—I think, some amendment; tongue really clean, less abdominal pain.

Thursday.—Has taken some broth and brandy, and no return of sickness. I gave him one ounce Ol. Ricini at 11 a.m.

Friday.—I thought him better, as he retained his food, and his bowels had acted well by an injection.

Saturday.—Much exhausted; wound looked sloughy, and feculent matter passed into it. I now found he must sink, and he died thirteen days after the operation.

Post-mortem examination.—Kidneys much diseased, pelvis filled with puriform urine; ureters both much enlarged; general pelvic peritonitis from the escape of urine into the cellular structure around the neck of the bladder; a large pouch found on cutting open the bladder, in which the stone had laid across the entrance of the ureters, which accounted for their great enlargement; the sloughing was extensive about the wound, but it did not appear that the wound from the operation was more extensive than was required for so large a stone; the division of the prostate was in the right direction.

This poor lad was a most unfavorable subject, and had been terribly neglected by his parents, otherwise they must have procured advice for him at an earlier period.

Case 43.—J. S—, æt. 47, from Yaxham, a butcher's servant, healthy in every respect except having a calculus in the bladder, admitted in August, 1852. I sounded him frequently, and detected a stone readily; urine tolerably healthy; he did not suffer much, but requested to have the stone removed entire, so I indulged him with the operation of lithotomy on the 12th of August. The transverse artery of the perineum was large, and in making my first incision it spouted out furiously and was immediately secured; the rest of the operation went off well, and I did not, as usual, introduce a canula; during the day the sheets were plentifully wetted. I once introduced my finger, as the water ceased to flow for two or three hours, the inner wound being plugged with coagulum. The calculus broke in the extraction, but all came away in the same grasp of the forceps, so that I had not to introduce them a second time; it weighed half an ounce, lithate of ammonia on a nucleus of lithic acid. After six days the urine passed by the penis, and he rapidly recovered.

Case 44.—M. B—, æt. 61, a coal-merchant, a large fat man weighing nearly nineteen stone, had stone in the bladder which

occasioned great suffering; he was continually groaning, and frequently passing water; his symptoms had continued about two years. Except from the irritation produced at the neck of the bladder, he was in extremely good health, but was a nervous subject, and dreaded the sounding so much that I did not propose to him lithotrity, although I discovered that the calculi were small. I detected two by the sound, and determined on cutting him under chloroform. We contrived to tie his hands on the outside of *his legs*, for I could by no means get them so low as his ankles. After the staff was introduced, I commenced by making an ample perineal section, three and a half inches, very carefully; then cutting deeper at the upper part of the wound, felt the staff, but at a great depth. By following my forefinger, I at last laid open the membranous portion of the urethra, pressing down the rectum with my middle finger, and continued the incision through the prostate into the bladder: most fortunately, there was no bleeding. I immediately introduced my longest gorget and forceps on it, and easily extracted two calculi, each about the size of a large filbert; he was then put to bed, with a long elastic canula in the wound. His perineum must have been at least four inches from the surface to the bladder: most fortunately, nothing unforeseen happened, and the operation was very much to my satisfaction. The operation was performed on the 3rd of October, 1852; on the 4th he was going on well; 5th, plenty of water, pulse 96. Bowels opened by castor oil, which relieved flatulence. On the fourth morning I removed the tube, which was a day later than my usual custom; but the great depth of his fat perineum determined me to keep it in. His bowels gave me some trouble, and in spite of castor oil would not act again until he had an injection, and then he was obliged to be *got out of bed into the chair*, when he passed a very large mass of feculent accumulation which had descended into the pouch of the rectum, and which he could by no means evacuate whilst lying on his side. I was a little afraid of this effort on the wound, but no bad result followed; and from this date he progressed so favorably that he was perfectly well at the end of one month.

Case 45.—C. M—, æt. 47, a muscular but thin man, was admitted November 1st, 1852, having had symptoms of stone for about one and a half year; they were not urgent, but unequivocal. I operated upon him on the day of the Duke of Wellington's funeral, November 18th. He was placed under chloroform. I think I began my incision rather *too high*, and I very readily hit upon the staff; but he bled most awfully, probably from the artery of the bulb, as it came from the upper angle of the wound and was very furious: I, however, speedily removed a flattened uric acid calculus of half an ounce weight, and then tried to stop the hæmorrhage. I

tied one vessel, which I think was the transverse, but this did not command the bleeding; I therefore introduced a canula and stuffed five pieces of lint around it with some force, and this effected my object. He was not faint; the canula was kept clear by a feather; urine flowed in a few hours. Next day there had been no return of bleeding, and he had recovered from his shock. On the fifth day I removed the lint and canula; the wound looked a little sloughy, but by improved diet and getting his bowels sufficiently relieved by castor oil he soon got quite well, and expressed himself as very grateful for his cure.

Case 46.—J. N—, æt. 8 years, of Stokesby, near Acle, was brought to Norwich for advice in January, 1853. I found he had a stone in the bladder of large comparative size, and had suffered for some years, but his parents did not know what was the matter. I operated on the 1st of April, he being under the influence of chloroform, and speedily removed, by the lateral operation, a very rough oblong calculus weighing about one third of an ounce. He went on favorably; the urine passed through the wound until the sixth day, after which time it all passed by the penis. About ten days after the operation he had some pain in the back, and I then observed pus in the urine. I conceive the external wound healed before the deep, and perhaps a little abscess formed near the neck of the bladder; but this symptom speedily subsided, and at the end of three weeks from the day of operation he was discharged cured.

Case 47.—G. A—, æt. 2 years, from Harlestone, was admitted in March, 1853, with a small stone in the bladder; his symptoms were troublesome, but I observed that he had organic malformation of the heart, producing violent impulse all over the chest; he was occasionally livid, but not "cerulean." He was accurately examined by my colleagues, some of whom considered the ductus arteriosus, others the foramen ovale, open; but, at all events, there was some congenital defect, indicating that I ought not to use chloroform. On the 7th of April I operated, and was obliged to use a very small and shallow staff, which caused me much difficulty in finding the groove. I began my incision too high, the consequence of which was that I opened the urethra too far forward, although there was not much hæmorrhage. I got wrong, and in putting in the gorget I fear I got it between the bladder and pubes (a grievous mistake). However, I found out my error and afterwards opened the urethra in the membranous part, and got a small pair of forceps into the bladder and removed two very small lithate of ammonia calculi. I was heartily ashamed of this operation, which was a decidedly bad one. I put an elastic tube into the wound in order to be certain of a free exit of urine, and hoping that no infiltration

would take place. He had a good deal of fever, but a free quantity of urine continually. On the third day I removed the tube. Unfortunately, erysipelas attacked the wound, extending to the buttock, back, and scrotum; this was constantly dredged with flour, and it gradually gave way; but he became very fretful, and I was obliged to give him cordials with eggs and milk. I hoped he was progressing favorably, when about the fourteenth day he had convulsive fits, and in one of these he died.

The post-mortem was very curious. In the first place, a large clot of blood was found between the pia and dura mater, which, no doubt, was the immediate cause of death. On examining the chest there appeared but one cavity in the heart, the ventricular septum being absent and the foramen ovale open. In the abdomen every organ was misplaced, the stomach on the right side, the liver on the left; there was no spleen to be found; the cæcum was in the left iliac fossa, and the sigmoid flexure of the colon in the right; in fact, a complete "travestie" of all the abdominal organs. There was a small abscess *behind the pubes,* where my finger had gone wrong, but the opening into the urethra and bladder was in its right place. Altogether it was a very unusual appearance, but unfortunate for me in its result.

Case 48.—J. H—, æt. 21, native of Norwich, came to the hospital in July, 1853, a robust well-shaped young man. He had suffered for six or seven years with occasional trouble after making water. On sounding I discovered a stone which I conceived to be of a flat oval shape and not very small. It was very sonorous, and could be easily felt through the rectum. I operated upon him on the 28th of July, using a very large staff, which passed easily, as his organs were particularly largely developed. I readily cut upon it and extracted a calculus weighing ten drachms, uric acid externally, of a brown colour, flattened, and nodulated on its surface. I tied one small vessel and introduced a canula; he was perfectly unconscious of the operation, from the effect of chloroform, and progressed favorably from day to day. I removed the tube at the end of forty-eight hours, and he was discharged at the end of a month from the date of the operation quite cured, and very grateful for what had been done.

Case 49.—M. L—, æt. 45, from Holt, was admitted in January, 1854, with stone in the bladder. He was a florid, yellow-haired, strong country labourer, in good health otherwise. He had had symptoms for some years, but on sounding I did not find a very large stone, but one that was flat and rough on the surface; his urine at first held a large quantity of the lithates in solution, but after a few days' rest and giving him a mixture of buchu, liq.

potass., and nitric æther, he was much relieved. On the 2nd of February I performed the lateral operation, using a large staff of Mr. Firth's. My first incision was as usual, but I suppose in continuing it I may have cut rather high, for there was a copious venous hæmorrhage, probably from the substance of the bulb; the bleeding mixed with the urine made rather a large show, but, seeing no arterial jet when I had got sufficient opening into the membranous part of the urethra and prostate, I immediately, upon the gorget, introduced the forceps, seized a flat-shaped stone, and, after a little traction, removed the calculus, which broke under the grasp. My opening into the bladder was not large, and if the stone had been bigger I must have enlarged the section. There was no hæmorrhage afterwards; the canula was introduced, and he was sent to bed. This operation was performed under the influence of chloroform, and the patient was not aware of any part of it. The canula was removed after forty-eight hours, urine plentiful. He progressed so favorably that on the ninth day all the urine passed through the penis, and he had the most rapid recovery I have ever seen in an adult. The stone weighed half an ounce.

Case 50.—J. A—, æt. 3, from Dereham, was brought to the hospital on the 8th of May, 1854, having suffered from stone for at least two years. His pain was great, and he had prolapsus ani whenever he voided his urine. I readily detected a calculus, and on Friday, May 12th, proceeded to operate. I introduced a large staff for such a child, used a small knife, no gorget, and readily removed with the scoop three compact calculi, each about the size of a large horse-bean, composed of lithic acid. There was no hæmorrhage, but a plentiful flow of water. On the fifth day he voided one lumbricus teres, and after this the prolapsus never again took place. At the end of a fortnight he was well and discharged. The calculi weighed a drachm.

Case 51.—W. H—, æt. 7, from Trowse, a healthy looking boy who had suffered from stone for three years, admitted June 1st, 1854. On sounding I readily detected a calculus. On the 3rd of June I operated, under chloroform, of which he took a good deal. I think I used too large a staff, as it went with much difficulty into the urethra. I immediately cut upon the groove and ran my knife along it until I thought I felt the end of it, but found I still was not in the bladder; fearful of cutting beyond the end and still finding the staff very firmly grasped, I removed it and introduced a smaller one; fortunately it went in without difficulty, and then I succeeded in cutting along it through the neck of the bladder and immediately removed two flattened uric-acid calculi of considerable size, weighing two drachms. I bungled about this operation in a

way perfectly disgraceful, as there ought not to have been the slightest difficulty. I was rather afraid that my assistant had withdrawn the point of the staff out of the bladder in his zeal to stretch over to witness the incisions. He was violently sick the following night and day, his pulse became 140, but there was a plentiful flow of urine and his body was soft. I believe his gastric symptoms arose from the chloroform. He had half a grain of calomel, effervescing draughts, and every means used for lessening sickness. On the third day his tongue, which had hitherto been very foul, began to get clean; he was so faint that I gave him one and a half ounce of sherry with egg and sugar. After this he got his bowels relieved, and his symptoms abated; he was very unwilling to take sufficient nourishment, but he took milk *ad libitum*. His pulse gradually became slower. A week after the operation I gave him two ounces of port wine daily; the buttocks were attacked with an erysipelatous blush, and the wound sloughed on the surface, but it speedily cleaned under the influence of port wine, egg, and milk. A fortnight after the operation his pulse was 110 and his appetite capricious, and all the water came through the wound. About this time he passed a good deal of pus through the wound, and I conceived that an abscess had formed near the neck of the bladder, probably between it and the rectum. *Three weeks* after, all water through the wound, but he is decidedly improving in health and spirits, and is quite free from pain. *Four weeks*, is allowed to get up for a little while, is hearty, and has regular motions. Still takes two ounces of port wine in the day, with two drachms of tincture of bark. *Fifth week*, although improved in appearance, the greater part of the water comes through the wound, with some pus; the opening is now a mere sinus; a few drops pass per urethram. The boy is hearty and playful. *Eighth week*, is now out of all danger, and much more water comes naturally, but there is still a sinus *in perineo*. I had hoped as he improved in health and strength that the sinus would heal. I occasionally introduced a piece of lunar caustic, and sometimes a probe dipped in nitric acid, either of which caused a small slough; and I afterwards employed a piece of lint and pressure. At the end of three months he was so much better that I allowed his friends to take him home; but he did not yet "stand up *like a man* and piss against the wall," but preferred sitting across a pot when he wanted to make water. This was not satisfactory, but I discharged him as cured of his stone symptoms.

Case 52.—S. B—, æt. 60, admitted July 28th, from Walsingham, for the second time with stone. A healthy-looking man, of abstemious habits, a carpenter by trade. Seventeen years ago I lithotomized him and removed a calculus one ounce in weight; he remained free from symptoms until about a year and a half ago,

when he began to suffer as formerly. He has been in the habit of occasionally voiding small calculi from time to time. I ascertained that the stone was a small one, and rather urged lithotrity, but he would not willingly consent, more especially as he had had so speedy a recovery before as to be quite well in a fortnight. I explained to him the comparative danger of the two operations, but he preferred being cut under chloroform. On the 10th of August I operated in the usual manner, using a very large staff, and removed a calculus very similar to the former one, uric acid externally, and rather rough and tuberculated. I passed the forceps over the stone once or twice before I succeeded in obtaining it. My incisions were limited, as I felt certain that the stone was small; the transverse artery required securing, as it spouted out rather furiously. I introduced a canula, but there was no further hæmorrhage. At the end of thirty-six hours I removed the canula, and he was doing well. On the seventh day he passed all his water through the penis and continued to do so. I never recollect this circumstance in an adult so early. He got quite well in three weeks and was discharged cured.

Case 53.—W. H—, æt. 8½ years, from Heigham, admitted September 24th, 1855, with strongly marked symptoms of calculus, but in spite of his great suffering he was muscular and well nourished. *One year and a quarter* ago I operated on this boy in the hospital and removed two calculi weighing a drachm each. He had a long, protracted, and very imperfect cure, the former operation being a bungling one, and, to myself, very unsatisfactory. On the 28th of September I operated upon him again, using a smaller staff than I did before, as his urethra was very contracted. I speedily made my incisions, and found that he had a large flattened calculus weighing three and a half drachms; this was removed by gradually distending the section of the prostatic neck of the bladder by the forefinger sufficiently to allow its extraction. On seeing the calculus I had no doubt that I must have left one in the bladder at the previous operation, as it was impossible that another entire calculus could have formed again after so short a period; this circumstance would also account for the continuance of symptoms after the former operation and the very long time before the wound healed; indeed, I am surprised that the perineal section did heal. After this second operation he had no bad symptoms. Some water came *per penem* at the end of a week. This case was a very instructive one to me, and shows how necessary it is to examine well the bladder after an operation to be quite certain that nothing is left behind. The boy's amendment was progressive, but not rapid, a circumstance arising from the morbid condition the mucous membrane of the bladder must have been in from the long suffering from so large a stone. He was perfectly cured by this second operation.

Case 54.—M. C—, æt. 66 years, gardener, from Hellesdon, a thin spare man, of rather sickly aspect, but who had always kept to his work, was admitted January 3rd, 1856, with stone in the bladder; his pain was not great, nor his water much loaded; it was slightly acid, and contained some mucus. I examined him once or twice, found his urethra large, the prostate tolerably healthy; he was desirous of having the stone taken from him whole. He occasionally suffered from dyspepsia and pyrosis, but this he did not mention to me until after his operation, when it much distressed him, and, I fear, tended much to the fatal termination. On Friday, January 25th, I operated; the incisions were speedily made, and there was a sufficient opening into the bladder; the forceps immediately seized a small calculus with a rough exterior, some portion of which broke off into the blades of the forceps. It was of lithic acid externally, and weighed only a drachm. There was no hæmorrhage. A tube was introduced, and during the rest of the day he went on well. On Sunday there was some flatulence and feeling of sickness, which continued during the day. Urine abundant. I removed the tube forty-eight hours after the operation. Tongue dry in the evening.

Monday.—Says he is always sick when lying on his back; used a bed-chair to support his back, and gave him sherry and water with twenty drops of Liq. Opii sed., which he returned; then tried an egg with brandy and sugar, but this succeeded no better; he had effervescing draughts with two drops of hydrocyanic acid. Pulse, quiet at first, became to-day very quick and feeble, but still he had no abdominal symptoms.

Tuesday.—He was very low; pulse 120, and more feeble. I saw he was sinking unless he could keep some nourishment on his stomach. I had three ounces of strong broth injected into the rectum, but he would not submit to that a second time. Nothing availed, and he died about 103 hours after the operation, on the fifth day.

Post-mortem.—An accurate examination of this body discovered no reason for his death as far as the operation was concerned; all the incisions were correct, and the parts about the neck of the bladder looked healthy. I can only suppose that the obstinate sickness, perhaps produced by chloroform, so exhausted him that he sunk from that cause; there certainly was no visible sign about the pelvic fascia to account for the unhappy result.

Case 55.—J. C—, æt. 59, a small tradesman, from Diss, was admitted February 1st, 1856, of healthy aspect and all his organs sound. Had symptoms two or three years, but the stone had been detected only a few months. He was of nervous temperament, and suffered much on sounding. I suspected a small, flat, oval calculus,

with a rough surface. Prostate healthy. On Friday, the 22nd, I operated, and used my largest staff, which I regretted afterwards, as I had some difficulty in introducing it. I began my incision as usual, about an inch and a quarter anterior to the anus, carrying it down obliquely for about three inches. I soon felt the staff, and cut on it, but hardly carried my incision far enough to the prostate; for on introducing the gorget, which I did very carefully, I found it did not enter the bladder. Mr. Nichols at the same moment withdrew the staff. On introducing my finger into the deep wound I found I could not possibly reach the bladder without enlargement; I was therefore obliged again to introduce the staff (using a smaller one), as I dare not go on without it as a guide. There was a *momentary* difficulty in getting the instrument in; I then with a beaked knife enlarged my opening into the bladder, which enabled me to introduce the forceps and grasp a calculus (oval and flattened) weighing half an ounce two scruples. There was no hæmorrhage; a tube was put into the wound, and he was sent to bed. On Sunday I removed the tube, and he was going on well.

Monday.—Had some oil to relieve the bowels. His nights are good, but he is of nervous temperament and imagines all kinds of vagaries.

Friday.—Still very imaginative about his complaint, maintaining that no "living man" ever suffered anything like what he has; he always speaks in the superlative degree.

Saturday, ninth day.—Passed a teaspoonful of urine per penem, which occasioned him some uneasiness, but his symptoms are all vanishing; allowed pudding and meat.

Monday.—Daily a large quantity of urine through the natural channel; is much more reconciled, and begins to think he shall get well. He was discharged cured on the 30th of March.

Case 56.—E. D—, æt. 21, came to me in May, 1856, with a large calculus in the bladder, suffering very much; he was sounded by me fifteen years ago and a stone was felt then, but I never could persuade the parents to insist on an operation; he went to school then, always wetting himself both at day and night; as he grew up he never complained, and was apprenticed to his father, a plumber and glazier, and continued to do his work with the other men; he lived as others and drank a good deal of beer, but never would consult me. He was astonishingly reserved in regard to his suffering, which must have been great. He was, however, muscular and remarkably strong, lifting heavy weights and doing a long day's work. I always told him that a day of reckoning would come. In the early part of May he came, having arrived at man's estate, and told me all his grievance, and I got him into the hospital. On sounding I found a large flat and rather rough calculus; urine sub-acid, contain-

ing mucus and phosphates. I prepared him for a serious operation, and performed lithotomy on the 23rd of May, using my large staff. I made an ample external incision and speedily laid open the membranous part of the urethra, continuing my incision through the left side of the prostate; without using the gorget, I introduced my largest forceps and grasped the stone, but was obliged to use the beaked straight knife to enlarge the perineal section. I think it was the lower edge of the levator ani, or the deep perineal fascia, which occasioned the delay. By continued and rotatory traction I removed a flattened, thick, and heavy calculus of two and a quarter ounces. A vessel was secured at the upper part of the wound, a tube introduced, and four pieces of lint round it. He was sent to bed, and in four hours a quantity of urine had passed through the tube. Next day, Saturday, pulse 120, face flushed; complains of the pressure in the wound; water free.

Sunday, forty-eight hours.—Pulse 106. Removed the canula and lint; everything comfortable.

Tuesday.—Bowels opened yesterday by castor oil; free from any bad symptoms, copious flow of urine. On the twelfth day there was some hæmorrhage from the deep wound, from whence I cannot say exactly, for the ligature which had been applied at the operation had come away many days previously. The nurse informed me there were three or four clots, amounting to several ounces in weight. By exposure and cold applications the bleeding ceased without any pressure or reintroduction of lint, which I was very glad of; he had a sharp rigor at this time. Fourteenth day, no more bleeding; allowed meat every day. He progressed rapidly from this date. On the sixteenth day all his water came through the penis, and he was discharged cured exactly one month after the operation.

Case 57.—J. L—, æt. 52, a robust countryman, from Dereham; has suffered from bladder symptoms four or five years, but a stone was not discovered until recently; he was admitted in the early part of May, 1856, and I found a very rough and hard stone in his bladder. I operated upon him on the 23rd; the operation was much to my satisfaction; there was no bleeding. The calculus weighed eleven drachms; uric acid, with a deposit of phosphates on the surface. A tube was introduced and left in for twenty-four hours, but no lint. On the tenth day he passed nearly half his urine through the penis, and on the fourteenth it all came the right way; at the end of the third week he was quite well.

Case 58.—H. H—, æt. 7, admitted in June, from the Norwich Workhouse, suffering much from calculus. He was well nourished, but stunted in growth. He had always lived in the workhouse; had rarely tasted meat, and never any beer. I operated upon him on the

7th of June, using as large a staff as I could introduce, a small knife, but no gorget. The stone was easily obtained, and there was no hæmorrhage. He proceeded towards recovery without any drawback, except that at the expiration of three weeks his appetite flagged. I gave him Vin. Ferri, egg for breakfast, and plenty of milk; after which he soon rallied, and was discharged cured. The stone weighed half an ounce.

Case 59.—R. R—, a respectable tradesman, from the neighbourhood of Diss, æt. 58, healthy and well made, consulted me in July, 1856, with symptoms of stone, not very urgent, and on sounding I found one of moderate size in the bladder. He was desirous of getting rid of it, and I operated on the 18th of July. I used a large staff, found a good deal of perineal fat, but readily opened the bladder; in this case I used the gorget, as my finger would hardly reach the prostate sufficiently to split it, which I usually do; a very small opening only was necessary in the bladder, and I readily grasped and extracted a lithic-acid stone, weighing about three drachms. There was no hæmorrhage, and all was satisfactory. In the evening it was necessary to put the finger into the bladder to allow the flow of urine; he had a good night. There was a strong tendency in the wound to heal—I think, too rapidly—so that on the third day I again put my finger into the bladder, as more than half the water had come the natural way, and I was afraid the superficial wound might close before the deep one about the neck of the bladder did, and produce infiltration. He speedily got well. There was a little difficulty in getting his bowels thoroughly relieved in bed; I therefore allowed him at an early date to get out of bed, and use the chair. I have experienced this difficulty once or twice in stone patients, and on some occasions scybalæ have been retained in the colon, which have occasioned ugly symptoms.

Case 60.—J. B—, æt. 70, admitted August, 1856, with a large calculus in the bladder; his suffering was great. I had operated on this man ten years ago, and removed a stone weighing half an ounce; he got well, but two years afterwards there were symptoms again, and I lithotrotized him and removed calculus to the amount of two drachms; his bladder remained free for some time and I lost sight of him. In August, 1856, he called on me and explained that he had all his old symptoms; I sounded, and discovered a large stone; he was at this time thinner and more feeble, but yet he had *pluck*, and determined to submit, if I thought right, to another operation. His bladder would retain but very little urine, sub-acid and very muco-purulent; his pulse slightly intermitted, and he was rheumatic. An operation was determined on, and I performed it, *without chloroform*, as I feared the cardiac irregularity; I used a very large staff

which passed *under* the stone; his prostate was considerably enlarged. I made considerable incisions and attempted to seize the stone, which was immediately felt, but the forceps slipped repeatedly. I then used a larger and longer pair, and found that the calculus was irregular in shape, with a projecting *mammillary* process, from which the instrument slipped; I got a larger grip, but could not extract it without enlarging the incision in *both* sides of the prostate; by careful manipulation and gradual distension the calculus came forth, very large and globular, with a projection on it. There was no active hæmorrhage at the time; I, however, put a tube into the wound and inserted four or five pieces of lint, as I expected bleeding from so large a wound. He bore the operation most courageously; there was a little oozing in about two hours, easily stopped by pieces of lint. Had an anodyne at night. Two days afterwards (Sunday) I removed the lint and tube; urine very free, pulse 100; expressed himself as comfortable; diet improved, as he required good support. He went on well until Thursday, when his pulse was 80, rather flagging, and the wound patulous and sloughy.

Saturday.—Very finely; bowels to be opened by oil; has meat and wine daily. He continued to improve for some time, but on the twenty-third day after the operation the house-surgeon found there was active hæmorrhage from the wound, and put in a canula with lint around it, which stopped it. It was removed in four days, but unluckily the hæmorrhage returned and made him very weak. I gave him more wine and brandy, and all the nourishment his stomach would bear.

September 22nd.—He went on improving a little from day to day, but still no water came *per penem;* thus he continued (without any more bleeding) for several weeks, but in October he visibly declined in power and could not relish his good living, and he died twelve weeks after the operation.

Post-mortem examination.—Left kidney small, and containing calculous matter; bladder much contracted and thickened; prostate enlarged; pericardium generally adherent.

Case 61.—W. H—, æt. 9, was admitted in August, with a return of his symptoms for the *third* time; I detected a stone immediately, and operated on the 22nd of August, 1856, removing an oblong uric-acid calculus, weighing three drachms, of very much the same character as the one removed twelve months before. I found the parts rather thickened, and had some difficulty in feeling the staff; but it was soon done, and he was sent to bed under chloroform, knowing nothing of his operation. He went on most favorably, and was quite well at the end of three weeks.

Case 62.—R. H—, æt. 3, from Statham, admitted July 20th,

1857, with calculus; a very healthy boy, but suffering considerably. I operated upon him on the 26th, under chloroform, but did not use the gorget. The stone was removed by the *scoop*, and weighed a drachm. There was no hæmorrhage, and he made a speedy recovery, having passed all his water through the penis at the end of ten days. He was discharged cured three weeks after the operation.

Summary.

No of cases .	62	Deaths .	8 Average, 1 in 7¾
Under 10 years .	21	„ .	2
From 10 to 20 years	2	„ .	1
„ 20 „ 30 „	5	„ .	0
„ 30 „ 40 „	1	„ .	0
„ 40 „ 50 „	3	„ .	0
„ 50 „ 60 „	7	„ .	0
„ 60 „ 70 „	16	„ .	2
„ 70 „ 80 „	7	„ .	3
Total „	62	„ .	8

At one period fourteen cases, and at another thirteen, were operated upon in succession without a death.

In perusing the foregoing cases of lithotomy one cannot help being struck with the care and caution exercised by the operator, as well as with the success of his operations; the latter being, no doubt, in great measure the result of the former. His usual mode of operation was the *lateral*, such as was always performed by his great predecessor, Mr. Martineau. With very few exceptions, Mr. Norgate, like Mr. Martineau, Mr. Dalrymple, Mr. Crosse, and others, used the blunt gorget to dilate the prostate and as a guide to the introduction of the forceps; but his successors have well-nigh laid the instrument aside, using the finger only.

The following appear to me the principal lessons to be learned from Mr. Norgate's cases, namely—

1. That young children should have time given them to get accustomed to those around them before operating, and that great attention should be paid to the state of their bowels as to worms or any other disorder.

2. That in adults the external incisions should be free, the staff large, but not *too large* for the urethra; that, as a rule, a canula should be introduced through the wound into the bladder, and, in case of hæmorrhage, lint introduced around it. Whether the lint should be in separate pieces, or in a single piece, or previously wound round the canula, may be a question; for, in one of the cases reported, a third piece of lint could not be discovered, being retained in the deep wound and causing symptoms of danger to the

patient, an occurrence I have never before seen noticed as one of the casualties attending the operation of lithotomy.

3. That both in old and in young subjects, especially the former, the constitutional health should be well supported, and no unnecessary depression of strength be occasioned either by medicine or diet.

4. That great as is the benefit and comfort of chloroform, both to patient and operator, the operation should not be prolonged more than needs be because of the unconscious state of the patient, lest the quantity of chloroform taken should produce sickness and depression afterwards.

5. That in children great care is required not to use the finger roughly, lest the connections of the bladder be disturbed and fatal mischief arise in consequence.

6. That childhood and moderate old age (viz. from fifty to seventy) are favorable periods of life for the operation of lithotomy.

7. That careful examination of the bladder be made before the patient is removed from the table, in order to avoid the possibility of another stone being left behind.

8. That difficulty may now and then occur by removing the staff before the opening into the bladder is certain and unmistakable, owing to the trouble which there often is in reintroducing it. I have myself in one instance seen this difficulty so great that the operator was advised to send the child to bed rather than prolong the operation, leaving further attempts to another day; and this by a surgeon of great repute in our locality, whose judgment is seldom at fault. But it occurred to me that, as the urethra had certainly been opened, *a director passed into the wound might find the opening and render the staff unnecessary.* At my earnest entreaty this was done, the opening was readily discovered, and the stone felt at the end of the director, and by enlarging the inner wound with a bistoury the stone was readily extracted, and the life of the child saved. The idea arose in my mind at the time the difficulty occurred; and although I claim no credit for the suggestion, it is possible the recital of the circumstance may lead to beneficial results.

Art. II.

On the Influence of Age in Hereditary Disease. By William Sedgwick.

(*Continued from* vol. xxxviii, p. 523.)

When the abnormal changes occurring during fœtal development, instead of being limited, as in the preceding cases, to some local defect of structure, affect the general condition of the blood, it is always difficult to distinguish between what is due to hereditary in-

fluence and that which is the result of infection or of contagion; for although, strictly speaking, any morbid affection of the system may be transmitted as an inheritance, yet the conditions attached to such transmission in the case of infectious or of contagious diseases are far from being the same. Consequently, when one of these diseases, such, for example, as smallpox, scarlet fever, or measles, is conveyed to a fœtus-in-utero by a mother suffering from the same disease at any period of her pregnancy, it would be convenient to employ the term imparted instead of transmitted to express the conveyance of the disease, in this class of cases, from the parent to the offspring; whilst the term inherited might with advantage be reserved for those cases in which any distinctive or exceptional phenomenon in the character of the same disease, affecting the offspring after birth, is recognised as having occurred also in some members of the same family on a previous occasion; and which, as there will be occasion to notice more fully on a subsequent occasion, either may or may not be limited by age, or by some other restrictive influence in its hereditary transmission. The necessity for thus distinguishing between these two conditions of the system has been well illustrated by Mr. Hutchinson's[1] observations on congenital syphilis, which tend to prove that, although the acquired disease is readily imparted to the fœtus-in-utero, yet "the offspring of the (so-called) inherited taint do not show any symptoms which can be recognised as syphilitic;" or, in other words, that syphilis, contrary to what might be inferred from clinical observation on hereditary disease, after being conveyed from the first to the second generation, is then lost, instead of reappearing, as it would be allowable to expect might sometimes be the case, in the third generation. These observations, together with others of a similar kind—such as the now well-established fact that in infectious diseases generally, and especially in smallpox, the fœtus may have the disease without the mother having been at any period of her pregnancy affected by it—show that in thus becoming what appears to be merely a medium of communication, the mother in such cases occupies a position which, as regards her personal relationship to this class of diseases, corresponds with what has been very commonly observed in the atavic transmission of hereditary disease. In admitting, therefore, the necessity for the distinction above referred to, it should at the same time be noticed that there is still an important analogy between these cases, which it may be useful to refer to again in a succeeding paper; since it is evident, from many carefully conducted observations on this subject, that, whether the disease be simply imparted or hereditarily transmitted to the offspring, the communication, as regards the mother, may in either case be intermediate or direct.

In directing attention to diseases of this description affecting the

[1] 'London Hospital Reports,' vol. ii, p. 160.

fœtus-in-utero, it is, moreover, necessary to avoid the error of supposing that the epoch of conception would serve as a distinct boundary of separation between a true and a false inheritance of disease; for it is, in like manner, often impossible, in the large class of syphilitic diseases, to distinguish between a diseased condition which has been imparted by the mother to the fœtus-in-utero during its development and that which has been conveyed to it as an inheritance. The chief cause of the difficulty in such cases is due to the disease being specially connected with the reproductive system, in consequence of which, notwithstanding the close analogy which is traceable between syphilis and the preceding class of exanthematous diseases, the phenomena attending its recurrence as a family defect are observed to be more complicated than in other infectious diseases. It is apparently owing to insufficient observation on this subject that many writers have been so ready to cite cases of syphilis in preference to other diseases, for the purpose of illustrating their opinion respecting hereditary transmission; and they have, in consequence, fallen into the error of supposing that an hereditary disease exists from the first in the form of a germ, connected either with some portion of the solid structure or with the blood. Dr. Whitehead,[1] who has studied the subject chiefly in connection with syphilis, adopts the latter opinion, and states "that in all diseases to which a predisposition was inherited the blood is the part of the system where the germ of the hidden evil is to be found;" and he thinks "it is highly probable that the blood, if carefully analysed soon after birth in an individual so circumstanced, would be found to possess some characteristic peculiarity in the arrangement of its elemental constituents." With reference to this application of the atomic theory to disease, it may be remarked that, although it might, perhaps, be allowable to hold such an opinion in cases where the disease was imparted to the fœtus in the womb, yet it certainly would not be allowable to do so in cases of inherited disease, since it cannot be reasonably assumed that the so-called "hidden germ of disease" could on any occasion be thus lodged in the embryonic germ, and be kept, as it were, waiting out of its place until a suitable material like the blood was developed for its accommodation; or that in any case of hereditary disease which may also happen to be congenital, such, for instance, as may sometimes be observed in cases of tubercle, the morbid products of diseased action which might, by improved means of investigation, be ultimately recognised in the blood after birth, could have been present in the form of a diseased atom in the germ previous to the development of the blood itself. This atomic theory of hereditary disease is, moreover, opposed to what occurs in many cases of hereditary transmission amongst the lower animals, especially in those

[1] 'On the Transmission, from Parent to Offspring, of some Forms of Disease, and of Morbid Taints and Tendencies.' 2nd edit., 1857.

cases in which the development of the offspring takes place within an egg. For the recurrence in such cases of a defect or peculiarity, which is often transmitted for several successive generations and manifested at a definite period of life within the shell, is simply an illustration of the restrictive influence of age in abnormal development.[1]

In addition to the preceding cases in which hereditary diseases and defects have been developed before birth, it may be further remarked that there are many cases of disease in after-life, besides those which

[1] The well-known liability to the repetition of abortion, which has often been erroneously associated with syphilis, is to some extent connected with the present inquiry, both as regards those cases in which it is due to the morbid condition of the mother and the fœtus together, and of the one independent of the other. There are, undoubtedly, many cases in which the abortion is specially dependent on a periodic change in the maternal system, in consequence of which, apart from any distinctively hereditary influence, its recurrence is often limited to the same period in many successive pregnancies, as in the cases referred to by Schultz (Lacroix, Thèse, Montpellier, Juillet, 1812); by Dr. Churchill ('Theory and Practice of Midwifery,' 4th edit., p. 169, 1860); and by many other writers who might be quoted for this purpose. But, in addition to this acquired force of habit, it has also been noticed that abnormal arrests of pregnancy are occasionally subject to hereditary influence; the mother, in some of these cases, having transmitted to her daughter, "with her constitution and certain organic conditions, a particular predisposition to abortion." (C. Devilliers, 'Nouv. Dict. de Méd. et de Chirurg. Pract.,' art. "Avortement," tom. iv, p. 307, 1866. See also Velpeau, 'Traité complet de l'art des Accouchemens,' 2nd edit., tom. i, p. 400, 1835); whilst on the other side there are cases, apparently more numerous than the preceding, in which the premature expulsion of the fœtus is consequent on disease either imparted or transmitted to it from the mother. The pathology, however, of these diseases is usually obscure, as it is often very difficult to obtain satisfactory information on the subject; and even where there is ample evidence to show that the death of the fœtus is connected in some degree with transmitted disease, it is seldom that the precise nature of such disease can be traced. The outward appearance of the fœtus alone, without any internal examination of the body, may perhaps be sufficient for the purpose in some forms of fœtal disease, as, for example, in hydrocephalus. But such cases of easy diagnosis are comparatively rare, and it seems to have been far too hastily assumed in the present day that the outward appearances of disease in the fœtus are usually indicative of its nature; and hence the customary fallacy of regarding nearly all those morbid appearances in the fœtus as syphilitic which do not admit of being suitably described by any other name, the term syphilis in such cases being used as if it were a lumber-room for fœtal pathology. There need be no hesitation in admitting that syphilis and abortion are often closely allied, and that Dr. Clay ('On the Frequency of Abortion from Hereditary Causes') is to some extent, although not altogether, justified in stating that "constitutional defect, and especially syphilis, on the part of either or both parents," is the most common cause of abortion, for many cases have been erroneously ascribed to this disease which were quite independent of it. Still, after making allowance for doubtful cases, and the hasty assumptions respecting hereditary transmission, on the part of those whose observations are either imperfect or inaccurate, there remains a very large and varied group of cases in which the recurrence of abortion, at the same period in successive pregnancies, has been directly due to this disease. It must, however, be acknowledged that in such cases there is sometimes an almost insuperable difficulty in tracing the disease to its source, owing to the protean character of syphilis in its transmitted forms, and the predominance of the constitutional over the local manifestations of the inherited disease.

have been already referred to, which seem to be more or less closely connected with abnormal development at some definite period of fœtal existence, although the congenital malformation cannot usually be recognised at birth. It is, for example, now very generally supposed that hydrocephalus, especially in its chonic form, is frequently, if not always, the result of cerebral defect in fœtal development. Dr. Battersby,[1] in his excellent papers on this disease, states that "hydrocephalus appears, in fact, to be always congenital;" and that "the period of its origin is always the earliest time of life." In many cases of chronic hydrocephalus, in which the disease is restricted to early life and is fatal to successive members of the same family at the same age, it may without hesitation be referred to congenital defect, dating from the same period of fœtal development. This is confirmed by the fact that hydrocephalus occasionally occurs as a recognised disease of the fœtus-in-utero, and sometimes, as already stated, leads to abortion at the same period in several successive pregnancies. There are, moreover, some well-marked examples of several children in the same family being born hydrocephalic at the full term, such as the case recorded by J. P. Frank,[2] of a woman who was attended by him in her confinements seven times, and who each time produced a hydrocephalic child. The same connection appears also to prevail between the acute form of this disease, limited by age to one period of childhood, and defective development of the brain, limited to one period of fœtal life; and it seems to be in consequence of this that several children of the same family (as Dr. Churchill[3] and almost all writers on the subject, including Sauvage, Ludwig, Cheyne, Odier, Forney, Gölis, Bouchut, and others, have stated) are successively cut off by it, the occurrence of death in these cases at an early age being the almost necessary result of the fatal character of the disease. Well-marked illustrations of this limitation by age occur in a case recorded by Dr. Underwood,[4] in which six children, born of the same parents, were observed to die successively from acute hydrocephalus at the age of two years; and also in a case, mentioned by the same observer, in which two children in another family died suddenly from the same disease before the age of two years. Whilst Mr. Joseph Adams[5] has recorded a still more decided case, as regards the restrictive influence of age, in which three children (sex not stated) in a numerous family "were seized with the disease about the age of puberty, and in all the rapidity was so great that the most powerful remedies seemed scarcely sufficient to protract the fatal issue; those who escaped that age have continued free from the complaint."

[1] 'Edin. Med. and Surg. Journal,' vol. lxxv, 1851.
[2] Quoted by Dr. Battersby, loc. cit., p. 10.
[3] 'Diseases of Children,' 2nd edit., p. 145, 1848.
[4] 'Diseases of Children,' 10th edit., pp. 340-1, 1846.
[5] 'Hereditary Peculiarities of the Human Race,' 2nd edit., p. 20, 1815.

A corresponding connection between unrecognised defects in fœtal development and the occurrence of disease in the faulty structures at some period of air-breathing life appears to prevail also in other organs besides the brain; and it is probable that many of those diseases which appear even at remote periods of life are connected with some defect or peculiarity in fœtal development. With reference to this subject, Dr. West[1] has observed that "congenital malformation of the heart seems to have an important, though not perhaps an easily explicable, influence in predisposing to inflammation of its valves, or of its investing membrane;" and he refers to some illustrative cases of this description which have come under his notice. In like manner, Dr. Daldy,[2] in directing attention to the frequency with which disease of the right side of the heart (consequent on the reduction of the muscular structure of the auricle to something like elastic tissue, not fatty degeneration) is hereditary, informs us that the disease is traceable through two and even three generations; and that, with regard to its connection with fœtal development, the late Mr. W. Aston Key asserted, in the course of discussion at the Hunterian Society in 1832, that "it may be taken as a general rule that diseases of the right side of the heart have a congenital origin." Other forms of disease of the heart besides those in which the right side is affected have in like manner been observed to be hereditarily subject to the restrictive influence of age, but the exact nature and seat of the pathological change in some of these cases are not stated; as, for example, in the case recorded by Sir Henry Holland,[3] in which four brothers and sisters had disease of the heart, and in "all fatal about the same period of life." My own observations on the subject tend not only to support these statements respecting diseases of the brain and heart, but also to extend their application to diseases affecting other organs and tissues; and they have induced me, in common with other observers,[4] to think it probable that the connection between disease in after-life, and congenital malformation of the organ or tissue in which it is located, is far from uncommon, especially in those cases which have been associated with hereditary influence; and to some extent, at least, it may be urged that the hereditary limitation by age of disease in after-life is in itself an indication of the hereditary limitation of the defect, with which it is associated, to one period of fœtal development. The occurrence of such a parallelism in the family history of disease is, moreover, needed to explain those cases in which there

[1] 'Diseases of Infancy and Childhood,' 4th edit., p. 482, 1859.
[2] On 'Diseases of the Right Side of the Heart,' pp. 12 and 34, 1866.
[3] 'Medical Notes and Reflections,' p. 44.
[4] According to Sir Henry Holland, who has bestowed special attention on this subject, "Eneuresis in children, from whatever source arising, occurs sometimes in so many individuals of the same family, as to make it almost certain that it has a common congenital origin."—Op. cit., p. 35.

is evidence of the restrictive influence of age in the morbid development of two or more organs or tissues at different periods of life; as, for example, in the case recorded by Dr. Whitehead,[1] in which a lady, suffering from congenital blindness of one eye, had two sons, who were her only offspring, and both of whom had congenital defect of the corresponding eye, similar in outward appearance to that of the mother. "They were both" (adds Dr. Whitehead) "educated to the medical profession, and both died of consumption before the completion of their studies."

There can, indeed, be no reasonable doubt that in many cases of hereditary disease limited by age, the progressive development of the body, during what appears to be a state of perfect health, is more or less intimately associated with the reproduction of disease; and with respect, therefore, to the date at which in these cases the abnormal change began in the tissue, which ultimately becomes the acknowledged seat of hereditary disease, although it may be referred to as probably coeval with a very early stage of development, it cannot be clearly recognised, any more than it is possible to recognise in the germinating seed the outward development of a tree or shrub, which is subsequently disclosed to our notice in the successive appearance of its characteristic leaves, its flowers, and its fruit. On the other side there remain to be noticed some congenital defects which are due to imperfect development, and which occasionally disappear soon after birth. This is especially noticeable in cases in which there has been simply delay rather than arrest in the development; and it is in consequence of this that cyanosis, for example, when it depends, as it usually does, on the open state of the foramen ovale during the air-breathing life, sometimes disappears spontaneously in a short time; the development of the heart, so far as regards its relation to this abnormal condition, having been completed a short time after, instead of a short time previous, to birth. A well-marked case of this description has been recorded by Richard of Nancy,[2] in which two children, born in succession in the same family, with cyanosis, were completely free from it before the end of the fourth week.

With respect to those hereditary diseases which are more or less obviously developed subsequent to birth, it must, as regards the influence of age, be admitted, before proceeding further in the inquiry, that the parallel evolution of disease in conjunction with certain epochs of life seems to have been recognised from almost the earliest times in the history of medicine, of which we have any definite records; and when, consequently, those diseases which are peculiar to certain ages or states of development show themselves to be

[1] 'On the Transmission, from Parent to Offspring, of some Forms of Disease, and of Morbid Tastes and Tendencies,' 2nd edit., p. 23, 1857.

[2] 'Traité Pratique des Maladies des Enfans,' pp. 136-7, 1839.

hereditary, it was evidently to be expected that the same order would continue to be observed, and that they would therefore be limited to one period of life. In later times it has, moreover, been noticed that certain diseases, such as cancer, vary according to the period of life at which they are developed; so that both the character of the disease and the locality in which it occurs are in some measure subject to the influence of age. In consequence of this, it was to be expected that when, as sometimes happens, such diseases become hereditary, the same influence of age would continue to affect both their transmission and their development. But, beyond this partial recognition of its connection with disease, very little attention has been bestowed on the subject; and as a result of this neglect, it must be acknowledged that, even at the present time, any attempt to trace the influence of age in the hereditary development of the various forms of disease occurring subsequent to birth can as yet be but very imperfectly carried out, and that it would require the co-operation of many careful observers in the different branches of the profession to supply the information required. In some forms of hereditary disease, scarcely anything more than a few vague observations on the subject of age have been made; whilst in other cases in which it would be difficult to overrate the importance of the inquiry, hitherto nothing has been done. It has often been noticed in certain families, that the deaths of children occur in succession at the same age, and it is very commonly supposed that the fatality is referable in many of these cases to the serious disturbance which sometimes characterises the transition periods of development; in the same way that in the physical world the periodic changes of the season are usually accompanied by dangerous storms. This explanation is undoubtedly in some respects quite correct, as in those cases in which the critical period, for example, of the first dentition is commonly associated with danger from disease; and even when, as is not unfrequently the case, the successive deaths in families at such a time cannot be referred to reflex disorders of the nervous system,[1] or to any disease which can rightly be connected with the evolution of the teeth, they may occasionally be ascribed to the increased fatality of other diseases which happen to be developed at that age. Moreover, as the variations observable in certain families with respect to the ages at which the first and second dentitions, puberty, &c. occur, are often subject to hereditary limitation, the diseases referable to these epochs in development are consequently liable to be

[1] The connection between such disorders and dentition is well illustrated in a case, recorded by Dr. Laycock ('Medical Times and Gazette,' April 21, 1866, p. 412), of "Nyctalopia, with partial deafness, in five children of the same family;" in all of whom the defects were first noticed at periods of dentition. There were five other children in the family who, although "living under the same roof," were unaffected.

restricted in their appearance by the same influence; and when such limits, as regards age, vary more than usual from the natural standard, so that the offspring are commonly referred to as being either extremely backward or extremely forward in their development, there seems to be, in some cases at least, an exceptional tendency at such periods to the hereditary evolution of disease.

The limitation by age of hereditary peculiarities in the evolution and decay of the teeth is sometimes noticeable apart from any diseases which may be associated with such periods in development. As an illustration of this it may be mentioned that, besides the cases already cited, there is a well-marked case of the early evolution of teeth recorded by Dr. Montgomery,[1] in which "the two middle incisors of the lower jaw were found projecting at birth," in two children of the same family; and a similar case has been mentioned by Dr. Whitehead,[2] in which a mother and two children (sex not stated) were observed at birth to have the two lower incisor teeth. But it is more common to meet with examples of retarded dentition hereditarily limited by age; and a case of this description has lately been under my own observation, in which a little girl, aged fourteen months, had not cut a tooth. The peculiarity was traced to her paternal grandmother, who had seven children, none of whom had, like herself, begun to cut their teeth before the age of fifteen months. Four of them (three paternal uncles and one paternal aunt of my patient) married, and had amongst them five children (first cousins of my patient), in all of whom the dentition was in like manner delayed till after their first year. Whilst in cases of hereditary decay affecting the teeth, the same influence of age is still more commonly observed, either with or without a corresponding limitation by sex. Among the illustrations of this which have come under my own observation, is a case in which caries, limited to the four upper incisors, and commencing at the age of two years, occurred in five children of the same family. The first-born child in the family is a girl aged fourteen years, in whom the upper central incisors began to decay at the age of two years, and this was quickly followed by decay of the upper lateral incisors. Corresponding decay of the same teeth, at the same age of two years, occurred in the second child, who died at the age of three years; in the third child, a boy, aged nine years; in the fifth child, a boy, aged four years and three months; and in the sixth child, a boy, aged two years and a half. The fourth child was a girl, who died at the age of fifteen months, without any appearance of caries in the teeth; and the seventh child was a female infant, aged four months. All the front teeth of both the parents in this case were sound, and no previous family

[1] 'Cyclop. Anat. and Phys.,' art. "Fœtus," vol. ii, p. 336.
[2] 'Op. cit., p. 12.

history of caries affecting the incisor teeth in early life could be traced.

The early or the late occurrence of puberty, the climacteric change, and the anticipation or the postponement of those changes in development which characterise old age, are in like manner often subject to hereditary influence. With reference to this subject, Prosper Lucas[1] has justly remarked that "there exist families who have fixed epochs for their development;" and whether the members of these families are precocious or backward, such epochs "are often the same in the fathers and mothers and in the children." And when, as sometimes happens, any pathological changes are associated with these physiological variations in the transition periods of development—such, for example, as the development of epilepsy in connection with puberty, or of insanity in connection with the climacteric epoch—they become invested with increased importance, in consequence of the associated disease being, in some cases at least, apparently the result of this exceptional and hereditary change.

It has, moreover, been observed that, independent of dentition and other periods in development, the children in some families are hereditarily liable to disease at the same period of life, and that they die in succession either from the same or from almost any disease which may happen to affect them at a certain age. In consequence of this, it is not unusual for parents, especially in humble life, to lose either some or all of their children at the same early age; and in such cases of mortality in families, it may sometimes be noticed that the restrictive influence of age is associated with limitation by sex.

The occasional importance of hereditary influence in some of the acute specific diseases has been already noticed in connection with limitation by sex; and an illustrative case was cited to show how the fatality from measles might be restricted to one sex in a family. So likewise it would appear that not only the fatality, but also the exemption from this and other acute specific diseases, may often be the result of the hereditary influence of age; and many of the remarkable differences which have been observed in the duration of the stage of incubation could, if carefully investigated, often be traced to the same cause. M. Bouchut,[2] whose accuracy of observation and whose extensive experience in children's diseases entitle him to be cited as an authority, emphatically states that it is evident that children owe to individual predisposition the immunity which they enjoy in the midst of a focus of infection. It is (he

[1] 'Traité Philosophique et Physiologique de l'Hérédite Naturelle,' t. i, pp. 244-5, 1847.

[2] 'Traité Pratique des Maladies des Nouveaux-nés et des Enfants à la Mammelle,' 2nd edit., 1852, p. 597.

adds) an inexplicable phenomenon, it is true, but it is admitted by all authors, and is found justified by a great number of observations made in all epidemics. Alone, it may account for the greater or less time the disease takes to develop itself in children submitted at the same time to the contagious influence."

It would be very useful, as well as interesting, to study with close attention this portion of the inquiry; but, owing to the imperfect and limited amount of information that is possessed on the subject, it is impossible at present to do so, as the comparative effect of successive epidemics of these diseases on the young members of successive generations in the same family, living, as regards endemic influence, either together or apart, has not as yet attracted any special attention. The subject, however, is one which, from its connection with hygiene, may perhaps before long attract more attention; in the mean time it will be allowable to state that there can be no doubt that both the progress and the result of acute specific diseases may be, and often are, controlled to a greater extent than is generally supposed by the hereditary influence of age; so that the younger branches of the same family, instead of suffering from the same disease in the same epidemic, or of suffering from it to an equal extent, escape the disease in one epidemic, and die from it on a subsequent occasion; or they may be observed to bear with almost equal impunity the attacks of different epidemic diseases till they have reached the age hereditarily fatal to their existence, when they yield to the attack of perhaps any specific disease which may happen to prevail at that time, even although the epidemic may be of a milder character than usual. In such cases it would scarcely be correct to speak about the uncertainty of life, for, although short, it could as a rule be maintained for a fixed time; and death on these occasions should be regarded less as the result of disease than of an inherited inability to live beyond a certain age: whilst even in some of those cases in which the usual course of an acute specific disease does not appear to be controlled by the hereditary influence of age, the effect of this restriction may occasionally be traced in some complication of the disease. This was shown in a case, lately under my observation, in which a family of four daughters and one son all suffered from scarlet fever, which at the time was prevailing as an epidemic in their district. In the four younger children the disease passed off favorably without any complication, but in the eldest child, a daughter, aged fifteen years, it was associated with rheumatoid affection of the joints; and it was ascertained that the mother, at the same age of fifteen years, had also suffered in the same way from scarlet fever associated with a similar affection of the joints.

One of the most interesting and perhaps also the most important subjects for consideration in the acute specific diseases with reference

to the hereditary influence of age, is the fact that they are, as a rule, self-protective only as regards the individual, and not as regards the descendants. This, coupled with the fact that there is, moreover, in some cases an hereditary tendency to the reappearance of these self-limited diseases at a later period of life, shows that the protection afforded by their occurrence is liable, like other acquired peculiarities of the system, to be superseded, in the ordinary course of development, by a recurrence to what has been called the normal type. It is probable, however, that the recurrence in these cases is always more or less incomplete, and that the system never fully regains what may be termed its virgin state, with reference to an acute specific disease. For a marked difference has been observed in the descendants of those who have suffered from the disease, and who in consequence are, in some degree, prepared for the attack, compared with others who have never been in the slightest degree acclimatised to the epidemic influence by any previous exposure to it of their race. Sir Henry Holland,[1] in some valuable and suggestive observations on this subject, has noticed that hereditary influence may account for "many seeming anomalies in disease, by connecting them with the more general laws of life;" and with reference more particularly to small-pox, scarlet fever, and measles, he is of opinion that "a constitutional or hereditary tendency exists throughout all these phenomena;—modifying susceptibility—determining, in conjunction with other causes, the regular or irregular course of the disease—affecting its degree of virulence—and giving liability to its recurrence: and that this habit of body is not limited to families, but extends even to larger communities." In the present imperfect state of our knowledge respecting the source, the diffusion, and the varying intensity of successive epidemics of these diseases, it would be impossible to pursue this inquiry to the end. But if, as may be reasonably inferred from the observations already made on the subject, the recurring development of such diseases, not only in individuals, but also in races of men, be subject, like other diseases, to hereditary control, it is obvious that the restrictive influence of age must, to some extent, be liable to affect all the phenomena which mark their progress in individual cases; and that with regard to the collective population of a country or a district, it is probable that the variations in intensity of successive epidemics of the same disease, constituting what is called the type of an epidemic, is in some degree an indication of the extent to which the community is, by previous attacks, protected from its recurrence. It is necessary, however, to distinguish between the protection afforded to an individual and that which is afforded to a community by a previous attack of the same disease; for whilst, as a rule, there is in the individual only a limited liability to the recurrence of the disease, which is progres-

[1] 'Medical Notes and Reflections,' 3rd edit., 1855, p. 89.

sively diminished as age advances by the development of an organisation less susceptible to the attack, there is in the community an ever-returning susceptibility to the disease consequent on the renewal of its youth. This is, in either case, strictly in accordance with the recognised influence of age on hereditary transmission; for although a previous attack in the parents might be expected to modify the disease in the offspring, so that in some respects it would differ in character from that which has appeared for the first time in a community previously free from it, yet it does not, as in the case of an individual, have the effect of checking its return; and hence, through the influence of age, the term self-protective, which has been applied to these diseases, is true only as regards the individual, and not as regards the race.

The cases of disease affecting particular organs or tissues independent of transition periods in development, and apart from epidemic influence, are so numerous, that it will be necessary to limit the illustrations in the first instance to a few well-marked series of cases, and subsequently to notice any other and detached cases as the occasion for doing so may occur. With this intention, it has been decided to select two large groups of cases in which the vascular and the nervous systems are respectively the seat of disease, since the hereditary influence of age in such cases can for the most part be clearly shown to prevail. This mode of investigating the subject will not merely serve to prevent confusion, but will, moreover, be useful in assisting to clear away some of the difficulties which are met with in tracing the hereditary transmission of disease in cases of early death. For when, as is not uncommon in some forms of hereditary disease affecting the vascular or the nervous system, the attack begins early in life and the disease itself proves fatal before the patient has arrived at maturity, there is no possibility of its being directly transmitted; and so far as the influence of age alone is concerned, it would soon cease to exist as an hereditary disease. The frequency, also, with which diseases affecting several members of the same family are either limited in their occurrence to one generation, or recur in succeeding generations in interrupted descent, is chiefly due to the same restrictive influence; and hence an effectual check would quickly be given to any further extension of the disease, if this fatal effect of limitation by age were not associated, as it frequently is, with some other hereditary influence. In consequence of this, it will very soon become obvious in the course of the inquiry that although the restrictive influence of age admits of being to some extent considered by itself, yet, owing to its being frequently combined with limitation by sex and atavism, its effect on hereditary disease cannot be fully appreciated until these other influences in morbid development have been more generally recognised and more fully understood. For, whilst the influence of

age serves on the one side to keep hereditary disease within certain bounds, and often by means of an early death effectually to arrest its direct transmission, the combined influence of sex and atavism on the other side assists in retaining it for future generations, and provides for its return long after its previous appearance has been altogether forgotten.

The large and important class of hæmorrhagic diseases contains many good illustrations of this influence of age, which is well shown in what may be considered the earliest and at the same time one of the rarest forms of hereditary hæmorrhage, namely, that which results from the separation of the umbilical cord in new-born infants. There seems to be in this, as in other forms of the hæmorrhagic diathesis, a geographical limit to its occurrence; for of the eighty-eight cases referred to by Grandidier[1] (some of which are the same cases reported more than once), fifty-eight occurred in North America, fourteen in Great Britain, and twelve in Germany; whilst in France the affection has been very rarely observed. This geographical limitation of the disease has been still further illustrated by the later and more careful researches of Dr. J. Foster Jenkins,[2] of New York, who has collected seventy-seven previously unpublished cases of its occurrence in America, of which fifty-seven, or rather more than 74 per cent., came under the notice of physicians practising in the State of New York. Our information respecting this form of the hæmorrhagic disease has in consequence been chiefly derived from American sources, among which may be specially noticed a paper, containing an analysis of forty-six cases, which was read by Dr. Francis Minot[3] at a meeting of the Boston Society for Medical Improvement in 1852; a paper, containing an analysis of seventy-eight cases, which was read by Dr. Stephen Smith[4] at a meeting of the Society of Statistical Medicine at New York, in 1854; and a report on 178 cases, by Dr. J. Foster Jenkins, of Yonkers, New York, which was reprinted from the 'Transactions of the American Medical Association,' in 1858. From these and from other sources of information, it has been ascertained that in a large proportion of the cases recorded, the hæmorrhage, which is necessarily limited by age in consequence of the time at which the separation of the funis occurs, is limited also to the members of one generation and of one sex. This was specially noticed by Mr Ray,[5] in a paper, published in 1849, on the occurrence of the disease in England; in which he refers to "the peculiar disposition this kind of hæmorrhage has to attack the male sex only, and its liability to occur in a succession of male children from the

[1] 'Die Hämophilie oder die Bluterkrankheit,' Leipzig, 1855, p. 135.

[2] 'Report on Spontaneous Hæmorrhage of the Newly-born,' Philadelphia, 1858.

[3] 'Amer. Journ. Med. Sci.,' Oct., 1852, vol. xxiv.

[4] 'New York Journal of Medicine,' new series, vol. xv, 1855

[5] 'London Medical Gazette,' vol. xliii, 1849, pp. 423—9.

same parents." From the statistical researches of Dr. Minot, it appears that 68¾ per cent. of the children affected belong to the male sex: Dr. J. Foster Jenkins has noted that 65¾ per cent. are males; whilst Dr. Stephen Smith informs us that "sex has an important but unexplained influence, two thirds being males." These statistics, however, must be received with caution, for they show the effect of sexual preference rather than of sexual limitation; and in the following cases, in which there was evidence of the hæmorrhage being hereditary, it will be perceived that the influence of sex was inferior to that of age, although in a large number of cases in which the sex was specially noted the limitation was, as a rule, strictly maintained.

Among the examples of its limitation to the male sex is a case recorded by Mr. Ray, in which three male children in the same family died from umbilical hæmorrhage. They were the second, third, and sixth in order of birth; the first, fourth, and fifth children being females, and free from hæmorrhage. The mother, in this case, after her marriage and two months before the birth of her first child, which was born at the full term, was thrown from a chaise, and bled largely from injuries then received. Her sister had an only child (male), who died of hæmorrhage from the funis very shortly after birth; whilst her brother's children, male and female, were healthy and free from hæmorrhage. In this case, the occurrence of the disease in first cousins of the male sex, descended through the female line from a common ancestor, is strong evidence in favour of atavic transmission. Mr. Ray also records the case of a lady, attended by Drs. Key and Babington, who had lost four male children from umbilical hæmorrhage associated with purpura: there were two female children in the family living and healthy. Wachsmuth[1] relates a case in which three male infants, two of whom were his own and the third was their cousin by the mother's side, suffered from dangerous bleeding after the division of the funis; in which respect the case differs from the preceding, in which hæmorrhage occurred subsequent to the separation of the funis. Dr. Campbell[2] records a case observed by Dr. Maxwell Adams, in which two male children in one family, who were the third and fourth in order of birth, died from umbilical hæmorrhage attended with jaundice; the other two children were females, both of whom were alive and well.

[1] 'Die Bluterkrankheit,' Magdeburg, 1849, pp. 6 et seq. In the above case, for a notice of which I am indebted to Dr. Chas. Drysdale, there was a well-marked history of hereditary hæmorrhage, which, in connection with limitation by age, is chiefly noticeable for the fact that two daughters of a cousin (by the father's side) of Wachsmuth's father-in-law "also suffered from ecchymosis and dangerous bleedings, but not from spontaneous bleeding; and the one, aged twenty years, died from the bleeding caused by the rupture of the hymen on her marriage night. The other also bled to death about the same age."

[2] "Contributions to Infantile Pathology," 'Northern Journal of Medicine,' Edinburgh, vol. i, 1844, pp. 238—240.

Dr. Steinthal[1] has observed a case in which two male children suffered from umbilical hæmorrhage, with slight jaundice and purpura, and recovered: four previous children in this family were free from hæmorrhage. And in a case communicated to Dr. J. Foster Jenkins[2] by Dr. P. C. Sampson, of Syracuse, New York, male twins died from umbilical hæmorrhage: in this case it was noticed that a nephew of their mother had hæmorrhage from the neck at the age of fourteen years, requiring ligature.

With respect to its limitation to the female sex, a case has come under my own observation, in which the third child (a female) in the family of a mechanic had hæmorrhage from the umbilicus on the ninth day after birth, and the sixth day after the separation of the funis. The hæmorrhage, which was considerable and caused much alarm, was arrested for a time by long-continued and well-sustained pressure; there was a slight recurrence of the bleeding on the following day, which was checked by a renewal of the pressure, and the patient recovered. The first child in this family was full-grown and apparently healthy, but had been born dead. The second child, a female, died on the nineteenth day after birth from umbilical hæmorrhage associated with purpura, which began on the twelfth day after birth, the cord having separated on the third day. In this case both parents were young and healthy, but the family history on the side of the mother was unfavorable; for her father had died insane at the age of thirty years, and her mother had died in childbirth with her fourth child, leaving two sons and a daughter. The elder of these sons died of fever at the age of twenty-seven years, leaving two children, a boy and a girl, both free from disease; the younger son has had occasional attacks of hæmoptysis; and the daughter, who is the second child in the order of birth, is the mother of the hæmorrhagic children referred to above. Another and a somewhat more imperfectly marked case is recorded by Dr. W. C. Anderson,[3] in which it is stated that "a mother who had lost a female child from umbilical hæmorrhage with jaundice and purpuric spots, had lost another daughter previously, who, at the age of four months, became icteric, and had purple spots without hæmorrhage, and recovered."

In some of the cases recorded, there is an omission with respect the sex of the patients; as, for example, in a case similar in some respects to that last described, and which came under the notice of the same observer, who states that "a woman who lost a child on the eighth day from umbilical hæmorrhage with jaundice and purpura, had another who died on the fifth day with jaundice and purpuric spots, but without hæmorrhage." A case of this description

[1] Cited by Dr. J. Foster Jenkins op. cit., p. 38.
[2] Op. cit., pp. 44-5.
[3] 'Boston Medical and Surgical Journal,' vol. xli, 1850, p. 442.

was observed by Dr. A. Hooker,[1] in which a woman lost four children from umbilical hæmorrhage, and had a fifth child "which presented a slightly jaundiced look, but no hæmorrhage took place, and it did well." The same observer met with another case, in which a woman "had two children, who are now alive and well, but one of them had jaundice at the age of four or five; she has since had two others, both of whom died of umbilical hæmorrhage preceded by jaundice."

Similar omissions with respect to the sex of the patients occur in a case communicated to Dr. J. Foster Jenkins,[2] by the Hon. J. W. V—, M.D., in which two children in one family died from umbilical hæmorrhage; in a case reported by Dr. A. J. Skilton,[3] of Troy, New York, in which two children in one family died from umbilical hæmorrhage; in a case reported by Dr. John Watson,[4] of New York, in which two children belonging to a numerous family died from umbilical hæmorrhage; in a case which occurred in the practice of the late Dr. Christie in India, and of which Dr. Simpson[5] read a detailed account at the Edinburgh Obstetric Society, on April 13, 1847, from which it appears that two children in the same family died from umbilical hæmorrhage associated with jaundice; and also in a case communicated to M. Roger[6] by M. Danyau, in which this form of hæmorrhage was fatal to twins born prematurely at eight months and a half.

In other cases of this description there is only an imperfect reference to sex, as in a case cited in the 'Medico-Chirurgical Review' for 1834 (vol. xxi, p. 232), in which three children, whose sex is not stated, died from umbilical hæmorrhage, and a fourth child, a male, likewise died from umbilical hæmorrhage, which was associated with jaundice and purpura; there were three other children in the same family who were free from the disease; and in Mr. Pout's[7] case of three children in a family of six, who died from umbilical hæmorrhage, and in which it is stated that one of the fatal cases was a male, but the sex of the other children is not mentioned.

Whilst recognised exceptions, as regards limitation by sex, and noticeable therefore only for limitation by age, occur in a case

[1] 'American Journ. Med. Sci.,' vol. xxiv, 1852, p. 316.
[2] Op. cit., pp. 40-1.
[3] Cited by Dr. J. Foster Jenkins, op. cit., pp. 158-9.
[4] Ibid., pp. 46-7.
[5] 'Monthly Journal of Medical Science,' vol. viii, 1848, p. 70.
[6] 'L'Union Médicale,' tom. vii, 1853, p. 147. With reference to the occurrence of umbilical hæmorrhage in twins, it may be remarked, that besides the cases observed by Dr. Sampson and M. Danyau, which have been already cited, and in each of which both the twins died from the hæmorrhage, a third case has been observed by Dr. Wm. Read, at the Lying-in Hospital in Boston, in which the first of twin children, whose sex is not stated, was still-born, and the second died from umbilical hæmorrhage. (Cited by Dr. Minot, 'American Journal of Medical Sciences,' new series, vol. xxiv, 1852, p. 315.)
[7] 'Med-Chir. Trans.,' vol. xii, 1822, pp. 183-5.

recorded by Dr. Bowditch,[1] in which two children in the same family died from umbilical hæmorrhage, who were the first (female) and the fourth (male) in order of birth; the two intermediate children were males, and free from hæmorrhage: in a case communicated to Dr. J. Foster Jenkins,[2] by Dr. H. W. Dean, of Rochester, New York, in which three children in one family, two males and one female, died from umbilical hæmorrhage with jaundice and purpura; a later child in this family, sex not stated, died from jaundice and purpura without umbilical hæmorrhage; and in a case recorded by Dr. W. C. Bailey,[3] of two children dying from umbilical hæmorrhage in the same family, who were the second (male) and the third (female) in order of birth; the first (female) and the fourth (male) children were healthy and free from hæmorrhage.

With respect to the jaundice which occurred in a large proportion of the preceding cases, it should be noted that its occurrence is far more significant than the simply yellow discoloration of the skin which is frequently observed at or before the same age; and that, besides being associated with umbilical hæmorrhage, it is apt to occur in some families as a distinct inheritance, subject to the influence of age. It must not, however, be supposed that the jaundice referred to in some cases on record was sufficiently important to take rank as an hereditary disease, although writers on the subject may refer to it as such. Morgagni,[4] for example, mentions that fifteen children of his own all became jaundiced soon after their birth, and some of them to a considerable degree. But whilst jaundice of this description might to some extent have occurred independent of hereditary influence, there have occasionally been noticed, in some families, cases of a more severe kind, in which jaundice has been the hereditary cause of death at a uniformly early age. Dr. Underwood[5] relates a very curious case of hereditary infantile jaundice which occurred in the practice of Mr. Pearson. In this case, out of a family of eleven children, in the first nine the jaundice appeared a few days after they were born, and they all died within the period of one month after their birth. The tenth child lived six years, was then affected with jaundice, and died. The eleventh child became yellow on the third day after it was born, and died on the eleventh day after birth. In like manner, also, Dr. West,[6] in referring to the connection between jaundice and umbilical hæmorrhage, informs us that he "knew a lady who lost in succession three out of five children, soon after their birth, with most intense jaundice, which in one instance was ascertained to be associated with

[1] 'Amer. Journ. Med. Sci.,' new series, vol. xix, 1850, p. 63.
[2] Op. cit., pp. 44-5.
[3] 'Amer. Journ. Med. Sci.,' new series, vol. xxiii, 1852, pp. 432—5.
[4] Lib. iii, lit. 48, art. 48.
[5] 'Treatise on the Diseases of Children,' tenth edit., 1846, pp. 168-9.
[6] Op. cit., p. 570.

malformation of the biliary ducts; in the second, no examination was made after death; and in the third, malformation was said, though scarcely on adequate authority, not to have existed."

The frequency, moreover, with which jaundice is associated with hæmorrhage from the umbilicus, contributes to give an hereditary character to cases in which the actual occurrence of the bleeding is limited to one member of a family subject to this severe form of jaundice, especially if purpura be also observed to occur; for the development of purpura in connection with jaundice at this early age is in itself a sign of the hæmorrhagic diathesis. This is well shown in a case which occurred in the practice of Dr. Samuel R. Smith,[3] of a female child in whom the hæmorrhage began on the eleventh day after birth, attended with jaundice and purpura, and was fatal on the twelfth day. Three years previously the mother had had a child, who at the age of four months was affected with jaundice and purpura, but recovered. A somewhat similar case, which was not, however, associated with purpura, has been communicated to Dr. J. Foster Jenkins,[1] by Dr. F. J. Bumstead, of New York, in which a male child died from umbilical hæmorrhage, with jaundice and purpura; "a former (female) child had jaundice three days after birth; recovered, and died convulsed at three months; other children healthy." Mr. Pooley[4] also has lately recorded a case in which umbilical hæmorrhage began in a female infant on the twelfth day after birth, jaundice having occurred seven days previously; and she died on the fourteenth day after birth: he mother of the child in this case had previously given birth to five other children (sex not stated), all of whom were "deeply jaundiced soon after birth," and died in early infancy.

The conjunction of jaundice with hæmorrhage in these cases shows that there is a very close and often perhaps a necessary connection between them, which appears to result from both being essentially dependent on the same cause of disease. With reference to this subject, it should be noticed that whilst on the one side M. Baumes[5] advocates the hypothesis advanced by M. Levret, "that the jaundice so common in new-born children often results from engorgement of the liver consequent on ligature of the umbilical cord,"—the blood in the umbilical vein between the abdomen of the infant and the seat of the ligature being supposed to become, after a time, corrupt,—Dr. Stephen Smith[6] is of opinion that the jaundice is the cause of the hæmorrhage, when the latter is not due to "a transmitted hæmorrhagic or syphilitic dyscrasia." It would, however, be very

[1] 'Boston Medical and Surg. Journal,' vol. xli, 1850, pp. 440-1.
[2] Op. cit., case No. 102.
[3] 'Amer. Journ. Med. Sci.,' 1866, pp. 560-1.
[4] 'Traité de l'Ictère ou Jaunisse des Enfans de Naissance,' 2nd edit., 1806, pp. 20 et 49.
[5] Loc. cit., p. 81.

difficult to trace the connection between syphilis and hæmorrhage from the umbilicus, even in those cases in which jaundice is also present; for not only have there been, on many occasions, intervening children perfectly free from disease, but there has also been a very decided tendency shown to limitation of the disease by sex, which would be altogether inconsistent with its dependence on a syphilitic taint. So that even in cases in which the hæmorrhage has been associated not only with jaundice, but also with skin disease of a suspiciously syphilitic character, such intervention of healthy children, either with or without limitation by sex, would tend to prove that the inheritance had not been derived from a syphilitic source. Mr. Gould[1] has recorded a case of this kind in which umbilical hæmorrhage, associated with jaundice and congenital ichthyosis (?), occurred in a family whose parents were cousins. In this case the first child, a male, who was born prematurely, appears to have had congenital ichthyosis, or some allied affection of the skin, in which there were bleeding fissures, but no hæmorrhage from the umbilicus; he died on the sixteenth day after birth. The next three children in succession were healthy; the fifth child (referred to by the author as the fourth) was a male, born at the full term, and affected with congenital ichthyosis; on the ninth day after birth, umbilical hæmorrhage commenced, attended with jaundice, from which the child died on the twelfth day after birth. The sixth child was a female, who died from umbilical hæmorrhage and jaundice on the eighth day after birth. It may be useful also to notice, with reference to what is liable to occur at this period of infantile life, that hæmorrhage from the umbilicus is not the only danger which attends separation of the funis, in cases in which there is an hereditary tendency to disease of the vascular system; for there is evidence to show that the trismus of new-born infants is a reflex affection which is sometimes due to inflammation of the umbilical vessels consequent on the separation of the funis; and that when, therefore, two or more children in the same family die in succession from this cause at the same early age, the occurrence may occasionally be referred to hereditary influence: although at the same time it must be admitted that trismus in infants belonging to the same family is often, if not even usually, referable to some morbid influence of a local or endemic rather than of an hereditary character.[2]

In the preceding cases of umbilical hæmorrhage, it will be seen that the limitation of the disease to members of the same generation, and even to members of the same family, with the exception

[1] 'Boston Med. and Surg. Journ.,' vol. liii, 1855, pp. 109—112.

[2] "On Inflammation of the Umbilical Arteries as a Cause of Trismus in Newborn Children," by Drs. Levy and Cazalis ('London and Edinburgh Monthly Journal of Medical Science,' 1842, pp. 549-550, from 'L'Examinateur Méd.,' 28th Nov., 1841).

of Mr. Ray's case in which a first cousin was affected with hæmorrhage from the funis, and of Wachsmuth's case, in which the disease in three male infants, two of whom belonged to the same family and the third was a cousin, was traced to a preceding generation, is more strongly marked than in cases of the hæmorrhagic diathesis, strictly so called; in which class of cases, together with an equally decided preference for the male sex, there is also a tendency in the disease to prove fatal at an early age. I am only acquainted with one other case in addition to that of Wachsmuth, already cited, in which the development of umbilical hæmorrhage occurred in a family hereditarily subject to the commoner forms of the hæmorrhagic diathesis. It occurred in the practice of Dr. Elssaesser,[1] of Stuttgard, who informs us that the first-born child in a family of three male children died from umbilical hæmorrhage, which began on the fourteenth day after birth. The second child, at the age of ten weeks, had purpura; hæmorrhage from the gums occurred at the age of eight months; subsequently three was epistaxis, alarming hæmorrhage from slight injuries, rheumatic pains and swellings of the joints; and he died soon after an attack of hæmorrhage from the nose and stomach. The third child, still living at the age of two years, has been subject to petechiæ and ecchymoses. The mother in this case was subject to the frequent formation of petechiæ in her youth, till her sixteenth year, and likewise to frequent epistaxis; and the son of her mother's sister died in his twenty-fifth year from uncontrollable epistaxis, consequent on a fall from his horse. Showing, as Dr. Elssaesser has remarked, that the hæmorrhagic disposition was directly traceable to the female side.

It has been often remarked that in the commoner and more diffused forms of the hæmorrhagic diathesis, members of several generations have become partakers in the inheritance, in consequence of the females possessing the power, which, as yet, has only been exceptionally observed in cases of umbilical hæmorrhage, of transmitting a disease which, as a rule, they do not share; so that in the one class of cases, but not in the other, the hæmorrhage commonly occurs in succeeding generations in interrupted descent. Grandidier informs us that this, according to the latest observations, is so far true in the majority of cases of hæmorrhagic disease, that a perfectly healthy mother is usually observed to give the disease; and in the Swiss families it has been noticed that the children are only bleeders when the mother is of a bleeding family, whether the father be a bleeder or not; on which account the daughters are there called "conductors" of the disease. This difference, however, with respect to the hereditary transmission of the defect, is perhaps more apparent than real; for there are many cases on record in

[1] 'Hufeland's Journal' for February, 1824, quoted in 'London Medical Repository,' new series, vol. iii, 1825, pp. 506-8.

which the hæmorrhagic diathesis, so far as the family history could be traced, was strictly limited to the members of one generation in the same family; and it is probably more due to the comparative rarity of umbilical hæmorrhage than to any essential distinction in its hereditary character, that atavism has not been more distinctly recognised in its transmission, as well as in that of the hæmorrhagic diathesis of the usual character. For, strictly speaking, umbilical hæmorrhage seems to be merely a circumscribed form of the hæmorrhagic diathesis, and, apart from atavism, there would be but little possibility of its being lineally transmitted; since in nearly all of the preceding cases the hæmorrhage was fatal in a few days; whereas in the commoner forms of the hæmorrhagic diathesis the limitation of life is less strongly marked, and the recurrence of the disease in succeeding generations, uninfluenced by atavism, has occasionally been observed.

With reference to the hereditary occurrence at an early age of hæmorrhagic disease, it may be remarked that there are cases of a still rarer form than hæmorrhage from the umbilicus, in which an equally circumscribed and uncontrollable hæmorrhage has been observed; such, for example, as that which results from circumcision performed on the eighth day after birth; and it is important to notice that in seven cases of this description mentioned by Grandidier, in all of which the bleeders, necessarily males, were the offspring of Jews, the separation of the umbilical cord was not attended with any accident; but nevertheless they all succumbed from hæmorrhage consequent on the circumcision. The following case recorded by Grandidier affords a good illustration of the uncontrollable occurrence of preputial hæmorrhage, in which limitation by sex, by age, and by atavism were, as usual in the commoner forms of the hæmorrhagic diathesis, associated together. The account refers to a Jewish family at Kurhessan in Kassel, near Hanover, in which four male infants—each of whom was descended from a different grandchild of a common ancestress, through the female line, four generations back—died from hereditary hæmorrhage consequent on circumcision performed the eighth day after birth. In this case, in which there was, as usual in cases of the hæmorrhagic diathesis, atavic transmission of the disease and limitation by sex, the ancestress (Mrs. H—), from whom the inheritance was derived, had two daughters, one of whom, by marriage, became Mrs. G—, and the other Mrs. W—; and both of them, with their children, were free from the disease. The family of the first daughter, Mrs. G—, consisted of one son and two daughters, all of whom married and had children. The son had only one child, a boy, who died from hæmorrhage after circumcision: one daughter had three children, two of whom were girls and free from the disease, and one was a

[1] Op. cit., p. 33.

boy, who died from hæmorrhage after circumcision: the other daughter had seven children, four of whom were girls, free from the disease; and the other three children were boys, one of whom died from hæmorrhage after circumcision, one is healthy, and the youngest, although a "bleeder," has survived the national rite. The family of the second daughter (Mrs. W—) consisted of one child, a daughter, from whom there descended an only child, a boy, who died from hæmorrhage after circumcision.

In the above-mentioned and circumscribed forms of the hæmorrhagic diathesis it will be noticed that whilst the occurrence of hæmorrhage is necessarily limited by age, in consequence of the lesions resulting both from the separation of the funis, and also from circumcision, being restricted to a certain period of life; yet in a very large majority of the cases recorded of what may be called the common form of the hæmorrhagic diathesis, the first appearance of the disease is found to have been delayed to a somewhat later age, a well-marked preference being shown in many of them for the epoch of the first dentition. The remarkable absence in such cases of any special risk of hæmorrhage consequent on the separation of the funis may be owing either to the time not having arrived for the hæmorrhage to occur, or to the umbilical vessels being exempt from structural peculiarity, such as, in the preceding cases, would have been capable of limiting the seat of the disease, and consequently also of limiting the time of its occurrence. When, moreover, as is sometimes the case, there is evidence of an hereditary development of the hæmorrhagic diathesis at a much earlier period than the first dentition, and altogether independent of the lesion consequent on the separation of the funis, almost the only explanation which is admissible seems to be that which refers to a difference in structure. In the following case, for example, recorded by Dr. Du Bois,[1] in which four sons in a family of five sons and one daughter suffered from the disease, structural peculiarity in development associated with limitation by age could alone account for the parallel sequence of the hæmorrhagic symptoms, which began at a very early date in all of the four children affected, without apparently any risk of hæmorrhage from the funis. In this case it was noticed that the disease began with the appearance of ecchymoses in different parts of the body, a fortnight after birth, and that the same symptoms occurred in all; about the end of the first year, but especially after the third, they were seized with violent epistaxis; coughing produced hæmoptysis, and diarrhœa was attended with clotted blood in the stools. Of the other two children in the family, the girl, who was free from the peculiarity, died at the age of three years from convulsions; and the boy died on the

[1] Quoted in 'Brit. and For. Medical Review,' Jan., 1840, pp. 247-8.

day of his birth from the same cause. As a contrast to the early occurrence of the disease in the preceding cases, it may be noticed that the latest age at which the hæmorrhagic diathesis has declared its presence for the first time by the occurrence of hæmorrhage is, according to Grandidier, twenty-two years. This occurred in a case observed by Steiner,[1] of a father and son, who at that age suffered for the first time from profuse hæmorrhage from the nose, of which both of them died.

The obvious tendency of the influence of age in the preceding cases of the hæmorrhagic diathesis is to check the hereditary transmission of the disease by means of an early death; and if, therefore, as is usual in many cases of hereditary disease, the family maintenance of the peculiarity was chiefly dependent on direct transmission, it would be very quickly lost. But so far is this from being the case that there is evidence on the contrary to show a progressive increase in the number of cases of this disease, beyond what can in any way be accounted for simply by endemic influence; whilst the records of its persistence as a family defect, which are in some cases remarkably complete, show that it extends over a longer period, and affects the members of a larger number of generations than has been hitherto observed, unless exceptionally, of any other hereditary disease. The two oldest families, according to Grandidier, in which hereditary hæmorrhage can be traced, are the American Appleton-Brown and Smith-Sheppard, the earliest news of which, as regards the occurrence of hæmorrhage, are from the year 1720—30, the last in 1806. The two great families at Tenna, in Switzerland, exist, as far as is known, since 1770 as bleeders, whilst their descendants at the present time have this sad legacy. Also in Hughes' case, ('Transylvania Journal,' April, 1832) the disease could with ease be traced from four to five generations; and Kuster's patient assured him that in his family the sons had been bleeders from time immemorial. One cause of this difference in the pedigree of the hæmorrhagic diathesis compared with that of other transmitted disease, is the facility with which hereditary hæmorrhage can be observed, and the difficulty with which it would be forgotten by those who may have a family interest in its occurrence; and, consequently, the influence of sex and atavism combined, in limiting its development to males, and its transmission to females, generally admits of being more readily traced in these than in any other cases of hereditary disease. In addition to which it has been remarked by Grandidier and others, that notwithstanding the extent to which some families are liable to be decimated by this disease, the risk of their becoming extinct is counterbalanced by the female "conductors of the disease" being unusually prolific; so that, according to a considerable number of observations, to which may be added some cases which have come under my own

[1] Cited by Grandidier, p. 91.

notice, the average number of their offspring has been observed to be nearly double that of the normal mean.

It should, moreover, be noticed, with reference to the failure of the influence of age to check the recurrence of the disease, that the combined influence of sex and atavism, which is common to it and to some other diseases, is not invariably attached to the inheritance; and although it could hardly be expected that males subject to the hæmorrhagic disease would be capable of directly transmitting it, even if their life were sufficiently prolonged, still there are a few and exceptional cases in which, owing to the disease having probably existed in a milder form, or having been developed at a later age than usual, certain individuals affected with the disease have lived to grow up and have children; and in these cases there has been on rare occasions direct transmission through the male line, consequent on the extension of the limitation by age. M. Sanson[1] has recorded the case of a man who died from urethral hæmorrhage, and six of whose children had perished from the bleedings from casual wounds. Sir Benjamin Brodie[2] has published a case which occurred at St. George's Hospital, in which a father and son, who were both liable to hæmorrhage from slight causes since infancy, were received as patients. The father died at the age of twenty-seven years, from hæmorrhage consequent on the extraction of a tooth; and when his little son afterwards became a patient at the same hospital, "it was found that he had laboured under the same hæmorrhagic tendency as his father." In a case observed by Steiner, which has been already referred to, the disease began and was fatal in a father and son at the age of twenty-two years. Dr. Steinmetz[3] has also recorded an instance of the same kind in a Jew, aged twenty years, in whose family the disease had been hereditary in the direct male line for three generations, and continued by atavism to the fourth generation. It was first observed in the paternal grandfather, who died from hæmorrhage, after having suffered from many previous attacks of an alarming character. The father was also liable to hæmorrhage from slight causes up to the age of fifty-five years, when the attacks altogether ceased, and no further hæmorrhage occurred during the succeeding years. Dr. Steinmetz's patient had three sisters, all free from the disease, but who imparted it to their male children. It will be observed in this latter case, that besides the usual mode of transmission by atavic descent through three females in the third generation to their male offspring in the fourth, that there was also direct transmission of the disease from paternal grandfather to father and son; and, moreover, that

[1] 'Concours Thesis,' 1836. Cited by Dr. David Hay, 'London and Edinburgh Monthly Journal of Medical Science,' 1842, p. 270.

[2] 'The Works of Sir Benjamin Collins Brodie, Bart.,' 1865, vol. iii, "Hæmorrhagic Diathesis," pp. 668-670.

[3] Rust's 'Magazin für gesammte Heilkunde,' xxvii, 375. Quoted in 'Edin. Med. and Surg. Journ.,' vol. xxiii, p. 439, 1829.

the male ancestor in the second generation outgrew his liability to the disease. Such an occurrence as this of outgrowing the disease shows that the structural defect in the vascular system on which the hæmorrhagic diathesis apparently depends, may be superseded and disappear as development proceeds, even in cases in which the age is much more advanced than in Dr. Steinmetz's case. For Grandidier has observed a case in which the "hæmorrhagic disposition" in the father of a hæmorrhagic family disappeared at the age of sixty-four years, when it was replaced by rheumatic pains and paroxysms of asthma.[1]

The influence of age in affecting these changes, by which the tendency to hæmorrhage in after life is checked, would, in all probability, be more commonly observed if the lives of bleeders were more frequently prolonged, instead of being, as is usually the case, abruptly terminated in childhood or early youth; and although there is still some doubt with respect to the pathology of this disease, yet, if we accept the testimony of the above-mentioned observers, we must also admit that the morbid condition on which the hæmorrhage depends can, as development advances, be superseded or overcome through the influence of age; and subsequently there would be little if any difficulty in understanding the nature and extent of the changes in the vascular system, on which the occurrence of such forms of hæmorrhage in early life depend. For whilst, as regards the normal condition of the vessels, the experiments and researches of Sir Clifton Wintringham have clearly proved the greater proportional density of the coats of the arteries to that of the veins in old than in young subjects, and the consequently greater tendency to arterial hæmorrhage in early life, and of venous at later periods, it has been ascertained by many skilful observers who have had opportunities of examining the condition of the vascular system in cases of the hæmorrhagic diathesis, both during life and after death, that there is

[1] It may be useful to call attention to the fact, that such cases as the preceding are directly opposed to some of the erroneous opinions which have been published about hereditary transmission, especially with regard to the direct inheritance of the hæmorrhagic disease; and they are also equally opposed to the hypothesis which has recently been endorsed by Mr. Moore, in his paper on "Hæmorrhage," Holmes's 'System of Surgery,' vol. i, p. 657, in which, after suggesting that "the diathesis probably exists equally in the male and the female children of a family, although it does not equally show itself," he informs us that "men having the hæmorrhagic diathesis, who marry healthy wives, do not appear capable of conveying the tendency to their offspring." As the cases above referred to, with some others which might be cited, contain illustrations of the occasional transmission of the hæmorrhagic disease in the direct male line, it only remains to state in reply that a part at least of the supposed incapability of men to transmit the disease is connected with the fact, that very few of those who as children are subject to it, ever arrive at maturity; and that its maintenance as a family defect is chiefly due to the combined influence of sex and atavism, in consequence of which its transmission is usually limited to females, and its development to males.

usually a structural deficiency or defect in the coats of the vessels, such as would more or less sufficiently account for the occurrence of the hæmorrhage. In the case, for example, of a male adult, subject since childhood to the hæmorrhagic diathesis, the coats of the temporal artery during life were observed by Mr. Blagden[1] to be very thin, "like a vein rather than an artery;" and after death, a year later, they were found to be "nearly transparent," whilst the trunk of the carotid artery was found to be "of its natural texture, except that there were opaque, white depositions on the outer surface of its inner coat, such as precede ossification." Mr. Wilson, in his lectures of the vascular system, delivered at the Royal College of Surgeons in 1819, mentioned the particulars of a post-mortem examination in a case of the hæmorrhagic diathesis, in which the coats of the arterial system were not more than half their proper thickness. Dr. David Burrows[1] has recorded a case in which the brachial artery and its branches were examined after death, and found to be evidently deficient in the fibrous coat, "numerous pellucid patches being seen all over the artery." Mr. Miller, in a paper "On the Treatment of the Hæmorrhagic Diathesis," which was read before the Medico-Chirurgical Society of Edinburgh in 1842,[2] after admitting that "the blood (in these cases) is not alone to blame," states "that the capillaries and arterial tubes are doubtless deficient in their healthy properties;" and he has cited evidence to show that the arterial vessels in those "who have fallen a prey to the hæmorrhagic diathesis" are "attenuated in their coats," and that "the capillary tunics are not only thin, but weak and easily lacerable." Lastly, Sir Benjamin Brodie[3] made three post-mortem examinations of patients dying from this disease, and found that there was abnormal thinning of the arterial coats; and although in these cases the examination did not extend to the smaller arteries and the capillary system of the skin, yet it is not improbable that if looked for in the locality from which hæmorrhage had occurred during life, some marked deficiency in their texture would also have been observed.

This explanation of the hereditary occurrence of the hæmorrhagic diathesis in early life does not, however, accord with the observations of some distinguished authorities on the subject; for Grandidier informs us that, with respect to structural deficiency in the vascular system, the small arteries and the capillaries have been examined with the greatest care by Virchow and Baerensprung, in some cases of the hæmorrhagic diathesis, and that they have not found any appreciable alteration in them. Whilst Rokitanski and some other

[1] 'Med.-Chir. Trans.,' vol. viii, 1817, p. 227.
[2] 'Lancet,' 1840-1, vol. i, p. 405.
[3] 'London and Edinburgh Monthly Journal of Medical Science,' vol. ii, 1842, p. 573.
[4] Op. cit., vol. iii, 1865.

pathologists of the present day are inclined to support the opinion, which was at one time very generally entertained, that the hæmorrhagic diathesis is due to some hereditary defect in the blood; which, in consequence of what Rokitanski refers to as a primitive anomaly, is deficient in fibrine, albumen, and blood-corpuscles. Such an explanation as this is, notwithstanding the authority which supports it, far from satisfactory; for if, as is very commonly supposed, deficiency of fibrine were the cause of hæmorrhage, it would follow that hæmorrhage would be most liable to occur at those periods of life when fibrine is naturally most deficient, and that it would be least liable to occur at those periods of life when fibrine is naturally most abundant. But such a limitation has not been generally observed, and it appears therefore more reasonable to refer the hæmorrhage in these cases to the condition of the blood-vessels rather than of the blood itself; and anything abnormal in the composition of the blood which has been detected in these cases would have to be regarded as secondary to structural deficiency in the vessels. For if, instead of being looked upon merely as a complicated arrangement of conduits, the vascular system be regarded as essentially connected with the formation of the blood, it would follow that any deficiency in the structure of the one must lead to some corresponding deficiency in the composition of the other.

The influence of age in connection with the development of the vascular system is also shown in the frequent occurrence of epistaxis in boys, who usually outgrow the hæmorrhagic tendency before they arrive at maturity. Such cases as these afford a corroborative proof of the correctness of the opinion that the hæmorrhage in the preceding class of cases is chiefly due to structural deficiency in the vessels; for it is well known that in these cases of epistaxis, many of which are not to be considered as hereditary, the defect may, as development goes on, be made good. And even apart from any limitation by sex in such cases of superficial hæmorrhage, the influence of age, both as regards the onset and also the cessation of the attacks in cases which are decidedly hereditary, is sometimes well marked, as in a case of hereditary epistaxis, recorded by Dr. Babington,[1] in which the affliction invariably occurred at an early age in such of the members belonging to five successive generations as suffered from it, most of whom were females; and although something more than the influence of age was associated with the result, yet it is useful to notice that in this hereditary case all of those females who were liable from a very early period to epistaxis ceased to suffer from it as soon as they had borne children. Exactly the reverse of this, as regards the influence of pregnancy, was noticed by Professor Quadrat, of Prague,[2] in a case of hereditary epistaxis, in which a woman, aged twenty-

[1] 'Lancet,' Sept. 23, 1865, pp. 362-3.

[2] 'Gaz. Méd. de Paris,' 3me sér., tom. iii, 1842, pp. 104-5.

three years, had epistaxis so copious in two of her pregnancies as to cause danger; her mother and also her sisters were in like manner subject to epistaxis. This last-mentioned case possesses additional interest from the circumstance of its being hereditarily connected with the earliest form of hæmorrhage in our series of cases, for the second child of Professor Quadrat's patient died three days after birth in consequence of hæmorrhage from the umbilicus and gums; and it affords a rare but not unique example of umbilical hæmorrhage resulting from the direct transmission of an hæmorrhagic diathesis, and serves to confirm the opinion which has been advanced that hereditary hæmorrhage from the umbilicus is merely a local form of the hæmorrhagic diathesis.

There are in addition to the preceding cases others in which the occurrence of hereditary hæmorrhage is subject to the influence of age, in consequence of its dependence on changes in the system, which can only happen within a limited period of life. As, for example, in cases of excessive hæmorrhage from the womb, either independent of or consequent upon parturition. Among the illustrations of this may be cited a case observed by Professor Quadrat,[1] of a woman, aged thirty-four years, married, but without children, who has been affected since four years with metrorrhagia; her mother and two of her sisters having also suffered in the same way. The dependence of the metrorrhagia in this case on the hæmorrhagic diathesis was shown by the patient having had frequent attacks of epistaxis and hæmorrhage in other ways also. With regard, moreover, to the occurrence of post-partum hæmorrhage, it may be observed that such cases are more often hereditary than is commonly supposed. Dr. M'Cowan[2] has lately recorded a case in which a lady had severe flooding after three successive confinements; her mother having also had "severe hæmorrhage after each of her confinements;" and similar cases have from time to time come under the notice of others engaged in midwifery practice. With reference to the occurrence of this form of hæmorrhage it remains to be noticed as a curious fact that the female members of families subject to the hæmorrhagic diathesis, strictly so called, are not, as a rule, more liable than others to post-partum hæmorrhage; and although, "as conductors of the disease," they often transmit it to successive male members of a numerous family, yet they have themselves no share in this risk of hæmorrhage, in consequence apparently of being personally exempt from any structural deficiency or imperfection in their vascular system. Before, however, concluding this notice of the influence of age on the hæmorrhagic disease, it is necessary to state that the customary exemption of the mothers of hæmorrhagic offspring from any personal share in the disease has not prevented some writers from

[1] 'Gaz. Méd. de Paris,' 3me sér., tom. iii, 1842, p. 104.
[2] 'Edinb. Med. Journal,' vol. ix, 1863-4, p. 956.

assuming that in this, as in other forms of hereditary disease, successive offspring in the same family suffer from the disease in consequence of the mother having suffered from a mental impression during pregnancy. This is very noticeable in the following case related by Dr. André,[1] which has been erroneously supposed to illustrate the influence of maternal impression, and in which there was limitation by age but not by sex:—A healthy couple had two children also healthy. The mother in her third pregnancy experienced a violent emotion, and the daughter of which she was subsequently delivered died of superficial and spontaneous hæmorrhage at the age of ten weeks. The next two children, sons, died in like manner at the age of two months each.

The diseases which result from atheroma, or fatty and calcareous degeneration of the vascular system, show in like manner a natural tendency to limitation by age, which is only noticeable on the present occasion when it is associated with hereditary influence; and when, as sometimes happens, disease of this kind occurs at an unusually early age, it seems to be in many cases specially connected with hereditary influence. The exceptional occurrence also of hæmorrhoids in early life, of which Trunka, in his "Historia Hæmorrhoidum," has collected thirty-nine facts in which children of less than fifteen years were affected with this malady, and in thirty-three of which the age was less than nine years, has been referred by Montegre, Gendrin, and others, to hereditary influence.[2] Whilst, on the other side, the frequent occurrence of hæmorrhoids in mid-life, as a family defect, simply illustrates the hereditary limitation by age, resulting from a natural preference in the disease for one period of life rather than another. In these, and in some other abnormal conditions of the vascular system, in which hæmorrhage is occasionally liable to occur, the effect of hereditary influence in leading to this anticipation of age, corresponds with what has been observed in the case of other diseases, and it tends to prove, as will be subsequently noticed, that hereditary disease is essentially the result of a permanently lowered standard of development.

Before leaving this division of our subject, it will be useful also to notice those conditions of the vascular system which are characterised by either undue slowness or frequency of the pulse in health; for peculiarities of this description have on some occasions been observed to be hereditary. In the case, for example, of two brothers who have come under my own observation, the pulse, otherwise normal in character, ranges from 52 to 56 beats in a minute; whilst Mr. Curgenven has favoured me with notes of a case in which the pulse

[1] Cited by E. Fritz, "De l'Hemophilie," 'Archiv. Gén. de Méd.,' 1863, 6me sér., t. i, p. 596.

[2] Gendrin, "De l'Influence des Ages sur les Maladies," Thèse, Paris, 1840, note, p. 15.

of two sisters in health ranges from 118 to 120 beats in a minute. These two sisters belong to a numerous family, but the rate of the pulse in the other members of it could not be ascertained, except in the case of one of their brothers, a distinguished surgeon, in whom it ranged from 72 to 75 beats in a minute. With reference to these cases, it is important to notice that such idiosyncrasies, in assuming the character of a family peculiarity, show that hereditary influence may, consistently with the maintenance of good health, so far disturb the usual connection between the rate of the pulse and the time of life, as on the one side to reduce very considerably its frequency, and on the other side to quicken it to the speed of infant life.

Art. III.

Functions of the Tympanum. By James Jago, M.D. Oxon.; A.B. Cantab.; Physician to the Cornwall General Infirmary, and to the Truro Dispensary.

(*Concluded from page* 186.)

Part II. *Tympanic membranes, ossicles, &c.*—It has been recently observed[1] by a writer who has earned much distinction by the application of physics to physiology that, "by the side of physical there exists a physiological acoustics which has to investigate the phenomena in the ear itself. That portion of this knowledge which deals with the propagation of sonorous vibration from the ear's inlet unto the nervous distributions in the labyrinth of the inner ear has had much labour bestowed upon it, in Germany that is to say, since Johannes Müller had made a beginning therein; though in all candour we must add that notwithstanding this there has been very little gain in the shape of assured results."

Savart[2] had set the example to Müller of constructing apparatus in imitation of the fundamental parts of the ear, for the purpose of experimentalising on their acoustic import. But the latter not only ingeniously extended and varied such devices, but sought information on the subject throughout the wide regions of comparative anatomy.

Finally, Helmholtz, having the cost of whatever apparatus he might desire defrayed for him, has still further refined upon acoustic experiments by aid of special apparatus; and in giving

[1] 'Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik,' von H. Helmholtz. Zweite Ausgabe, S. 5. (1865. The first edition in 1862-3.)

[2] 'Ann. de Chim. et de Phys.,' t. xxv, pp. 12, 138, 225. References extracted from 'Müller's Physiology.' I have not access to the original essays.

to the world, in the treatise just mentioned, the results of eight years' study, has criticised and brought on the inquiries of such predecessors to the present time. For, though his subject was broader, in aiming at establishing a "physiological foundation for the theory of music," he necessarily discusses the mechanism of hearing.

There are numerous other able works that treat on the physiology of the ear, but I select the above as best characterising the progress of elaborate and subtile investigations into physiological acoustics in general, as well as of the more limited subject which we have under consideration. I make no pretensions to having pursued such a copious series of researches, and want of space, if nothing else, forbids me to reproduce the details of theirs; so I must assume my readers to be acquainted with them, or somehow with the general inductions arrived at by such authors.

In a retrospect of such investigations, though they present very much the aspect of a development of induction in ascertaining the acoustic principles involved in the construction of the tympanum, yet we cannot avoid noticing that in no instance does the later writer adopt the views of a predecessor without some important modification. It is also curious to observe what a variety of modifications such views are susceptible of; whilst the fact remains that views on the subject that are not even alluded to by such physiologists are still propounded and still claim disciples in the ranks of the profession. Under these circumstances it is that I have persuaded myself that it may not yet be impertinent to sum up in this Journal, in a general sense, the acoustic axioms that have in the main been assented to by such diligent inquirers as the above as available for explaining the physiology of the drum, and to see how far their theories or other salient ones are competent to account for all the tympanic acoustic phenomena that may be observed in the human ear, that is, both in the sound and unsound organ.

It has been ascertained that sonorous vibrations travel freely in solids and incompressible fluids like water of uniform density, and though less freely, pretty well, in air such as we breathe.

That they pass with ease from solids to water and *vice versâ*, but only with difficulty from either of these immediately to air or *vice versâ*.

That, however, they readily pass through an intervening membrane from air to air, or from air to water and *vice versâ*. In which case the transition is easier through lax than through tense membrane.

That a fibre or membrane, stretched in air, will receive sonorous vibrations from the air and transmit them to a solid

body to which it may be partially attached, though for the application of this law to the tympanic structures it must remain, thus far, an open question how far the said fibre or membrane acts more effectually in the lax or tense state, and whether it may convey the vibrations to a solid in more ways than one.

Hence it follows that an animal provided with an acoustic nerve spread out on fluid (liquor Cotunnii), that is, with a labyrinth, may hear sounds created within its own body, especially if originating near this rudimentary organ.

That for many fishes a labyrinth alone *may* suffice.

That an animal living in air with a labyrinth covered with little else than skin (as happens with some serpents and amphibia) may enjoy hearing enough for its requirements.

Where, however, the labyrinth, as a precaution against injury, is lodged deep in bone, and where a greater nicety of hearing is needful, a tympanum or middle ear is superadded to the labyrinth or inner ear.

In many reptiles, in birds, and generally in mammalia, the tympanum is found essentially as in man. It has an Eustachian tube as its sole means of communication with the throat. The membranous fenestra rotunda separating it from the cochlea, and at the fenestra ovalis, if not a like membrane, at least the vestibular membrane itself, in contact with the base of the stapes, occupy the only openings that lead from it into the labyrinth. The fenestra is connected by a chain of *three* bones (ossicula), or a columella, with the membrana tympani. The base of the stapes moves glibly to and fro in the fenestra, like a piston in a cylinder, the plane of this stirrup being horizontal. The other two bones are in a vertical plane, the malleus being attached by its long process or manubrium (handle) from towards the upper margin of the membrana tympani through nearly half a diameter, so as to be free to oscillate with it. The middle bone or incus is articulated to the projecting head of the malleus and to the top of the stapes so as to hold a direction parallel to the said manubrium. The rotations of these bones are steadied by ligaments and guided by muscles in such a way that the lax membrane may carry the malleus inward and outward through the extent of its play, without disturbing in any way the perpendicularity of the piston-like movement of the stapes in its fenestra.

In animals where the labyrinth has no fenestra its walls are found to be membranous, cartilaginous, or not of hard bone, as if to afford, through their elasticity, the contained fluid room for the swing of sonorous undulations, and to ward the expansions of the auditory nerve from hurtful concussion. Hence, where, as in man, the labyrinthian walls are of dense bone, except

at the fenestræ, it may be assumed that these membranes allow the fluid the required freedom of movement.

According to this conception it may well be inferred that with dense walls, should one fenestra exist for the introduction of waves of sound, there must needs be another of some elastic texture between the fluid and the air, that they may be able to flow through the mazes of the inner ear. Thus, each of the fenestræ has in its turn been presumed to perform, as its sole office, the said elastic or buffer part; that is to say, in rival theories, which would make one or other of them the sole inlet for sound.

In this way the middle and inner ears, taken together, are so associated that an inward movement of the membrana tympani, by occasioning (if through a less space) a similar one of the stapes, will cause a flow of the whole of the labyrinthian fluid, so that every portion of the expansions of the nerve receives the impulse, and that the membrane of the round window juts outwards. Should now the drum-head move outwards, drawing the ossicles after it, the pressure of the air upon this membranous window would force it inwards and push the whole body of Cotunnian fluid before it, so as to wash all the intricacies of the labyrinth and push the stapes more or less outwards at the oval window. In like manner it may be asserted that any movement inwards or outwards originally impressed by the tympanic air upon the membrane of the round window, through displacing the whole of the fluid, and therefore the stapes, makes the drum-head move outwards or inwards. Thus the membranes of the drum and of the round window, with the said osseous and watery links, may be regarded as oscillating together, and the auditory nerve as receiving on all its distributions the watery impulse, whether it be imparted to the labyrinthian fluid through the round or through the oval window, and the shock as spending itself at the other. Even if we imagine sonorous waves to traverse the drum-head and ossicles in succession without having strength enough to cause them to oscillate as one body, the impulses thus given to either the membrane of the drum, or that of the fenestra rotunda, will be felt by all the intermediate parts and the other membrane in the manner described.

I must remind the reader that this general description of the mutual dependence of the parts of the middle and inner ears upon one another leaves out of consideration how far the results may be modified or annulled by the action of the tympanic muscles upon the ossicles or drum-head. It does not weigh, either, how far the results may be moderated by the condensation or rarefaction of the tympanic air, and, therefore, change

in amount of pressure at the fenestra cochleæ, by an inward or outward excursion of the membrana tympani. For the purposes of the questions immediately before us no error can arise from such omissions.

In strict conformity with the above principles both Müller and Helmholtz maintain that both fenestræ are destined for the transmission of sonorous vibrations to the liquor Cotunnii, agreeing that though their main path is the ossicular, yet that, to avoid waste, the round window utilises such vibrations as pass through the membrane of the drum into the cavity.

Though I do not doubt the validity of the principles from which these physiologists proceed, I shall yet venture, by and by, to question how far they are right in assigning even this minor office to the cochlear fenestra. But in the meanwhile, in the face of such admissions of its fitness for facilitating the ingress of sound, it may be thought by many that the arguments of those, if any there still be, who look upon it as the chief or sole portal, deserve more consideration than was bestowed upon them in the first part of this paper. It will be advantageous to glance at their reasoning, as the doing so may rebut an objection that might possibly be raised against certain observations of my own from which I draw weighty inferences.

Before Müller's day there were physiologists who thus dignified the round window; but, as far as I know, they thought that the object of the tympanum was primarily to protect the membrane of this window and labyrinth from injury. They could have advanced no reason why hearing should not be as good, to say the least, on the removal of the drum-head as before, provided no morbid condition had been set up at or within the fenestra, and a few cases of absence of the drum-head were actually recorded to prove as much. However, more recently, when a large amount of evidence has accumulated to show that rupture, or other destruction of the drum-head, seriously damages hearing, and that this becomes restored on healing of the wound, or even to a great extent by the introduction of damp cotton or a membranous disc to the bottom of the meatus auditorius externus, the idea was started that it was necessary for the drum to be a perfectly closed cavity, because such a cavity intensifies sonorous vibrations by resonance. It had long been known that one's own voice, the act of swallowing, the grating of the teeth, or the sound of a tuning-fork vibrating whilst in contact with them, or with any part of the head, is heard much more loudly if the meatus auditorius be stopped than when it remains open. But when sonorous vibrations have once been impressed upon the substance of the head, they radiate from its surface into air, and from every surface in con-

tact with it. Hence, if the palm of the hand, or the tragus be squeezed over the inlet of the canal, they radiate from the whole of the parietes of the shut chamber into it, and resound therein; and thus vibrations which would otherwise escape into the atmosphere fall upon the drum-head itself and affect it in the usual way. It is clearly a mistake to compare the chamber of the drum with such a one: for what we have to catch are sounds impressed upon the atmosphere at a distance from the body, and such cannot be intensified in such a closed chamber. On the contrary, if we place a tube with a few membranous dissepiments at intervals in it, with one end in the ear, we may hear such outer sounds through it, but we find that as we remove the dissepiments one by one, so we hear better and better until the last is gone. Nor must my own experience of an open Eustachian tube be forgotten; nor that Müller, who was unacquainted with this theory, practically refuted it by showing, in an experiment (presently to be adduced) on a box with three fenestræ, that an open tube being inserted into one side of it, in supposed imitation of a constantly patent Eustachian tube, made no difference in the results. To all this it may be added that Mr. Toynbee saw, latterly, that for an artificial membrane to be of service it must lean against the malleus, or in absence of it, against a remaining ossicle.

Those who try experiments by stopping the external meatus must bear in mind that when the finger or hand is pressed into or over it, there is always heard a rumbling noise which is presumed to be conveyed to the ear from the muscles of the hand and arm, and to be indicative of the rate of muscular contraction, and is so characteristic that it need not be confounded with any other sound. I must also request the reader to notice that the *souffle* made by the entrance of breath or eructated gas into the drum, through the Eustachian tube, does not appear louder when the external canal is stopped. This shows that these *souffles* do not reach the auditory nerve through bone as a conducting medium, but somehow viâ the tympanum. This fact, which I wish to be remembered, may be verified by listening through a tube connecting one external meatus with that of another person: we can hear him swallow, and perhaps a click from displacement of the membrana tympani, but we do not hear the Eustachian *souffle*, though he inflate his tympanum.

Having disposed of the claims of the fenestra cochleæ to be regarded as the principal portal, I shall leave the discussion of the question, how far it is meant for a portal at all, until the claims of the other fenestra have been gone into.

Both Müller and Helmholtz concur in the generally received opinion that the drum-head and ossicles are, *par excellence*,

the acoustic apparatus. The former fortified his induction by the following among other special experiments:—He inserted two membranous windows on one side of a box, and one on the opposite side, connecting this sole one with one of the others by a wooden rod, which was glued to the middle of each of the membranes to which it was attached. He immersed the side with the two fenestræ in water. He found that sound was transmitted from the external air to the water through the sole and either opposite fenestra, but with much greater distinctness through those connected by the rod. And, again, he subjoins:[1] "Comparative anatomy also furnishes proofs of the truth of this statement, for frogs, in which a tympanic cavity otherwise perfect exists, have no second fenestra, or fenestra rotunda, the chain of auditory ossicula being in them the only means of conducting the sound to the labyrinth. In this case the air in the tympanum can scarcely be regarded as an auxiliary, since its sonorous vibrations cannot be communicated in any intensity to the solid parts of the organ. The principal use must therefore be to insulate the small bones of the ear and the membrana tympani."

The proofs of the important part played by the membrana tympani and ossicles are so various and irresistible that I shall pursue them no further. I shall pass on to the consideration of the manner in which their functions are performed. In this respect the views of physiologists are not yet in accord. The hypothesis held by Dr. Thomas Young among others, and adopted by Helmholtz, is most in esteem. According to it the lax and flexible membrane equally supported by air on both sides swings to and fro with the aerial sonorous oscillations that impinge upon its outer surface, and carries the malleus with it, so that the liquor Cotunnii partakes of the pulsations through the stapes. On the other hand, Müller conceived that the sonorous vibrations of the atmosphere, by virtue of a special permeability of animal membrane, get into the substance of the drum-head, in the shape of what are called waves of condensation and rarefaction, and that they pass in the same form into the substance of the ossicles, and so on to the labyrinth, without that the membrane and ossicles suffer any bodily to-and-fro oscillation at all. He concluded from his experiments that for ordinary use the membrane should be lax.

Nevertheless, he thought that it should more freely receive sounds which are in unison with its fundamental note, and in due degree even such as approximate to it; or that by being more or less tensely braced by the tympanic muscles, it might be adjusted for listening to particular tones. In this opinion, however, he was

[1] Op. cit., vol. ii, p. 1265.

not singular, as many who rely upon the other hypothesis are of like mind as to the effects of modified tension of the membrane. Dr. Wollaston had taught that the membrane's habitual functions are fulfilled solely by its facility in modifying its fundamental note. The wavering or concession of his successors upon this important principle seems to me to give some justification for the vitality of the theory. These considerations induce me to introduce here a notice of the theory in its most elaborate form, particularly as the arguments on which it is grounded are plausible, to say the least. It can, in this way, be tested by the same sort of criticism as will be applied to other phases of the theory. Besides, in the nice attention which it gives to the anatomical structure of the drum-head it sets an example worthy of imitation.

The membrana tympani, to adopt Toynbee's[1] description, consists of five laminæ—the epidermoid, dermoid, radiate fibrous, circular fibrous, and mucous layer with its epithelium. The radiate fibres "are attached, externally, to a circular cartilaginous ring, which is received into a groove of the osseous meatus and centrally to the malleus. The most attenuated portion of this layer lies between the posterior part of the long process of the malleus and the circumference of the organ. This lamina is continuous with the periosteum of the meatus.

"The circular fibrous layer is attached to the radiate lamina by fine cellular tissue, and the two structures may be readily separated. It consists, as its name implies, of circular fibres, which are firm and strongest at the circumference of the organ, but towards its centre become so attenuated as to require care in order to detect them. The strong fibres at the circumference form a complete circle, and are attached to each side of the body of the malleus, as well as to the sides of the upper third of the processus longus. The circular fibrous layer is continuous with the periosteum of the tympanic cavity."

Though many anatomists look upon this arrangement of fibres as primarily designed for firmly suspending the malleus in the interior of the membrana tympani, yet the gradations in size and length of fibre are so remarkable as to have suggested to the minds of many that they are intended to respond to different tones, and that the tympanic muscles have the power of tuning a membrane containing such a system for a greater diversity of tones than they could a homogeneous membrane. Mr. Hinton[2] tells us that Pilcher considered the radiate, and Erhard also the circular fibres, to be thus endowed. Dr. Bonnafont, crowning these views, says that the tensor tympani and stapedius muscles act in opposite ways upon the membrana

[1] 'Art. Memb. Tymp.,' p. 6.

[2] 'Clinical Remarks on Perforations of the Membrana Tympani.'

tympani, the first rendering tense the posterior and lax its anterior fibres, whilst the other does the reverse; that the first is many times the stronger because it adapts the shorter posterior radiating fibres for the reception of acute sounds, whilst the longer anterior ones take the grave. He has found hearing to be damaged for acute or grave sounds according as the cases he has met with presented loss of substance in the posterior or anterior portion of the membrane.[1]

Having set before the reader what I believe to be an impartial *résumé* of the present aspect of the question: In what way do the membrana tympani and ossicles affect the Cotunnian fluid with sonorous vibrations? I now propose to inquire whether any of the foregoing hypotheses explain such phenomena as we may observe in our own ears.

Wollaston's ideas as to the effect of tension upon the acoustic functions of the membrana tympani did not proceed from refinements in anatomy. He had remarked that in different individuals the scope of hearing might differ much, and that in individuals who could hear a like range of notes, one might be able to discern lower notes and one higher notes than another. In studying the cause of this difference he discovered that his own ears could be rendered insensible to grave sounds. "I remarked," he says, "that when the mouth and nose are shut, the tympanum may be so exhausted by forcible attempt to take breath by expansion of the chest, that the pressure of the external air is strongly felt upon the membrana tympani, and that in this state of tension from external pressure the ear becomes insensible to grave tones, without losing in any degree the perception of sharper sounds."[2]

Again, he says, "In my endeavour to ascertain the extent to which this kind of deafness may be carried, some doubt has arisen from the difficulty of finding sounds sufficiently pure for the purpose. The sounds of stringed instruments are in this respect defective; for unless the notes produced are free from any intermixture of their sharper chords, some degree of deception is very liable to occur in the estimate of the lowest note really heard. I can, nevertheless, with considerable confidence say, that my own ears may be rendered insensible to all sounds below F marked in the bass cliff." After instancing a

[1] 'Traité théorique et pratique des Maladies de l'Oreille et des Organes de l'Audition, par le Docteur J. P. Bonnafont,' pp. 10—23 (1860). I have taken the liberty of transposing the words anterior and posterior in his concluding remark, as it is plain from the context there is a clerical or typographical error in his book. Mr. Hinton has been unable to connect deafness to particular sounds with particular loss of substance in the membrane.

[2] 'Phil. Trans.,' 1820, p. 307: "On Sounds Inaudible by certain Ears," by W. H. Wollaston, M.D., P.R.S.

few ordinary grave and sharp sounds which may be resorted to for tests, he adds: "In the same manner, in listening to the sound of a carriage, the deeper rumbling noise of the body is no longer heard by an exhausted ear, but the rattle of a chain or loose screw remains at least as audible as before exhaustion."

In my earliest trial of this experiment I fell into partial error, and though rather from impatience of the somewhat disagreeable feeling in the head attending the exhaustion than from any difficulty inherent in it, yet the fact makes me feel the force of the above caution. I have now most carefully repeated the experiment, and with the most various sorts of sounds; and as the results I have obtained are of an undeviating character, I cannot doubt that they may be depended upon. Like Wollaston, I can so exhaust my drums that both Eustachian tubes, if I refrain from swallowing, remain closed for minutes, so as to give me ample time to make observations in comfort. Having decided that any one who can distinguish one note from another cannot confound the tone of a wire with a nodal (sharper) one yielded by a section of it, I have for convenience' sake made the greater part of my observations on a piano-forte of seven octaves. Wollaston conjectured that few persons would be able to "effect so high a degree of exhaustion" as one so long practised in the experiment as himself. Since he does not notice that sounds not only differ in tone but in loudness (not to say instruments differ in pitch), it is possible that this fact may have something to do with the margin between his experience and mine. However, though I can perceive no incapacity in myself for complete success in such an experiment, I must state that with my utmost efforts I quite fail to approach his results: I cannot thus extinguish the very lowest note of the piano ($2\frac{1}{2}$ octaves below the said F). All that happens is a striking reduction of its loudness as heard upon the normal ear. It becomes an exceedingly feeble muffled sound. But not only is this tone muffled, but every one through the whole seven octaves, though the degree of muffling gradually diminishes from the lowest note up to the highest. If not too near to an organ, I might possibly make my ears insensible to its very lowest tones. I have not neglected to try with this instrument, and can affirm that the result is essentially the same as with the piano. I may encounter a weak grave sound which I cease to hear, and a strong shrill sound which I cannot perceive to affect me less loudly; but generally it is true to say that the loudness of all is lessened from the lowest to the highest, whilst the highest certainly never appears louder, and no sound before inaudible by its sharpness becomes audible by the exhaustion.

Müller,[1] in confirming Wollaston's description, points out that a precisely similar effect may be produced by forcing, with shut nose and mouth, air into the drum, so as to press tight the membrane by internal condensation. My trials of this experiment yield results in all respects parallel to those obtained by the exhaustive method.

I would direct attention to the hesitating language in which these two eminent physiologists hint at the probability that acute sounds may be more clearly heard upon a tense than a slack membrana tympani: "the rattling of a chain or loose screw at least as audible as before," says the one, and the other, "the ticking of a watch is heard at the distance of eight feet quite as distinctly when the membrana tympani is rendered tense as in the natural state, perhaps even more distinctly, while all grave dull noises in the street cease to be heard." According to both authors, it is because the fundamental note of the membrane is greatly raised that grave sounds wane and sharp ones escape such a fate, or, may be, grow louder. Yet is it not remarkable that so faint an affirmation of a possibility is all[2] that either can utter in behalf of what should have been a very easy observation to verify, had their theory been well founded? For nothing is clearer than that their tightened membranes (Wollaston's especially, who believed the range of audible notes to be determined by the degree of tension of the drum-head) should have endowed them with a power of hearing sounds so acute as to be inaudible by the lax membrane. As both were plainly conscious of this fact, it is strange that their inability to achieve such a feat did not raise doubts in their minds as to whether their notion as to the augmented loudness of acute sound was not mere fancy, originating in a preconceived hypothesis which ought to be reconsidered.

I am so persuaded that my version of the results of these experiments will be found to be just, that I reiterate that both exhaustive and condensative stretching of the membrane impedes the hearing of *all* tones, and more so as they are due to larger waves. According to this interpretation, they are fatal

[1] Op. cit., vol. ii, p. 1260.

[2] Dr. Bonnafont (op. cit., pp. 22, 23) makes Müller, though advocating the cause of a lax membrana tympani, say that "to perceive the sharp sounds it must undergo degrees of tension proportionate to their degrees of sharpness;" and having himself obtained a corroborative result from a mechanical device, adds, "Wollaston, who made such numerous experiments on hearing, observed the same phenomenon, and as he possessed the faculty of making a vacuum in the cavity of the drum, and consequently of relaxing or stretching its membrane, he admits that when the membrane was relaxed he could perceive no sharp sound, whilst when it was stretched he became deaf for grave ones." This puts into Müller's work on Physiology what he never wrote, and makes, I care not to think what, of Wollaston.

to all auditory theories that involve the idea of accord between a tone to be heard, and the degree of tension of the membrana tympani, or any part of it. If we could conceive the multitudinous fibres in the membrane as sufficiently insulated to be free to vibrate each to its own tone, we might understand why low sounds may cease to be heard by rendering the whole of the fibres tense; but we could not understand why we should not make acquaintance with new supremely high sounds, nor in any way why the tones throughout seven octaves should simply be made dull but not inaudible.

In fact, these experiments should teach us that we never enjoy the privilege of hearing through the membrana tympani or any thread in it vibrating after the mode of musical strings or membranes—that is, in virtue of a given degree of tension. Besides, in addition to what has been said, "if an elastic body be set in vibration by any tone, it vibrates in accordance with the exciting tone; so soon as the exciting tone ceases, however, it yields its own tone."[1] We have no such experience, hence the membrana tympani must be lax.

To me the cause of the phenomena of these experiments is, in a general way, obvious. By an excess of pressure upon one or other surface of the membrane, or by an excessive tension, an obstacle is put in the way of those deviations of surface that constitute sonorous waves. The graver the sound or the greater the deviation, the more effectually it is quelled. The almost infinitesimally minute waves of the highest audible sounds scarcely require room, so that their play is not appreciably checked by such means. This explanation palpably squares with the theory which makes the membrana tympani oscillate laterally, carrying with it the ossicular columella. However, it is not clear that it is inconsistent with Müller's theory, because there must be partial displacements of surface where waves of condensation and rarefaction affect a body, and these would be greater for grave sounds; and it is comprehensible that undue lateral pressure upon the membrane may tend to suppress them, or that mere tension of the membrane may hinder such oscillations of its substance as constitute sonorous waves in it, and therefore particularly the greater waves.

In order to gain a further inkling of the way in which the vibrations pass from one of the media in question to another, I passed a wire (a silver ear-pick) into one of my ears until it touched the membrana tympani. I tried it upon the concave surface, and then blew out the membrane against it and passed it over the convex surface, thus changing the site and form of the surface, to guard against the bare possibility of my feelings

[1] Helmholtz, op. cit., p. 216.

deceiving me as to actual contact being effected. I found that the act of gently carrying the end of the wire over the membrane produced a sound as of the rubbing of fine parchment. If I passed my finger and thumb, no matter how delicately, along the wire, the fine friction was loudly heard, and a watch in contact with the end of the wire seemed to tick very many times louder than when this contact was barely just broken. As soon as I withdrew in the least degree the wire from the membrane, the loudness of the friction-sound and of the ticking dwindled below that yielded by the same rubbing between finger and thumb and by the watch, at the same distance from the ear, when no wire occupied the meatus.

This experiment evinces that the membrana tympani will receive sonorous vibrations from a solid rod, and (a fact incompatible with the fibre theory) a given tone at any point of its surface. Reciprocally, we may assume that it would have imparted any sonorous vibration that might have been otherwise excited within it to the wire. Among other modes of trying the experiment, I lay on the other ear so as to allow the wire to rest upon the membrane with its own weight, and with the watch against its side, as well as with it at its end, and still the result mentioned was obtained. It is plain that the wire could not in every case have oscillated bodily up and down. It must have imparted its waves of condensation and rarefaction to the membrane. Hence, reciprocally, the membrane could impart *its* waves to the wire, and therefore, inferentially, it can transmit its vibrations in the same manner to the ossicles.

Besides, this experiment shows that loading the membrana tympani does not prevent its proper action. It oscillated, in this case, if it does so in the to-and-fro sense at all, strongly enough to carry with it both wire (weight half that of a threepenny piece) and ossicles. Such a wire not filling the meatus does not prevent sounds from the external air traversing it, though it detracts from their volume. Such sounds are heard on whatever part of the membrane the wire rests, and therefore the preceding statement embraces sonorous movements thus produced. From this experiment I conclude with confidence that aërial sonorous vibrations so interpenetrate the membrana tympani, that it can perform its functions without lateral vibrations, even though it should not prove that it never performs them in that fashion.

Having thus far obtained insight into the conduct of the membrana tympani, I ask myself the question:—If the aërial sonorous undulations make their way into the substance of the membrane, may not the precise constitution of the outer surface be of much acoustic moment?

To test this question, I syringed my ear with water to the bottom, and then held the auditory canal downwards for a few minutes to let most of the water leak out. This done, I lay down on a couch with the other ear towards the pillow, and, with my head a little rotated on the occiput so as to slightly lift the face from the pillow, I could, with the syringed ear, hear my watch ticking at arm's end, and loudly so at half that distance. I then carefully turned the side of my face towards and below the horizontal, so as to get the membrane at the bottom of the canal. I felt the water as it trickled from the walls of the canal slowly crawl over the membrane with a weak rumbling sound. The ear was, as soon as this was completed, so deafened that the watch only became audible again by being brought into actual contact with the pinna. By inclining my head a little, I felt a dragging or creeping movement on the membrane, as the water, gravitating away, and attracted by the walls of the canal, broke its continuity of surface with a distinct smack-like sound; when my hearing power was at once restored. I obtained this alternation many times in succession. I repeated the experiment also on separate occasions, and always with the same result, and even succeeded several times with the head erect by water attracted by or gravitating downwards on the vertical drum-head. The deafening was plainly producible by the merest film of water on the membrane; whilst the membrane acted fairly in the intervals in spite of its recent wetting. It is manifest, too, that the flexibility or elasticity of the membrane could not have been deteriorated by the presence of the water; and from the modes in which the experiment succeeds, and from what has been said above as to the effects of loading the membrane, that it was not by its weight the water impeded hearing. It was, if I may so speak, the existence of a cuticle of water upon the membrane which caused the damage, by presenting to aërial undulations a surface which repelled them.

It would not do, then, for the membrana tympani to be coated externally with a watery lamina. Accordingly, instead of being furnished with sudoriferous glands which in states of profuse perspiration might cause the membrana tympani to be covered with water or vapour, it is provided with ceruminiferous glands, which keep the membrane dry and the meatus clean.

This much being decided as to the outer surface, it is to be remembered that the membrane really does present a watery surface towards the tympanum. Insomuch that for acoustic consideration it has another lamina besides the five mentioned awhile ago—for the epithelium of the mucous lining is covered with its mucous secretion. This mucous membrane is described as being incomparably smooth and compact for such a structure, so

as to have rather the appearance of a serous than a mucous membrane; and more or less the membrane bears the same character on all the parietes of the drum. Such a secreting surface would, during life, yield a very pure mucus, flowing constantly in just such quantity as to keep the epithelium covered with an even tegument, a glassy watery face—any excess being removed through the Eustachian tube, by means of the minute ciliæ which may be discerned on certain portions of the epithelium.

It would follow as a corollary to the observations just recorded, that the innermost face of watery fluid must be a bar to the transition of sonorous vibrations between the membrana tympani and the tympanic air. Such a fact, if established, leads to important information as to the functions of the structures whose uses we are endeavouring to settle.

It seems impossible to experimentalize directly upon the interior of the drum without running the risk of a permanent injury to hearing. However, there are opportunities afforded by disease of getting insight into the question that now interests us. By far the most common of aural maladies is acute catarrhal inflammation of the mucous membrane of the drum—a usually mild ailment self-curable within a few weeks. If diligently observed, it yields acoustic phenomena which are full of instruction. It was my meditations upon such as I had experienced in my ears that prompted me to the present inquiry.

Having learned to be more discreet than to sit long with uncovered head in a doorway or window through which an unduly heated room is supplied with cold air, it is now eight years ago since I had an attack of tympanic catarrh, though within a few years I had it four times. Of the symptoms attending it on the last three occasions I took notes, and it is only with respect to these that I speak. It was the left ear that suffered on two occasions, and once it was the right. The duration of the complaint varied from four to seven weeks, and was not complicated with any other disturbance of health. A tinnitus began in the ear, and grew to be more and more audible, as with equal pace the organ began to fail in its power of hearing external sounds and to daily grow worse. In a word, just as the tinnitus increased, so did the deafness. When within a fortnight the tinnitus had reached its maximum, so had the deafness, until my watch could not be heard unless it touched the ear. The tinnitus was plainly the noise of the circulation of the blood in its vessels, for I could at pleasure augment its amount by bodily exercise, or any other mode of exciting the heart's activity. Besides, I can by a strong voluntary contraction of the abdominal muscles, and by thus, I presume, checking the course of blood through its aorta, produce a faint soft *souffle* in my healthy ears,

especially in the left ear. Whilst the deafness existed this *souffle* could be rendered much more distinct, and at the same moment the tinnitus also.

But what I particularly regard as noteworthy is the fact that after the maximum of tinnitus and deafness had been reached, both, with any given rate of pulse, remained for weeks unchanged—day after day absolutely *in statu quo*. To this I must add another noteworthy fact, that without any warning the complaint finally terminated *instantaneously*. In the ear was heard a loud rip or crack; the tinnitus had exploded, and the hearing had been re-enthroned.

It is obvious from this history that for weeks antecedent to recovery there had been no unusual energy of the aural vessels, but that throughout the prolonged *in-statu-quo* condition there had been established a modification of the acoustic mechanism which produced this singular effect:—It rendered the vascular circulation which in the healthy ear is unheard, audible, and, in an equal degree, rendered the atmospheric sounds that the normal organ hears, inaudible.

A watch applied behind the pinna, or the act of rubbing the skin there, was heard as well upon one ear as the other, so that the labyrinth was unaffected by the complaint.

Again, in each case when the symptoms had for a certain while assumed the constant type, and I had with a probe found the membrana tympani naked, by means of a silk handkerchief wrung out of hot water and wrapped round the probe I removed all the wax from the external meatus to the very bottom, not shrinking from well wiping the membrana tympani. I hoped to accelerate cure by arousing the mucous membrane of the drum to renewal of healthy secretion, and with this view applied the counter-irritation as close to it as possible. In two cases the treatment seemed to be effectual, for within three days of the operation the complaint vanished by explosion. In the last case it was tried early in the complaint, and did no good. This attack lingered the longest. In each case the canal was so thoroughly cleaned, that a week or more elapsed before any wax at all could be found in it. But its absence had no effect upon the tinnitus or deafness, nor upon the purity of hearing on its sudden recovery. These facts demonstrate that the disease did not affect the meatus. However, as it was not in the inner or outer, it must have been in the middle ear.

To limit its whereabouts in the drum we have several facts to guide us. The Eustachian tube was permeable, because I frequently blew the breath into the drum, and exhausted it, just as far as may be done in its healthy state, of air; and, at least as far as the *in-statu-quo* condition was concerned, these operations

were as easy and as effectually displaced the membrana tympani as in the sound organ. Besides, every bubble of gas that arose from the stomach pressed into the drum and caused a momentary jutting outwards of the drum-head. These acts furnish us again with an acoustic lesson which must also be treasured up:—The Eustachian puff attending the quick current of air through the tube in health is very faint compared with that heard in this complaint. Here, as the breath is forced through the tube, its walls seem to tear asunder, and a crackling is heard, immediately followed by a loud *souffle*. Gas from the stomach enters the healthy drum with a *souffle* only just audible to one who has learned to know it; yet in this complaint the tube is loudly ripped open by it and a loud puff assails the ear. It is one of the annoyances attendant on the complaint, and it was through this circumstance that I have become aware of the entrance of gastric bubbles into the drums.[1]

As confirmatory of the distinction already pointed out between the Eustachian and a series of noises, such as that of swallowing, which, through traversing the substance of the head, loudly resound on closure of the external meatus, it is found that in the deaf ear these cannot be heard at all, whilst the others, unaffected by the closure, are so loud.

Now, assuming that in the *in-statu-quo* condition of the complaint the Eustachian tube was not only permeable but normal, a disease exists which makes the ear more sensible of the Eustachian whiffs and tympanic vascular noises, whilst it deafens it to all sounds that fall upon the external surface of the membrana tympani.

To these observations I may append, that in my last attack on my left ear the tympanum was often affected by a feeling

[1] Since the first part of this article has been in type, my attention to the conduct of the eructative gas has been reawakened. I am now satisfied that I underrated the liability of the drum to its intrusion. It always enters the drum strongly enough to *palpably* give a momentary protrusion to the membrana tympani, whilst on its subsidence, by the force of atmospheric pressure, this returns to its place. This draught of air sweeps the drum more thoroughly than is done in the act of swallowing, and, notwithstanding its origin, is plainly not injurious, but probably useful in moving the secretions. It may often be felt to enter without generating an audible *souffle*. It must have a certain velocity to do this in the healthy ear. Now, in belching, the rima glottidis is closed by being drawn up under the root of the tongue, precisely as in deglutition. Though solid masses gravitate so as to escape by the mouth, ascending bubbles usually glide *behind* the posterior arch of the palate into the nares, so as actually to be passing through the pharyngeal region at the instant the glottis is raised to provide room for them, and therefore when the Eustachian tube is open, and exert at the same time a much greater pressure than the barometric. The correlation between the glottis and Eustachian tube should be thus expressed:—*The Eustachian tubes are shut whenever the glottis is open, but whatever act shuts the latter opens the former.* I should anticipate that the muscles that drag up the rima under the tongue, that is, towards the tubes, must have some share in preserving the correlation.

precisely like muscular twitching, which tugged at the drum-head; it came on in fits during the last weeks of the complaint. I could often excite it by a continued friction with the hand over the skin beneath the ear, or by taking out in a cool place a pellet of wool which had been worn in the ear. I fancied that it was one or more of the tympanic muscles which had been provoked by over-irritation or constraint. But what I would comment upon is that this twitching was attended with a sharp sound, a sort of crack, and that though the twitching did not subside until several days after the cure, yet the sound attending had vanished at that instant. I have in and out, at distant intervals, felt this sort of twitching within the drum in a healthy ear, but the noise it yielded was none or the very faintest. I mention this observation, but not being quite certain as to the origin of the sound, I will reason as if this observation had not been made; though if it be the tympanic muscles, we have another sound generated within the membrana tympani intensified, whilst those impinging upon it from without were deadened.

I think that there is evidence enough before us to fix upon the membrana tympani as the seat of the alteration in the acoustic mechanism which alone is competent to produce such a singular result. But before I attempt it, I will detail a few observations made upon a deafened ear, which few will suspect to have been affected more deeply than the drum-head.

Wishing to ascertain what acoustic effects might be generated by solid plugs resting against the membrana tympani, I passed a thread through some chewed brown paper and tamped this plug firmly upon the membrane. The ear was deafened, but no vascular noises were heard, nor were the Eustachian sounds augmented. The plug drawn out by the thread was coated with wax. I then pushed wax from the sides of the meatus so as to coat the membrane with it, and, as regards the noises, with a similar result. Again, with the view of verifying or otherwise some experiments upon the effects of conveying sounds by solid conductors to the membrane, a few weeks since I ultimately proceeded so far, by using larger and larger conductors, as to rub off much of the wax from the side of the meatus, and much to irritate the drum-head, so that I made it very tender. I syringed and wiped away the wax from the drum-head as perfectly as possible, and left the ear to itself.

But what I think more worth marking is, that after this ill-treatment, on each occasion, the ear got deafened for about a week to nine days. From about the day after the manipulation, a tinnitus attacked it, and with it hearing was dulled; and this proceeded until my watch could hardly be heard at four inches

from the ear, and only loud talking was audible. The deafness had its period of unalterability: in the one case it finished *instantaneously* by a small cracking sound; in the other it seemed to disappear by three stages; the hearing being found better on three successive days, until it was quite restored. In these cases the contrast between the relative audibility of sounds generated without and those generated in or within the membrane was precisely as in the cases of deafness from cold. It was my right ear I experimented upon, and at once, as the vascular tinnitus and deafness ensued, so did the Eustachian tube seem to rip open, and its *souffles* to blow loud.

Nothing could justify the prolixity of these tales but the belief I have that they furnish us with unique information for throwing light upon a very shy problem.

We have here a group of phenomena which proclaim that the mechanism of the drum may undergo such a modification as to convey less thoroughly to the labyrinth sounds originating without the drum-head, and more thoroughly those originating within it. The instantaneity of the restoration of the machinery to its normal action precludes us from thinking that in the morbid condition it had been altered in more than one part, and if we had no other inner exaggerated noises to account for than those generated in the Eustachian tube, whilst the cause that occasioned the exaggeration also blunted external atmospheric sounds, we should be obliged to settle upon the drum-head as the site of the change. For if we were to imagine the Eustachian sounds to find an entrance to the labyrinth at the fenestra cochleæ, the better adaptation of this for the admission of sonorous vibrations could not hinder those from the external meatus from finding ingress if they travel by way of the ossicles, and should favour their ingress if they traverse the tympanic air. This remark has equal force as to any other portion of the tympanic parietes except the drum-head, regarded as possibly carrying the Eustachian sounds to the inner ear.

As to the ossicles, it is clear that no change could happen to them, and that nothing could be applied to them (affecting their mobility or what not) which could render them a better conductor for Eustachian sounds and a worse one for external ones. But the remarks in the preceding paragraph leave us no alternative but to conclude that the Eustachian sounds really do get to the labyrinth by way of the membrana tympani and ossicles —a conclusion that everything we know of the functions of the tympanic structures corroborates. Thus we derive, with what I regard as certainty, that the inner surface of the membrana tympani had been so changed in its character as to be more easily affected than the normal structure by the Eustachian

noises, and therefore by all sonorous vibrations impinging upon it from the tympanic air; and that the same change rendered it less fitted for propagating to the ossicles the vibrations falling upon its outer surface.

We cannot conceive that the morbid action had removed something from the inner face of the membrane which had been restored with the suddenness and crack as of an electric spark. On the contrary, something added by that action must have been then removed. Guided by what visibly occurs in the case of another mucous membrane exposed to atmospheric air—the Schneiderian—I explain the events as follows:—The mucous membrane lining the drum-head is thrown into a state of inflammation by cold penetrating it from the external meatus or thereabouts, or other irritation there applied, and exudes an unusual amount of mucus, and that of an unhealthy sort. This ultimately becomes too much and too viscid to drain away or be swept away by ciliæ, and dries on the membrane, where it remains protecting the irritable surface from the air whilst the inflammation subsides. When the tone of the membrane becomes restored, it may be aroused to secrete, or will of its own accord secrete, again its usual fluid under the false pellicle, so as to loosen it from its adhesion, until some accidental movement of the membrane causes it to peel suddenly off. It falls to the bottom of the drum, where its presence is harmless, to be redissolved and carried off through the Eustachian tube, or to be absorbed. The membrane is now again covered on its inner surface with its lamina of mucus—a watery cuticle.

In the morbid condition, this pellicle of concrete mucus assimilated the acoustic qualities of the inner face of the membrane to those of the outer. Thus, the Eustachian clicks and puffs, and (?) the twitches of the tympanic muscles, sounds that were scarcely adequate for penetrating the healthy watery cuticle, easily passed into the dry one to be carried in an annoying proportion to the labyrinth; whilst the sonorous vibrations of the atmosphere which were retained in the membrana tympani by its inmost face of water pass through it in such great proportion, to be wasted in the drum, that they no longer are stored up for their proper destination.

If this solution be right, ought the tinnitus to be associated with the inner group of sounds? The membrana tympani is liberally furnished with blood-vessels which chiefly occupy the dermoid and mucous layers. Those in the former pass downwards from the roof of the meatus to the centre of the membrane, thence radiate to its circumference, and also communicate with the very minute vessels that are in the latter. These vessels are often seen much enlarged in diseased states of

the ear. Hence, though in the normal ear no tinnitus is heard from them, we should not be surprised to find that they contribute such a noise in disease. However, whether they are or not active agents in this respect in the early part of an inflammation, they must be passive, if agents at all, in the long period of unchangeability preceding the explosion of the complaint. The hypothetical pellicle might, by the contraction it would undergo in drying, or even getting into a pasty consistence, squeeze upon the little vessels in the mucous layer, so as to narrow[1] their calibre in places, and thus produce very fine sharp pipe-notes. On the peeling off of the pellicle these would disappear as the vessels would regain their normal appropriate liberty. But on the supposition before us, any vascular tinnitus that might radiate into the drum from its parietes, though inaudible with mucus on the membrana tympani, would be caught up by the foreign pellicle. And further, should such a pellicle, as is most likely, line other portions of the mucous membrane of the drum, not only would it create *bruits* in such vessels by pressing upon them, but such *bruits* would pass through the dry pellicle into the drum, though they might not penetrate a watery pellicle. In the minor deafness from direct irritation of the bottom of the external meatus, the tinnitus wholly consisted of mild sharp pipe-notes; in the height of the other attacks there were superadded loud bee-buzzing, sea-wave roaring sounds, which must have issued from vessels of far larger diameter, and, I have no doubt, were projected into the tympanic air from the walls of the cavity. In the minor deafness, my conjecture is that rude friction upon the membrana tympani and adjacent portions of the meatus provoked its mucous membrane to an unhealthy secretion beyond the ability of ciliæ to remove, and the tinnitus could not depart nor the hearing return until the inspissated exudations were separated from it.

If my theory of the importance of a watery coat upon the epithelium of the drum to the mechanism of audition have foundation, we can perceive that the fact of its existence makes the fenestra rotunda unadapted for the propagation of sound to the labyrinthian fluid. Its use must be confined to affording space through its elasticity for the play of the sonorous vibra-

[1] I can at will produce a *souffle* in my temporal artery by a due pressure with a finger in front of the tragus, and which is much louder if the meatus be stopped. But the internal carotid pierces the very bone in which the labyrinth is placed without its circulatory current being heard, and the arteries which feed the bony walls of the tympanum *ramify* through the periosteum supported by the mucous membrane without a noise resulting That is because the blood is never forced with disproportionate quickness through a *narrower* channel. To compress an arterial branch or twig is to cause a noise.

tions that enter at the other fenestra. The said fenestra, moreover, lies at the bottom of a recess which is placed behind the promontory, and in a line with these is found the tympanic orifice of the Eustachian tube; an arrangement which shelters the fenestra from its puffs and clicks, and generally from sounds circulating about the drum. Should it ever have been desirable that the fenestra should give passage to such vibrations from without as have affected the tympanic air through the drum-head, it could not have been made available for such a purpose, as it would then also have admitted the tympanic noises. Thus, too, the intricacies of the mastoid cells are destined for stifling sonorous vibrations straying about the drum. And we may add, that none of the ossicular muscles arise within the drum, but their fleshy parts are sheathed in bony estuaries which only open into the drum by minute orifices through which the tendons issue into the main cavity in order to reach the malleus or stapes. Hence there is a careful provision against the conveyance of the sounds of the contractions of these muscles to the auditory path. On the other hand, the drum presents a concavity on its outer aspect, at the bottom of which swings the malleus; and the mucous membrane does not adhere to it throughout, but is reflected over this and along the other ossicles. Thus by the shape of the membrane, and comparatively massive form of the head of the malleus and the length of its handle, the mucous membrane, or rather its watery coat enclosing the membrana tympani and ossicles, forms a sort of compressed watery funnel for concentrating the vibrations on their road to the oval fenestra.

This view of the sundry tympanic functions, and particularly the observations made upon the tympanic noises are confirmatory of the decision already arrived at, that sonorous impulses are not communicated to the liquor Cotunnii by a bodily oscillation of the drum-head and ossicles; for it is inconceivable that the appropriate flexibility of the membrane could have been improved by the adherence of a foreign substance to its inner surface: at all events, that by any means whatever it could have been so changed in texture or in control as to be more easily impressed with to-and-fro oscillations by aërial impulses from within, and at the same time less easily by those from without.[1]

For more than eight years I have seized upon every opportunity of testing the foregoing exposition of the tympanic functions upon such appropriate cases of aural disease as have fallen in my way, and I have never met with a case whose

[1] See 'Proc. Roy. Soc.,' Jan. 23, 1858, for my first enunciation of the acoustic use of the tympanic mucus. It is only more carefully elaborated here.

symptoms have been inconsistent with it. I have also read through numerous such cases reported by other observers; and though some of these present apparent anomalies, and many omit to mention whether certain symptoms were present or absent, this survey tended to confirm me in my views.

I am, of course, aware that the instantaneous explosion of the symptoms in tympanic catarrh has been regarded by Toynbee and others as due to the reopening of a long-closed Eustachian tube—and, indeed, as a proof that the deafness was occasioned by its closure. However, not only am I sure that the tube was pervious in the instances argued from, but I have rarely met with a patient so affected who could not inflate the drum, much less who could not have it inflated for him. Politzer's method of inflation will soon convince aurists of this fact. The tube, nevertheless, may become occluded, though this is more likely to occur when the tympanic is an extension of a naso-bronchial catarrh. It is questionable if the tube can remain a great while impervious without such an accumulation of secretion or unhealthy exudation taking place in the drum, as in denial of exit through the tube will penetrate the drum-head. A few short remarks on the application of the acoustic theory here advanced to aural disease, and I have done.

According to Toynbee, and as is probable, the tympanic air becomes absorbed when the tube is impervious. Such a disease, if the mucous lamina of the drum remain unaltered, will be attended with a greater deafness for grave sounds than sharp, and will not have tinnitus as a symptom. Puncturing the drum-head would prove a temporary relief.

Of tinnitus as a symptom it is somewhat difficult to speak accurately. There may be intracranial noises, and even nervous or mental. These must be diagnosed upon other principles. But confining our attention to the ear, we must remember that there are many arteries about it, and that the partial narrowing of the calibre of any one may produce a noise, if the same may not happen in a vein. However, large plugs of wax may exist in the external meatus without tinnitus; and so may accumulations of effete epithelium, or an eczematous fluid, or perhaps a polypus. At other times in such condition there is a tinnitus; yet it does not follow that because a tinnitus disappears within a few days of the cleansing of the canal, it might not have been due to the unhealthy secretion on the inner face of the membrana tympani excited by irritation of the outer. It is with the unhealthy condition of the mucous layer of the tympanum that tinnitus is more especially associated. I can well imagine that there may be cases of persistent tinnitus where it would be well for the patient that the drum-head were removed

and a substitute supplied. A disconnection in the chain of ossicles would cause deafness; and if the idea be correct that an artificial membrana tympani benefits in some cases of imperforate membrane by bringing the separated ossicles into contact, I should suppose that the most fitting support would be one that would not prevent the atmosphere from playing upon the natural membrane—an elastic thin cloth, or coiled spring, for instance, rolled on its edge, and retained at the bottom of the meatus by pressure against its wall.

An absence of a portion of the drum-head away from the malleus, and uncomplicated with other aural injury, may only diminish the hearing power in proportion to its size. But it commonly happens that a chronic catarrh of the tympanum accompanies the complaint. The escape of the discharge through the hole in the membrane, which is for the most part situated at or towards its bottom, leads to its external face acquiring more or less completely a liquid coat, and thus in this way among others to deafness. In such an example as this, I should say that, where any considerable portion of a still healthy membrana tympani may remain, any kind of plug which would cover the hole or fill it so as to protect the drum from the unwarmed air reaching it through the external meatus, and to divert, if I may so speak, the catarrhal secretion to the natural outlet into the throat, would answer better than an artificial membrane that keeps off the atmosphere from the remaining portion of the natural membrane. An artificial membrane supplying the place of one entirely lost is not very likely to give rise to tinnitus, for it contains no blood-vessels in its substance, and not being accessible to weak sonorous vibrations, is not very prone to admit such as may impinge upon it from within, and which may have radiated into the drum from its parietes. In the few attempts to supply the place of a missing portion of the drum-head, before Mr. Yearsley detailed the benefits he had seen to accrue from the use of moist cotton-wool for that purpose, or since then to invent a substitute for the membrane itself (as a disc of vulcanized india-rubber by Toynbee), no close imitation of its essential qualities has been accomplished. From not being understood, they have not even been aimed at. It would require, perhaps, some ingenuity to devise a very flexible disc which should be more accessible to sonorous vibrations on one side than the other, and which might be properly placed so as to stay in contact with the ossicle. I am not sure that a cottony disc with one side dipped slightly in some waxy material might not be better than the vulcanized india-rubber; a cottony or mealy surface would permit a free contact between it and the atmosphere. In the total absence of mem-

brana tympani and ossicles, it may happen that the unhealthy state of the mucous membrane of the drum rather diminishes the attendant deafness than otherwise—that is, by depriving the membrane of the watery lamina which was designed to repel sonorous vibrations. In these cases, astringent injections, by drying the lining membrane, may give some relief. I fear it would be a vain dream to hope that in some stray case an artificial membrane carrying a columella might be so placed that the free end of the latter might rest against the fenestra rotunda, should it remain more or less intact by disease, and thus convey sounds that way to the auditory nerve.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

Member of the Royal Collage of Physicians, Physician to the St. Pancras and Northern Dispensary, and to the Eastern Dispensary, London.

On the Milk Cure. By Philip Karell, M.D., Physician to the Emperor of Russia.—In this paper, which was read in March, 1865, before the Medical Society of St. Petersburgh, and has been translated into English by Dr. Garrick, Physician to the British embassy at St. Petersburgh, the author strongly recommends the use of milk in the treatment of several diseases, although he by no means declares it to be a panacea. He also considers that the numerous recoveries made by this treatment are in great measure due to its judicious employment, and to strict observance of method in its administration. Dr. Karell commences his subject by adducing the recommendations of milk as a remedy by many ancient and modern physicians. He then states, as the result of his own experience, that milk is the best and surest of remedies in all dropsies, in asthma when it is the result of emphysema and pulmonary catarrh; in obstinate neuralgia, when its cause lies in the intestinal canal; in diseases of the liver, as simple hypertrophy and fatty degeneration, and generally in diseases where there is faulty nutrition, as a consequence of obscure subacute inflammation of the stomach or intestines, followed by affections of the nervous centres. Even in cases where the dropsy is the result of organic heart disease, or of old-standing liver complaint, or of far-advanced Bright's disease, Dr. Karell has seen very marked improvement from the use of milk. In answer to the objection that milk is a well-known remedy, and that every physician uses it in appropriate cases, Dr. Karell asserts that the cure by milk *scrupulously administered, and in strictly measured doses*, is not sufficiently and only very rarely recognised as a sovereign and useful remedy. The peculiarity of his treatment consists in his employing it *alone*, and forbidding all other kind of nourishment. He at first prescribes for the patient three or four times daily, and at regularly observed intervals, half a tumbler or a

tumbler, namely, from two to four ounces of skimmed milk, and when it is found that it is well digested, as is proved by the fæces becoming solid, the dose is gradually increased. During the second week two ordinary quarts are generally administered each day. Dr. Karell has never observed the same satisfactory results when other substances, such as beef-tea, bread, and water, were administered at the same time. At the beginning of the treatment, the patient's bowels are frequently constipated, which is to be regarded as a favorable sign, as it shows that the fluid parts of the milk are absorbed; and the constipation may be relieved by warm water injections, or by the use of castor-oil or rhubarb. Several cases are related in detail, in which the milk cure was successful, and Dr. Karell has the notes of 200 cases which he has thus treated, the greater number being cases of gastric disease. Fatty degeneration of the arteries, leading to apoplexy, he regards as peculiarly well suited to the milk cure, and fever is no contra-indication to its adoption, although the utmost caution must then be exercised.—*Edinburgh Medical Journal*, August, 1866.

On the Use of Styrax liquidus in the Treatment of Itch. By Dr. W. Schultze.—The experiments of Dr. Schultze were made in the section of the hospital appropriated for itch, in Magdeburgh, and the preparation used was a mixture of Styrax and olive oil. In carrying out the rubbing, which is to be performed over the whole body, with the exception of the head, great care must be taken that no folds of the skin are passed over. Baths are to be used only at the beginning and the end of the treatment for the sake of cleanliness, but are unnecessary in the treatment itself, and washing should also be forbidden. Twenty-four hours after a careful rubbing, the acari taken out of the itch-furrows were all dead. Styrax does not cause any irritation of the skin, and it has no direct therapeutic effect upon the itch eczema, the latter getting well of itself by the expectant treatment, and the itching disappears. Dr. Schultze, in reference to the resemblance in chemical composition between styrax and the Peru and Tolu balsams (also used in treating itch), makes some original remarks on the similarity of therapeutical effects produced by bodies of similar chemical composition. All the substances lately recommended for itch, as balsams, ethereal oils, benzoin, and petroleum, owe their efficacy to volatile hydrocarbonaceous compounds, and their therapeutical effects depend upon the length of time that these bodies are retained on the skin. Benzoin and petroleum are therefore less efficacious than the ethereal oils; but the balsams are better than these, and especially the balsam of Peru. In the experiments made with styrax by Dr. Schultze, the duration of the residence in the hospital lasted two days in twelve cases, three days in nineteen cases, five in seven cases, six in two cases, and seven in three cases.—*Schmidt's Jahrbücher der Gesammten Medicin*, August, 1866.

On the Use of the Sulphite of Magnesia in the Treatment of Zymotic Diseases. By H. R. de Ricci.—M. de Ricci thinks that the want

of success which has sometimes been observed in the treatment of zymotic diseases by the alkaline and earthy sulphites, is attributable to the fact that these remedies have not been administered early enough. If the treatment is too long delayed, the blood becomes so loaded with poison and deteriorated in quality as to be no longer able to perform its normal function, and then the sulphites are of no more service than any other remedies, because they cannot restore to life the dead blood-corpuscles. The sulphites should therefore be administered early, while still a large portion of the blood is in a healthy state, and capable not only of carrying on life, but of throwing off what has been rendered inert by the presence of the sulphurous acid. M. de Ricci attributes another source of failure to the administration of hyposulphite of soda, instead of the sulphites, and especially the sulphite of magnesia. The hyposulphite of soda is less efficacious than the sulphites, because in the former the greater part of the acid becomes oxidized in its passage through the animal economy, and appears in the urine as a sulphate, because, being a salt of hyposulphurous acid, it is a less active anti-zymotic; and because it often causes troublesome diarrhœa, while the sulphites of soda and magnesia never produce such effects. M. de Ricci prefers the magnesian salt for internal administration, as it is less unpalatable, and contains a larger proportional quantity of acid than the soda salt; but he uses the sulphite of soda for external application, because, from its greater solubility, a stronger lotion may be made with it. The sulphites of potash, lime, and ammonia are also active anti-zymotics; but they are in no way superior to the salts of magnesia and soda, while their very noxious taste renders them objectionable. M. de Ricci relates some cases illustrating the efficacy of the sulphites, and he concludes his paper by predicting that eventually the treatment of zymotic diseases by the administration of the sulphites will be as fully recognised as that of ague by cinchona.—*Dublin Quarterly Journal of Medical Science*, November, 1866.

On the Use of Hypodermic Injections in the Treatment of Disease. By R. B. Maury, M.D., Port Gibson, Miss.—This paper relates chiefly to the hypodermic use of quinine in malarial fevers, and contains the results of experiments made by Dr. Maury while he was surgeon in the Confederate army in charge of a general hospital. The cases were twenty-five in number, of which six were bilious remittent, five quotidian intermittent, thirteen were tertian, and one a double tertian. The instrument used was the ordinary Wood's syringe, carefully graduated and found to carry thirty minims. The part selected for the injection was usually the cellular tissue of the arm, about the insertion of the deltoid, but sometimes the forearm was selected. The preparation used was a perfectly clear solution of the sulphate of quinia, in water, with a sufficient quantity of dilute sulphuric acid, and sixty mimims of the solution contained eight grains of quinia. In most of the cases six grains was the entire quantity of quinia used during an intermission or remission, but in

the severe cases eight grains were used. Cinchonisin was fully produced in from forty to sixty minutes. It is thought that the hypodermic may be preferable to the ordinary mode of administration —1, when the economy of quinia is an object of importance, as the hypodermic method requires only about one third of the quantity necessary to be given by the mouth; 2, in cases of fever when the stomach is so irritable that remedies cannot be used in the ordinary way; and 3, in the congestive forms of fever where absorption by the stomach is slow. In two cases only of the twenty-five abscess occurred, attributable to a repetition of the injection in the same place, and therefore in other cases the subsequent injections were made in the forearm. Dr. Maury does not claim for his method of treatment any more constitutional effect than may be accomplished by the administration of the alkaloid by the mouth. He has also had under his care a very aggravated case of neuralgia of the sciatic nerve and its branches, in which relief could be obtained only by the administration of large doses of morphia, the after-effects of which, however, were so unpleasant that the patient hesitated between enduring the pain or taking the remedy. Under these circumstances the hypodermic injection of sulphate of morphia was performed, one grain being used for the injection, and the result was relief from pain without nausea or constipation.—*American Journal of the Medical Sciences*, October, 1866.

On the Inhalation of Atomized Medicated Liquids in the Treatment of Diseases of the Respiratory Organs. By JOHN HART, M.D., Boston, Mass.

After describing the history of medicated inhalations in the treatment of disease, and the apparatus by which they may be applied, Dr. Hart considers the question whether the atomized liquids penetrate into the air-passages, and he answers it in the affirmative. Not only have Demarquay, in France, and Friber, in Germany, proved from their experiments on some of the lower animals that the inhaled liquids pass into the air-passages, and even into the parenchyma of the lungs; but Demarquay further found that in a nurse who inhaled some tannic acid through a canula, by which she was obliged to breathe, the presence of the acid was detected in the trachea by a paper which had been moistened with a solution of chloride of iron. Two other cases are referred to by Dr. Hart, proving the same fact; in one, the solution of sesquichloride of iron was inhaled for hæmorrhage, and iron was detected after death in the lungs, and in the other, the solution was inhaled for hæmorrhage occurring during Bright's disease, and iron was also found in the lungs after death. Dr. Hart observes that inhalations of atomized fluids have been used more extensively in chronic than in acute diseases, because in the former every variety of treatment is often successively employed; and he thinks that diseases of the pharynx will generally yield more readily to this treatment than those of the larynx, bronchi, and lungs, probably because the application is more direct. But the remedial effects of inhalation are not constant, and

are often not immediate, the diseases sometimes seeming at first to be aggravated. The diseases in which Dr. Hart has employed the medicated inhalations are affections of the nasal passages, for which he has devised an instrument; diseases of the mouth and pharynx, both local and constitutional, in which the atomized liquids have been used with the most brilliant success; diseases of the larynx, which are treated very successfully, especially if they are of recent origin; croup, diphtheria, œdema of the glottis, which have been treated successfully by Barthez, Fieber, Trousseau, and others; hooping-cough, which has been successfully treated when other means had failed, by Fieber, Wedeman, Gerhardt, and Siegle; diseases of the bronchi, especially simple bronchial catarrh, which, if it has not become inveterate, is almost invariably cured or improved; asthma, emphysema, phthisis, and hæmoptysis. Asthma depending on other affections, and the purely nervous form of the disease, are both treated with success by the inhalation of atomized fluids, and although Dr. Hart does not speak from his own experience as to their efficacy in emphysema, he quotes the opinion of Dr. Wedeman in their favour. That physician states that they have a decidedly beneficial influence in bronchiectasis, emphysema, and other asthmatic affections, and that at Gerhardt's Klinik the most urgent symptoms were removed by their use. Dr. Hart purposes to make the application of the inhalations to the treatment of phthisis the subject of a separate paper, and he speaks very favorably of the use of hæmostatics conveyed by inhalation into the air-passages in the treatment of hæmoptysis. He concludes his paper by giving a list of the most important substances which may be used for inhalations, and among them are opium, morphia, hyoscyamus, conium, sesquichloride of iron, tannic acid, alum, corrosive sublimate, &c. The dose cannot be given with exactness, as it must vary with circumstances.—*American Journal of the Medical Sciences*, October, 1866.

On the Physiological and Therapeutical Action of Narceia. By Dr. A. Eulenburg.—Dr. Eulenburg employed in his experiments the hydrochlorate of narceia in the dose of one-sixth to one-half a grain when administered internally, but in that of one-eighth to one-fourth of a grain when used in hypodermic injection, in which form of application the successful results of the treatment were well observed. He never saw any unfavorable symptoms after these doses, such as headache or gastric effects, which usually follow the use of morphia when given in corresponding doses. Among the physiological effects of the operation of narceia, the most striking and most constant is the diminution of the frequency of the pulse, with a corresponding weakening of the flow of blood, and sometimes a subsequent slight acceleration of the pulsations. The respirations were sometimes temporarily retarded, and occasionally they were rather accelerated, but generally no effect was observed on the temperature of the skin. On the extremities of the sensitive nerves in the skin, narceia acts in an analogous manner to other narcotics; but Dr. Eulenburg could discover no effect upon the urinary organs. As to its therapeutical

effects, he employed it with good results as a sedative and hypnotic in the most varied diseases where local irritation, attended with great pain or general excitement, required the use of narcotics. Its operation was also favorable in certain cases of peripheric neuralgia, and in one case of hysterical convulsions with spastic contractions, in which form of disease morphia is of very little use. Narceia is, therefore, a very valuable remedy in all those cases in which morphia is either not tolerated from the beginning, or in which it has lost its effects from long use. — *Schmidt's Jahrbücher der Gesammten Medicin*, August, 1866.

On the Hypnotic Properties of Narcotics. By Dr. FRONMÜLLER, Sen.—1. *Hyoscyamus.*—In the experiments relating to this substance, Buchner's and Merk's extracts were employed. *Buchner's* extract was found most efficacious in doses from one-fifth to one-half a gramme (a gramme is about fifteen grains), but larger doses were uncertaiu in their effects. In sixty-three different cases of illness, comprising twenty-two of tuberculosis, the hypnotic effect was completely produced in twenty-one, partially in nineteen, and not at all in twenty-three. This property of hyoscyamus cannot be compared with that of Smyrna opium, but it may be compared with that of some of the inferior kinds of opium. The stools were not lessened by the administration of hyoscyamus, but the secretion of urine was diminished. Swelling and dilatation of the pupil were often observed. The pulse and the respiration were somewhat retarded, and the temperature in the night was rather increased.—The extract of *Merk* was employed in doses of one-tenth to one-fifth of a gramme. Three of the persons who were the subjects of experiment were suffering from inflammatory diseases—one from tuberculosis, and three from other complaints. In three cases the results were decisive; but in others they were incomplete, and in some there were no effects at all. The narcotic property of this drug is, therefore, very striking. Dr. Franmüller considers that hyoscyamus, the especial relation of which to the vagus nerve is indubitable, is a hypnotic of the third order, and that its properties are displayed when it is desirable to allay the irritation of coughing.

2. *Cannabis Indica.*—The experiments on this substance were made chiefly with Merk's alcoholic extract, and that of J. Bell of London; but some were made with purified resin of cannabis, and some with the powder. Out of 1000 cases in which the experiments were made, there were 387 of tuberculosis, and in the greater number of instances sleep was produced an hour after the drug was administered. The chief results of the experiments have been tabulated in the following manner by Dr. Fronmüller:—1. Indian hemp belongs to the hypnotics of the first order, without causing great vascular excitement, stoppage of the secretions, or any specially unfavorable after-consequences; but it is weaker and less certain in its operation than opium. 2. No constipation is caused by its use. 3. It is well adapted for alternate use with opium when the latter fails.

3. *Lupulin.*—With this substance, which is regarded as a narcotic,

Dr. Fronmüller made two experiments, but in neither was any hypnotic effect produced, and he therefore excludes lupulin from his category.

4. *Solanin.*—Obtained from *Solanum nigrum* (black nightshade) and *Solanum tuberosum* (potato). The white crystalline preparation of this substance excites, when moistened, an acrid smell like that of potatoes, and a nauseous taste. With sulphuric acid it gives a blood-red colour, passing into brown. When given in doses of one to four grammes, it caused slight burning in the throat and eructation: the pupil was dilated only in one case. In larger doses it produced disagreeable effects, such as vomiting, loss of appetite, giddiness, &c., but did not act as a decided soporific, and no remarkable hypnotic operation is therefore to be expected from its use.

Daturin, *Atropin*, *Nicotin*, *Coniin*, and *Aconite* are not suitable hypnotics, owing to their dangerous properties. Coca, according to Dr. Fronmüller, is nearly inert.—*Schmidt's Jahrbücher der Gesammten Medicin*, June, 1866.

On the Use of the Lignum colubrinum in Intermittent Fever.—By Dr. Berdenis van Berkelow.—This author reports, in a Dutch medical journal, upon the successful employment of the *Lignum colubrinum* in intermittent fever. The first experiments were made with some wood obtained from the East Indies, and its antiperiodic properties were so well marked, and its identity with the *Lignum colubrinum* was so fully proved by comparison with other specimens, that Dr. Berdenis was led to institute experiments with the commercial *Lignum colubrinum* in a series of cases of intermittent fever. The cases, which were twenty-two in number, included both quartan and tertian fevers of different duration and in persons of different ages, and the medicine was given in decoction, and in the majority of the cases (which had generally been treated without effect by large doses of sulphate of quinine) the result was favorable. Chemical investigation detected brucia in considerable quantity in the wood, and strychnia in smaller proportions, and consequently, although Dr. Berdenis has never observed any injurious consequences from the use of the wood, he recommends that it should be cautiously employed. But he thinks that the *Lignum colubrinum*, in consequence of its cheapness, may deserve a recommendation as a febrifuge in pauper practice. He insists, however, that the kind of wood employed by him has great hardness, a yellowish grey colour, and a thin marbled bark, and ought to be well distinguished from the yellowish brown porous kind of *Lignum colubrinum* with a thick bark found in commerce.—*Schmidt's Jahrbücher der Gesammten Medicin*, May 24th, 1866.

On the Therapeutical Properties of Narcein. By Dr. Oettinger.—Dr. Oettinger performed sixteen experiments with solution of narcein on healthy persons, the drug being injected beneath the skin, and in some cases administered internally, at the same time, in the dose of half a grain to one grain and a half. All the experiments showed that the injected solution caused severe burning pain,

and an inflammatory swelling remained for a long time after, passing into suppuration. The experiments also showed that narcein acts by hypodermic injection as a sedative and anæsthetic, but not so certainly or in the same degree as morphia or atropia. Dr. Oettinger has also had the opportunity of observing the therapeutical effects of narcein in six cases. They were cases of neuralgic pain in the hip, angina pectoris dependent on arterial ossification and emphysema of the lungs, cancer of the pylorus, persistent cough from broken rib, emphysema of the lungs with hypertrophy of the heart, and bronchial catarrh. In the first case the narcein was injected beneath the skin, and it produced severe burning pain in the spot where the injection was applied; but it allayed the neuralgic pain. In the other cases the narcein was given internally, and the results were sometimes, but not invariably, favorable. Dr. Oettinger has arrived at the conclusion, from the result of his observations, that narcein acts almost as a pure hypnotic, without causing any previous excitement. The phenomena it especially induces are, muscular weakness, sleepiness, and, in a slight degree, obtuseness of sensation; and also, in small doses insufficient to cause narcotism, it causes retardation of the pulse. At to its internal use, Dr. Oettinger thinks that narcein, which in his experience is at least four times weaker than morphia, ought to be specially recommended in those cases where it is desirable only to cause sleep, and not to allay pain. In cases where opium and morphia, on account of their action on the stomach, and on account of the stimulant properties of the former and the narcotic powers of the latter, are unsuitable, or the hypnotic property of morphia is exhausted, then late in the evening half a grain of narcein may be ordered, either in powder or solution; and next evening, if the hypnotic action has been too weak, and no irritation of the stomach has been manifested, a grain may be given. In fractional doses, as the eighth to the fourth of a grain, it may be used as a sedative in violent cough. As to its hypodermic use, Dr. Oettinger thinks it is unsuitable, because there is no combination of it which is readily soluble in warm water, and thus easy of introduction into the system by *one* injection; and the solutions at present used cause great pain and irritation. — *Schmidt's Jahrbücher der Gesammten Medicin*, October 24th, 1866.

On the Employment of Subnitrate of Bismuth in the Epidemic Dysentery of Hot Countries. By Dr. Brassac, of the French Navy.— In 1854, M. Monneret made known the therapeutical properties of subnitrate of bismuth; but he had not been able to administer it in the endemic dysentery of hot countries. But when Dr. Brassac met with such cases in the year 1860, he employed the subnitrate in large doses, and with success. He gave it to fourteen patients; but when he first employed the drug, he combined its use with other treatment. In cases which had been attacked with dysentery on repeated occasions, he never employed it until all other means had failed; and he found that the condition of some of the patients was improved. In some, the disease was complicated with engorgement of

the liver; and although the subnitrate often fails in such cases, he nevertheless employed it, as it seems to improve the local lesion, and retards the ulcerative process. The doses of the subnitrate were very large, as Dr. Brassac began with fifteen to twenty grammes (a gramme is about fifteen grains), going on rapidly to sixty and seventy grammes, increasing the dose daily by ten grammes. When convalescence was established, he gave decreasing doses, continuing it, however, for fifteen or twenty days, and even when the patient had only one stool in twenty-four hours. He advises this course to be adopted, in order to secure the success of the treatment, and to protect the mucous membrane which has long been exposed to morbid secretions. The subnitrate was given in unfermented bread, or mixed with a little broth or rice-water, or administered together with the food. The patients subjected to this treatment were principally soldiers in one of the French islands in the West Indies.—*Bulletin Général de Thérapeutique*, October 30th, 1866.

On Milk Diet in the Treatment of Disease of the Heart. By Dr. Pécholier, of Montpellier.—M. Pécholier attributes great importance to the milk diet, not only in dropsy, like some of his predecessors, but also in other diseases, and especially those of the heart. He states that in active hypertrophy—namely, in those cases where the consequences of the development of the muscular fibres prevail over the embarrassment of the circulation caused by the dilatation of the cavities, the contraction of the orifices, or the insufficiency of the valves, and where, in consequence, the tension of the blood is great in the arteries, and the radial pulse is full and hard;—in such cases, at their commencement, the milk diet, together with the use of digitalis, and sometimes without it, will, if continued long enough, induce at once an amendment of the symptoms, and even, at last, an absorption of the superabundant muscular tissue, and thus effect a cure. But in order to secure success, the patients must strictly obey the injunctions given to them; and hence the treatment is more successful in those cases where the lesion has produced great inconvenience and suffering than in those where the patients suffer little, because in the latter case they are unwilling to submit to rules of diet. Under the influence of the milk diet, it is found that the impulse of the heart diminishes, together with the palpitations, and the congested condition of the face, the brain, and the lungs. The patient experiences an unexpected improvement, and by the adoption of this plan life may be prolonged and rendered more supportable; and even where a cure cannot be hoped for, a great palliation of the symptoms may be induced.—*Bulletin Général de Thérapeutique*, October 30th, 1866.

On Respiratory Therapeutics. The Comparison between the Gastric and Bronchial Passages for the Administration of Medicines. By M. Sales-Giron.—M. Sales-Giron has lately developed his theory of what he terms *respiratory diet*, and, in connection with it, its thera-

peutical applications. The respiratory diet is defined by the author as the theory of the relations existing between the living organism and the atmospheric medium, or, in a more restricted sense, the theory of the relations of the lungs with oxygen. From respiratory diet to respiratory therapeutics the transition is easy; and M. Sales-Giron has shown that by the pulverisation of liquids it is possible to cause the absorption, by the pulmonary passages, of medicines not only under the form of gases or vapours—a proceeding which is worthy of little confidence—but under that of aqueous solution, which is more manageable, M. Sales-Giron is said to have cured an obstinate case of intermittent fever by means of a solution of quinine breathed in the pulverised form, and he has thus been led to examine the conditions under which it is advantageous to choose the respiratory passages for the administration of medicines. From the parallel which he draws, in reference to absorption, between the pulmonary and the gastric passages, it follows that these conditions or circumstances must be numerous; and cholera furnishes one of the most striking examples. In fact, under the influence of choleraic poisoning, the digestive tube is inert, and is not only incapable of absorption, but it becomes an organ of *disassimilative* excretion. It is then impossible for medical men to rely upon the action of medicines administered by the digestive canal, and their administration by the pulmonary passages may perhaps be of great service. As long as the period of asphyxia has not arrived, absorption by this channel is possible, and it takes place more certainly and more rapidly than by any other; and, besides, rapidity of action is of primary importance when symptoms are urgent. It would therefore be useful, in the treatment of cholera, to administer the most active medicines by the respiratory passages, reserving accessory ones for the gastric canal. What, again, is more natural than to pursue the poison by the very same path as that by which it entered the system? The choleraic miasm, having the air as its vehicle, enters into and is absorbed by the respiratory passages. The prophylactic treatment ought, therefore, to aim at destroying it in the external air, in the air of dwellings, and in the air entering the lungs in respiration. For a long period medicine was administered solely by the gastric passages, and this was the only plan which occurred to the minds of our ancestors. But subsequently the iatraleptic, the endermic, and the hypodermic methods were introduced, and now the respiratory method is presented to notice. It has, indeed, been known and employed already for the absorption of gases and vapours; but it is now generalised by the pulverisation of liquids, and reasoning is in its favour, and the practical results will determine its value.—*Gazette Médicale*, September 8th, 1866.

On the Indications and Contra-Indications of the Revulsive Treatment. By Dr. Maurice Raynaud.—In a recent thesis by Dr. Raynaud, the author describes the revulsive treatment of diseases, and points out the conditions in which it may be adopted. In advocating the propriety of such treatment, he refers to the fact that when the disease left to itself terminates in a cure, it often happens

that just before convalescence is about to commence, some prominent phenomenon makes its appearance, such as an important evacuation by the skin, the intestine, or the bladder; or a disease affecting one part of the economy suddenly disappears, and seems to be transported to another part; or, again, a new disease suspends or terminates the original affection on which it has supervened. Dr. Raynaud defines revulsion, in the words of Hunter, as the cessation of morbid action in one part, in consequence of the production of action in another part. This mode of treatment will be most useful in local diseases; for whenever a malady consists essentially of an alteration of the blood, there is very little chance that it will be successful; although an exception may be made in the case of some eruptive fevers, in which it is proper to cause from the beginning such a revulsion as may fix on the skin the congestions which are threatening the viscera. When dry cupping-glasses are used in the treatment of typhoid fever, the revulsive treatment is directed to the visceral congestions, and not to the general disease. Among local diseases, it is necessary to distinguish clearly those that are in their nature superficial, movable, and apt to spontaneous change in their place, from those which rapidly take root, and when fixed on one point, tend to perpetuate themselves there. The first class, such as neuralgia, rheumatism, and different congestions, will obey the least impulse carrying them to the surface; while the second, such as the parenchymatous inflammations, accomplish their evolution almost by force when an unsuccessful attempt has been made to cut short their first manifestations. The choice of the revulsive agent is a very important point, and must be made with great care. Thus, dropsies require hydrogogue purgatives, or if the kidney is more amenable to the treatment, diuretics; visceral congestions require an antagonistic discharge on the skin or by the rectum; inflammations passing into the chronic form will require repeated blisters, or, if necessary, cauteries; and hæmorrhage, while it lasts, requires a powerful revulsion to the extremities by means of cupping-glasses or intense rubefacients. The contra-indications to the use of revulsive treatment are chiefly observed in the diseases of infancy. Thus, it is found that in young children all kinds of depletion of blood may induce anæmia, and in cachectic children simple blisters will frequently cause extensive ulcerations, and even gangrene. Again, the choice of revulsives must be guided and controlled by the existence of prevailing diseases. Thus, at certain periods of the year, the slightest wound will give rise to erysipelas, or a blister may induce diphtheria; or in a choleraic epidemic, purgatives given for the relief of a colic may induce a more severe disease than that for which they were administered.—*Gazette Médicale*, August 4th, 1866.

On the Employment of the Hypophosphite of Lime in Pulmonary Phthisis. By Dr. Tirifaley.—Dr. Tirifaley administered the hypophosphite of lime to forty-two persons suffering from tubercle, and obtained some satisfactory results. Eight of the patients in the first stage who were so treated for a period varying from six weeks to

two months became so much improved, that the presence of tubercles could no longer be detected in the lungs. Only one patient had a relapse at the end of some months. Eight other patients in the first stage, treated with the hypophosphite for periods varying from three to ten months, were considerably improved, but were not cured. Three of them experienced a return of the disease after the suspension of the treatment, and two of these latter died. In three tuberculous patients in the first stage, the hypophosphite was employed for a period varying from one to four months; but it produced no effect, either favorable or otherwise, and two of the patients died after the disease had run its ordinary course. In several instances the results were negative. The hypophosphite was given for a period varying from six weeks to a year in six cases where one lung was in the first stage and the other in the third stage, and it was found that the least affected lung was improved or cured; but most frequently—namely, in four cases out of the six—the lung with the cavern was improved, while in two cases out of the six the caverns pursued their destructive course. From these facts Dr. Tirifaley considers that the hypophosphite of lime is useful in the treatment of pulmonary phthisis, and he recommends it in this disease in all its stages, as he feels certain that it will cure some of the cases and will improve the condition of others.—*Gazette Médicale*, October 20th, 1866.

On the Treatment of Hooping-Cough with Hydrophenyl, or Benzine. By Dr. Lochner.—In the year 1864, Dr. Lochner having been convinced that the ordinary remedies had no effect over the paroxysms of hooping-cough, sent his patients affected with that disease to the purifying rooms in the gas-works; but the inconvenience of taking children there in bad weather, and other circumstances, induced him to modify the treatment, which, however, he considered essentially to be a good one. In order to attain his object, he made use of one of the substances contained in the purifying chambers of the gas-works, namely, hydrophenyl, which when unpurified is called benzine. He employed it at first in very small doses; but after he found that it had been given in large quantities in Germany for the destruction of trichinæ, he used it more freely, and he obtained, as he believes, the same results as from a residence in the purifying chamber of the gas-works. In order to assist the internal treatment, he poured a few drops of benzine on the bed of the patients, so that a smell similar to that from the gas-works should be constantly diffused through the room. Dr. Lochner tried the plan on his own child; and he states that the precursory symptoms of the hooping-cough lasted a week, and the disease itself only six days; and although the paroxysms were very violent, they did not amount in number to more than five or six in the twenty-four hours, and were shorter in duration than in other children. He gave this child from ten to fifteen drops of the benzine every day; and as soon as it was asleep, he sprinkled a few drops on the bed. He thinks that the best mode of administration is in a spoonful of water.—*Gazette Médicale*, October 20th, 1866.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,

Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

Transmission of Tuberculosis by Inoculation. By Dr. Villemin and M. Lebert (de Breslau).—M. Villemin has presented to the Academy of Medicine a series of memoirs containing the details of experiments, whereby he thinks it proved that pulmonary phthisis, as well as other tuberculous diseases, belong to the class of specific affections—that tuberculosis is capable of being transmitted by inoculation, and that it is nosologically related to syphilis, or perhaps more closely to farcy or glanders. His first series of experiments were made on rabbits. He inserted, by means of subcutaneous incision, tubercular matter and puriform liquid taken from the lung and intestine of a phthisical patient, who had died thirty-three hours previously, in the ear of a healthy sucking rabbit three weeks old. The operation was performed three times. When the animal was three months and a fortnight old, being apparently in good health and having grown a good deal, it was killed. Miliary tubercle was found in the stomach and in the small intestine. Tubercle was found in the kidneys, and the lungs were full of large masses of tubercle. Another rabbit, of the same brood, which had lived under precisely the same circumstances, was killed, and found to be entirely free from tubercle. Similar experiments, with the same result, were performed on rabbits living in the open air, in an enclosure in which there was a covered retreat, and where they enjoyed ample and varied nourishment. The rabbits which were inoculated exhibited, when killed after about three months, abundant tubercle in the lungs and other organs whilst other rabbits which lived under the same conditions were, when killed, completely free from tuberculization. These experiments were repeated, and always with the same results. Another was made by inoculating a rabbit with various morbid products, such as choleraic discharges, pus from a phlegmonous abscess, pus from anthrax. This rabbit, when killed after some weeks, exhibited no pathological peculiarity. Not knowing at what stage of its development tubercle was best adapted for inoculation, M. Villemin always collected it from two granulations—the one grey, the other at the commencement of softening. In order to avoid inflammatory products, he selected it as far as possible elsewhere than in the lungs. The subjects from whom the matter was taken had been dead from twenty-four to thirty-six hours. The local phenomena resulting from the introduction of a small fragment of tubercular matter broken up into a slight wound on the rabbit's ear were the following:—"During the first two or three days nothing appeared; the inoculation puncture was closed by a slight

incrustation; but generally about the third day a little redness, heat, and swelling were perceptible. At the end of three or four days this redness and swelling disappeared; but there remained beneath the puncture a slight nodosity movable with the superjacent skin. This nodosity was sometimes found without apparent previous redness or swelling; it then increased, and, after a longer or a shorter time, an opening appeared upon the skin, by which a very thick, white purulent matter escaped. The small aperture then closed, and the nodosity disappeared—only, however, to reappear after a little while, again to empty itself, and so on." M. Villemin is of opinion that, beyond the human race, which has unfortunately a special aptitude for phthisis, there is but the ape, the cow, and the rabbit, and perhaps a few analogous rodents, which can truly be recognised as susceptible of tuberculosis. With regard to the quadrumana which are so frequently carried off by phthisis in the menageries of Europe, M. Villemin doubts whether the mortality be due to physical causes resulting from change of climate: he thinks it is rather owing to contamination transmitted through an atmosphere vitiated by the human crowd. The last memoir contains accounts of experiments under the following heads:—1, Inoculation of the rabbit from man; 2, inoculation of the rabbit from the cow; 3, inoculation of rabbit from rabbit; 4, inoculation of the guinea-pig from man; 5, inoculation of the dog from man; 6, inoculation of the cat from man; 7, inoculation of sheep from man; 8, inoculation of birds from man; 9, injection of tubercular matter into the trachea; 10, inoculation of the matter taken from the tubercle developed at the site of an inoculation; 11, period of incubation of phthisis; 12, influence of tuberculosis upon parturition and offspring. The following points, which M. Villemin thinks he has made out, are of interest, if they meet with future confirmation:—Calcareous phthisis of the cow is identical with tuberculosis in man; the rabbits inoculated from the cow were affected much more rapidly, and generally, with tuberculosis, than those inoculated with human tubercle. To meet the objection that the results were due to the introduction of cadaveric matter, M. Villemin inoculated a rabbit with tubercle taken from another rabbit at the moment of its death—in fact, whilst the heart was still beating. The inoculation of rabbit from rabbit is much more rapid in its effects than the inoculation of rabbit from man. The guinea-pig is as susceptible of the tubercular virus as the rabbit. From his experiments on dogs and cats, M. Villemin concludes that tuberculosis seems to have the power of attacking the carnivora; but it is less easily induced in them than in rodents, and the dog and cat may be considered to be relatively antagonistic to the disease. The sheep he believes to be antagonistic to tuberculosis. The birds experimented on also presented no morbid appearance several months after inoculation. The initial tubercle developed at the point of inoculation becomes, he believes, the source of the generalisation of the disease. Tubercle shows itself in internal organs at a certain distance of time after inoculation: according to experiments, pulmonary tubercle appears from the tenth to the

twentieth day. He thinks that multiplied cases of phthisis in a family may often be traced to cohabitation as much as to inheritance. Abortion or premature death of the offspring was the common result of tuberculosis in rabbits and guinea-pigs. Young rabbits the offspring of phthisical parents presented no trace of tuberculization.

M. Lebert's experiments on rabbits and guinea-pigs are confirmatory of M. Villemin's. From his researches, he infers that there is a special virus for tubercle, as there is for smallpox, syphilis, and glanders.—*Gazette Hebdomadaire de Médecine et de Chirurgie*, Dec. 15th, 1865—Oct. 19th and 26th, Nov. 9th and 30th, and Dec., 1866; *Archives Générales de Médecine;* and *Edinburgh Medical Journal*, Feb., 1867.

Temperature of the Body in Cancerous and Tuberculous Affections. By Dr. Da Costa.—After relating a case of cancer in the stomach which in clinical history presented some points of similarity to cirrhosis of the liver, Dr. Da Costa notices that, notwithstanding progressive and rapid wasting, the temperature of the body was always below 100° Fahr. He had noticed the temperature in three other cases of internal cancer. In a case of cancer of the liver, the evening heat was never more than a fraction above 99°; in a case of cancer of the mesenteric glands, the temperature did not exceed 98°; in another case of internal cancer it did not reach 100°. Dr. Da Costa suggests that temperature may prove a guide in cases of doubtful differential diagnosis between cancer and tubercle of internal organs. As far as his observations extend, they show that the temperature in cancerous affections is comparatively low; whereas in tubercular disease the thermometer indicates a heat much higher—a fever temperature.—*Proceedings of Pathological Society of Philadelphia; American Journal of Medical Sciences*, Jan., 1867.

On Alterations in the Muscles in Fevers, and particularly in Smallpox. By M. G. Hayem.—The author inquires if the lesions of muscles described by Zenker as occurring in typhoid fever are degenerations peculiar and proper to that fever, as the above observer thinks, or rather if they do not represent in the muscles alterations analogous to those, commonly met with in the liver, the spleen, the kidneys, &c., of subjects who have died, not of typhoid fever only, but of other general febrile maladies. His researches have been principally, but not solely, conducted on the bodies of patients who have died of smallpox. He has found the same alterations of the muscles in fatal cases of variola, typhoid fever, scarlatina, measles, erysipelas, tubercular meningitis, generalised acute tuberculosis, and puerperal fever. On the other hand, in a great number of diseases, as phlegmasiæ, tuberculous and cancerous cachexiæ, Bright's disease, &c., the examination of the muscles only gave negative results. In persons dying of cholera the author has always found perfect conservation of the muscular fibres, and this also although the disease has not terminated fatally until after a long typhoid stage; but when

cholera has supervened on typhoid fever, variola, or measles, the alterations of muscular tissue which are found in these diseases may be observed. Of the alterations spoken of, the author recognises three degrees. 1. In the first stage the muscles generally are somewhat swollen and hard, their colour brownish or deep red, their friability a little increased. These conditions are due to general hyperæmia. At irregular points, generally in the form of elongated bands, decoloration and fragility of muscular fasciculi are observed. These characters acquire their maximum of intensity and extent in the next stage. Under the microscope, capillary congestion is observed, and swelling of the muscular fibre, which appears wavy and irregular; striation remains. Soon, greyish granules appear, scattered here and there irregularly, but always between the fibrillæ of the primitive fibres. These for the most part dissolve in acetic acid; but, as their number augments, the muscular fibres acquire a granular aspect, and a certain number of granulations resist the action of acetic acid: the muscular nuclei become also more numerous by division. At the points where the muscular fasciculi are paler and particularly fragile, the author has observed that form of alteration called "waxy" by Zenker.—2. In this stage the colour may be variable, depending upon sanguineous exudation, or upon change in the muscle itself. The normal tint of the muscle becomes paler, but not equally so in all the fasciculi: some become reddish grey, others yellowish or whitish; at the same time, their smooth aspect and special brilliancy are modified. Their consistence is altered; the muscular mass is harder, but its fragility is increased, and the torn surface is markedly granular. Microscopical examination shows an exaggeration of the appearances observed in the former stage. The appearance of fatty granules and beads, and the breaking up of the altered fibres, indicate the commencement of disorganisation; meanwhile the multiplication of nuclei continues both in the interior of and outside the altered fibres.—3. The third degree is that of disintegration or softening of particular portions or centres. The fascicular aspect is excessively modified or entirely lost; in fact, the consistence may be so altered that at certain points the muscular tissue is diffluent. The muscular fibres tear as if boiled, and in some cases the cut surfaces exhibit a kind of puriform detritus tinged with blood more or less altered. Microscopic examination shows, in these cases, advanced disintegration of the muscular fibres, which are always more or less broken up under the form of waxy, granulo-waxy, or simply granular *débris*. These muscular *débris* exhibit abundant nuclei. Fatty granules and free globules, finely amorphous granular matter, and elements of altered blood are also found.—*Societé de Biologie—Gazette Médicale de Paris*, Nov. 3rd and 10th, 1866.

On Disorder of Motor-power after Diphtheria.—Dr. Brenner, of St. Petersburg, describes three forms of this affection:—1, true ataxy; 2, paralytic ataxy; and, 3, genuine paralysis. In the first form a voluntary muscle finds, when in action, neither the normal

counterpoise of its lateral antagonistic muscle nor the support of its normal co-operation. This condition may depend upon an affection of the co-ordination centre, or on disorder of the conductors of co-ordination impulse. Certainly, muscles appear in consequence to act with undue power.—2. Paralytic ataxy consists in partial paralysis with normal condition of other muscles, or in unequal paralysis of all the muscles of a limb. The less or not at all paralysed muscles overpower those more or wholly paralysed; but if the paralytic affection progresses, this preponderence of power will be lost.—3. In the genuine paralysis there are no symptons of ataxy; the paralysis may present diversities of degree.

Dr. Brenner gives an example of each of these forms: the treatment consisted essentially in Faradization and galvanization; the cases lasted 5, 5, and 18 months. He considers that some cases may be cured by tonics, but that many of these can only be cured by the electrical treatment, and that even paralysis of the soft palate is more quickly cured in this way than by any other means; and, therefore, that it is advisable to treat all diphtheritic motor disorders at an early stage by Faradization and galvanization.—*Petersb. Med. Ztschr.*, x, 4 u. 5, p. 259, 1866); *Schmidt's Jahrb.*, B. 133, p. 34, 1867.

The Pathology of Cholera Collapse. By Dr. Horace Jeaffreson.—From the post-morten examination of patients who died in the cholera wards of the London Fever Hospital, and from a comparison of the weights of the viscera of persons dying in the collapse and non-collapse stages of the disease with the normal standard, Dr. Jeaffreson believes he has arrived at the most probable cause of collapse. He admits the action of a special cause—cholera poison—and agrees with Dr. G. Johnson in rejecting the theory that collapse arises from the profuse alvine discharges; but he also rejects Dr. Johnson's theory that the circulation of the cholera poison produces contraction of the pulmonary artery and its branches. He thinks that his observations point unmistakeably to a definite and constant lesion present in death during collapse—that this condition precedes collapse, while it is less defined or even absent in those who die in a subsequent phase of the disease. "This cause is intense congestion and inflammation of the whole length of the small intestines, causing them to be fleshy, pink, and heavy, with sticky glutinous matter on the injected, finely granular peritoneal surface in some instances, and with general injection and œdema of the mucous membrane and patches of ecchymosis in *all* of those cases which died in the collapse stage of the disease. This condition is associated in the same class of cases with a more or less anæmic state of the lungs, kidneys, and spleen, while the liver maintains about its normal appearance—a fact probably due to its main supply of blood from the but little contractile vena portæ. * * * The chain of causation appears to be the following:—A poison in the alimentary canal acts there as a direct irritant, causing more or less rapidly-developed congestion and inflammation of the whole small intestine, to which much blood is

determined. The intestine, meaning by the term the tissue of the various coats, becomes full and turgid, and actually œdematous, whereby a strong rapidly developed impression, resulting in shock, is made upon the innumerable and widely spread branches of the sympathetic from the solar plexus, by which the duodenum, jejunum and ileum are supplied. The well-known intimate connection of the solar plexus with the splanchnic and pneumogastric nerves, and also with the posterior roots of the corresponding spinal nerves, insures the diffuse spread of this impression, amounting to a shock, from which results a general slow contraction of the organic muscular fibres of the whole arterial system, affecting not only the pulmonary artery, but the systemic arteries, including those of the kidneys and spleen, which are found anæmic after death, and also including in all probability the hepatic artery, though from the peculiar nature of the circulation in the liver the effects there are less manifest."—(*Edinburgh Medical Journal,* Dec., 1866).

Cases of Aphasia. By the late Dr. R. E. Scoresby-Jackson, F.R.S.E.—The death of Dr. Scoresby-Jackson is a great loss to clinical medicine. How carefully he observed and how lucidly he recorded his observations is exemplified in the records of two interesting cases of right hemiplegia with loss of intellectual language which were published just before his death. The first case was that of a well-educated gentleman, æt. 21, who was seized with right hemiplegia and amnesic aphasia (loss of intellectual language both articulate and written) during convalescence from typhus fever. The occurrence of hemiplegia and loss of intellectual language in connection with typhus, Dr. Jackson believed to be unique. Two near relatives of the patient, however, were affected late in life with brain disease, and another in early life. This latter was the result of an accident. At the end of ten months after the attack, Dr. Jackson's patient had recovered almost entirely from the hemiplegia; but he had not regained entirely his power of speech, and he still made blunders in writing.

The second case was that of a man, æt. 47, who two months previously to his illness had fallen from a cart, and received two slight wounds, one over the right olecranon, the other on the left temple—the latter extended upwards from the outer angle of the orbit. Ever after this he complained of pain in the head. He suddenly lost power on the right side. He was brought to the Royal Infirmary five weeks after the seizure, on September 20th, 1866.

State on admission.—Cardiac action weak, irregular, and tumultuous; sounds altered in character, but no murmur; pulse 75, weak, irregular, compressible; respiratory murmur feeble generally. Loss of motion on right side both of upper and lower extremities; right side of face slightly paralysed; apparently there is impairment of sensibility on the affected side. When asked to put out his tongue, he opens his mouth, but cannot protrude it; when asked to nod his head, he rolls it from side to side: the only articulate sounds he can produce are, "No," "Yes," "Oh dear," and "Sair." When the

patient was admitted, he had several large bedsores. He died on October 22nd.

Autopsy.—Body moderately well nourished; pericardium natural; heart of natural size—its muscular substance pale, the valves thickened at the margins, and on the mitral valve numerous vegetations and loose fibrinous masses; lungs adherent and congested throughout, the right in a state of solid œdema; liver congested, many of its cells fatty; spleen pulpy and enlarged. In the left kidney several conical masses in different stages, some deeply congested and solidified, others fawn-coloured with a red margin; a third corresponding to a depression on the surface was more white and fibrous. In the right were a few minute red patches, which appeared to have resulted from minute embolisms. Stomach and intestines natural; skull-cap natural—dura mater firmly adherent to it; arachnoid at some points thickened—a little milky fluid in its cavity. The vessels of the base were atheromatous at several points; but there was no embolism, excepting in some of the branches of the middle cerebral artery of the left side. There were at several points of division decolorised firm clots, and from them in the line of the current extended dark solid coagula. Outer surface of left hemisphere was flattened, and fluctuated on pressure "over an area extending from between two or three inches in front of the fissure of Rolando to the posterior boundary of the parietal lobe, and from within an inch of the great longitudinal fissure to the inferior margin and round to the base. At the base it extended to the tip of the temporo-sphenoidal lobe, but very slightly affected the inferior temporo-sphenoidal convolution. On opening up the left Sylvian fissure, the opposed surfaces of the hemisphere were found shortened and of a yellowish colour. The insula was so softened, that the sulci were obliterated, and the convolutions could not be defined. This condition and discoloration extended from the insula to the inferior frontal convolution, and embraced the whole of its posterior extremity, extending backward through the ascending frontal convolution, and as far back as the supra-marginal and angular convolutions. The corresponding parts of the right side were natural. Over the diseased surface the membranes were so connected with the substance of the brain, that they could not be removed without tearing it." "On cutting into the brain, the extensive fluctuating and collapsed portion of the left hemisphere was found softened, the degree of softening varying in different parts; in some a creamy-looking fluid, in others of a consistence like that of switched cream, and in others presenting the characters of fawn-coloured softening. It was most advanced in the anterior part, just above the insula and the inferior frontal gyrus. The posterior part was of the consistence of pulp. The softening extended from the insula and the inferior frontal convolution as far inwards as the corpus striatum, the whole of the extra-ventricular portion of which was involved. The anterior end of the intra-ventricular portion also presented some softening. The superior and middle frontal gyri, the upper ends of the ascending frontal and parietal gyri, the posterior parietal lobule, and the anterior end of

the inferior frontal gyrus, were not softened. On microscopic examination, the softened parts were found to contain innumerable compound granular bodies, and numerous flakes of red and reddish brown pigment. The right side of the brain was healthy."—*Edinburgh Medical Journal*, January and February, 1867.

Aphasia with Right Hemiplegia, Cerebral Ramollissement: supposed Tertiary Syphilis. By MM. Bouchard and Lepine.—The patient was a woman, æt. 40; she was admitted on June 20th, 1866, at the Hôpital de Lourcine, under M. Jules Simon. Three weeks before, she had been attacked with aphasia and hemiplegia of the right side. For six months she had suffered from violent headache. There was a tumour on the head and on the right clavicle. She could only say "Oui" and "Non," and these words were not always properly employed. Hemiplegia was almost complete. The aphasia had improved considerably at the end of some weeks; she could say almost any words, but did not always apply them correctly. The hemiplegia made no progress, and she died on September 24th.

Autopsy.—The anterior and external part of the left frontal lobe (second and third frontal convolutions) was reduced to a yellowish *bouillie*. There was a gummatous tumour of the size of a large pea on the meninges at the level of the parietal convolutions. The tissue of the pitnitary gland was somewhat whitened and hardened. The liver was adherent to the diaphragm; its substance was congested, and near the posterior border there was a portion which exhibited diffuse inflammatory action, and gummata of the size of a filbert in caseous degeneration. Gummata were also found in the Fallopian tubes. The patient was married, and there was no history of syphilis. —*Société de Biologie—Gaz. Méd. de Paris*, Nov. 10, 1866.

The Cause of Aphasia. By M. E. Gintrac.—In the course of a very valuable paper on Cerebral and Spinal Hæmorrhages, of which we should have been glad, had space permitted, to have given a full analysis, M. Gintrac thus sums up his observations on the pathological cause of aphasia:—What do we learn from anatomical examination in regard to the loss of speech? a symptom so often observed in encephalic hæmorrhages. It may be connected with lesion of the anterior lobes of the brain, and more particularly of the third frontal convolution. But to seven or eight examples cited may be opposed 34 thus distributed:—3 belong to meningeal hæmorrhages, 7 to the middle lobe of the brain, 2 to the posterior lobe, 9 to the corpus striatum, 1 to the thalamus opticus, 1 to the corpus striatum and thalamus opticus, 7 to the mesocephalon, 4 to apoplexies of multiple seats. Therefore, aphemia or aphasia has not a unique source; but I have remarked a sufficiently curious circumstance which accords with that which others have noted, viz., that in the majority of cases the seat of the hæmorrhage appertains to the left hemisphere.—*Journal de Médecine de Bordeaux*, and *Gazette Méd. de Paris*, Dec. 15, 1866.

Hæmatinoptysis in a Case of double Pleurisy and Hypogastric Peritonitis, probably of a Traumatic Origin. By Professor Lebert, of Breslau.—The case was that of a man of intemperate habits, who had received some severe blows on his sides and on the epigastrium. Double pleurisy and peritonitis followed. At the end of the fourth week of the attack, abundant expectoration set in: this, under the microscope, was found to contain elements of pus and abundant crystals of hæmatoidine. This kind of expectoration continued, with interruptions, for eight weeks. The patient ultimately recovered. The colour of the sputa differed from that of all other kinds of sputa containing blood, and presented in appearance muco-purulent elements side by side with those of the colour of rust, ochre, &c. Under the microscope, the expectorated matter was found to contain no blood-globules, but pus-cells with a large quantity of crystals of the colouring matter of the blood, some of a fine red colour and of the form of rhomboidal prisms—some needle-shaped, single or united in bundles. In addition, in all the specimens examined there were granules or very small amorphous corpuscles of a blue colouring matter, such as is not uncommon in hæmorrhagic residue.—*Communicated to the Society of Biology; Gazette Méd. de Paris*, Oct. 27.

Fatty Embolism of the Capillaries of the Lung. By E. Wagner.—E. Wagner has verified the presence of fatty embolia in the capillaries of the lung. This fat is contained in the capillaries in a liquid state, in the condition of globules or larger masses, often marking out the anastomotic network of the capillaries, which are frequently much distended. Round the points which are the seat of fatty embolia are frequently found hyperæmia of the pulmonary tissue, inflammations, hæmorrhages, and metastatic abscess; but these lesions are not constant. Observations show that in these cases of fatty embolism there is always found in the organism, at a distance from the lung, liquid fat, under a form which allows its introduction into the vascular system—such as an abscess the pus of which has undergone fatty metamorphosis, or an abscess forming in situations where normal fat is set at liberty by suppuration. In some cases, however, there were no peripheric abscesses, but fractures in which the vessels of the bone had been torn and permitted the penetration of the fat of the marrow into the veins and to the right heart.—*Archiv der Heilkunde; Gazette Méd. de Paris*, Feb. 23, 1867.

Pneumothorax, with rapid Recovery. By Dr. A. Vogel.—The patient was a woman, æt. 29, of robust constitution, and excellent health since an attack of enteric fever when she was ten years old. During the last five years she had been twice confined, and had afterwards, on each occasion, acted as wet-nurse for a long time, and enjoyed excellent health, excepting that during the last months of nursing she had some bronchial catarrh, with teasing cough and a little slimy expectoration. In the summer of 1865, after weaning, she was quite well, with the exception of a dry cough. In August she complained only of darting pains (*stiche*) at times in the region

of the liver. On the morning of August 6th, while she was moving a heavy bath, she was suddenly seized with severe pain in the right side, and extreme dyspnœa. She took to her bed, lying on her left side, and believed herself to be dying. In the evening, when Dr. Vogel saw her, the features were most significantly altered; the pallor and cyanosis striking; the anxiety, restlessness, and dread of death extreme. The pulse was 106, very small; respirations 52. Examination showed a pneumothorax of the entire right side, with displacement of the liver downwards, of the heart to the left, and so on. Behind, on the right side, over about a hand's breadth, there was dulness with bronchial breathing. Some ease was at first obtained through large doses of opium. On August 7th the patient woke with suffocation and sickness, and complained again of anxiety and severe pain. The physical signs remained the same, and especially there was no symptom of any pleuritic exudation. After the exhibition of morphia the anxiety lessened somewhat, and from that time a steady diminution could be noted in the space occupied by air. By September 10th the patient was quite well: the sound over the right side of the chest was normal, and the respiratory murmur vesicular; the countenance was fresh and blooming. A year later she remained well, and not the slightest lung affection was discoverable.—*Deutsch. Archiv für Klin. Med.*, ii, p. 244, 1866; *Schmidt's Jahrb.*, B. 132, p. 299, 1866.

Researches on Insufficiency of the Tricuspid Valve and Venous Pulse. By A. Geigel.—The following are the author's conclusions:—

1. Systolic regurgitation of the blood into the inferior vena cava may often be recognised by pulsations, perceived both by sight and touch, in the middle right region of the abdomen. These pulsations, very evident after paracentesis, may be also perceived, although less distinctly, in the case of ascites, the pulsations being transmitted to the right lobe of the liver. In slight insufficiency this sign may fail.

2. The sphygmographic curve of the venous pulse is characteristic, and very different from the arterial curves. Dicrotism does not fall in diastole as in arterial curves, but it is systolic.

3. Lastly, he has verified, in several cases, that strong pressure by the fingers on the inferior vena cava, beneath the right hypochondria, augments the pulsations in the veins of the neck. This sign has considerable importance in cases where the insufficiency of the valve is only relative.—*Wurzberger Medicinische Zeitschrift* and *Gaz. Méd. de Paris*, Feb. 16, 1867.

Syphilitic Tumours of the Liver in the Fœtus. By M. J. L. Prévost.—In a still-born fœtus, aged about seven months, born on July 19th, 1866, whose mother said she had contracted syphilis in the preceding January, and had mucous tubercles on the labia majora at the time of her confinement, the following post-mortem appearances were found:—The exterior gave evidence of maceration, the epidermis separated easily, and a sanguinolent serosity had accu-

mulated under the teguments. The inferior surface of the diaphragm adhered to the upper surface of the liver over a considerable portion of the right lobe. This adhesion corresponded to two indurations of the hepatic tissue, of which the larger, of the size of a walnut, was situated two or three millimètres from the free border of the organ, and the smaller (of the size of a filbert) was situated a little farther back. The surrounding hepatic tissue was greyish yellow, with small paler spots scattered through it. The limits of the tumours were wavy and ill defined. Their tissue was hard, resisting the scalpel, differing from the tissue of the rest of the organ, which was of normal consistence and colour. Microscopic examination showed that the tumours were composed of laminated fibres, of fusiform bodies, some normal, others misshapen and globular, and of a large quantity of free nuclei and fatty granules. The remainder of the liver and the other viscera were healthy. The structure of these tumours, therefore, closely corresponded with that of syphilitic gummata, and the history of the mother confirmed the diagnosis.—*Report of the Société de Biologie—Gazette Médicale de Paris*, Oct. 6.

Constriction of Ileum by Appendix Vermiformis. By Dr. T. H. Andrews.—The patient, an Irish labourer, æt. 55, was admitted into the Pennsylvania Hospital, apparently suffering from strangulation of an old inguinal hernia. He was operated on. The sac was opened, but it contained no intestine. The man sank, two days after the operation, with all the symptoms of intestinal obstruction. After death, the peritoneum was found minutely injected, very dry and sticky from exudation. There was no appearance of any part of the bowel having been strangulated in a hernial sac. The adhesions of intestine to iliac fossa and elsewhere were thick and tough; and at one point on left side of pelvis, above the obturator foramen, a loop of ileum was adherent. The small intestines, to within four inches of the ileo-cæcal valve, were distended with fæces; below that point the ileum and the large intestine were greatly contracted. The cause of the obstruction was adhesion between the end of the vermiform appendix, which was twisted back and to the left, and a loop of the ileum. This adhesion, one third of an inch thick and half an inch long, bridged the ileum four inches above the valve, so as to compress it against the spinal column, and completely to occlude it. There was thickening of the mesentery and hardening of the mesenteric glands.—*Proceedings of the Pathological Society of Philadelphia—American Journal of Medical Sciences*, Jan., 1867.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

Injuries of the Head.—Professor Roser is desirous of calling the attention of practitioners to a few points which he believes possess some novelty:

1. *A traumatic brain-sound.*—This is heard in open wounds of the skull accompanied by an aperture in the dura mater, without injury to the pia mater; and it arises from the entrance of the air during the subsidence of the brain, and its exit on the heaving-up of the organ. Professor Roser has observed this sound twice at his Marburg Clinic. In one of the cases, a small rent in the exposed dura mater was covered with blood; and whenever this was displaced by coughing, a clacking sound became plainly audible by all present. In the other case, the patient stated on his entrance that there was a whistling noise in his head; and a hissing noise was heard by all the assistants.

2. *The pulsations of the brain when the dura mater is laid bare are sometimes absent.*—Judging from two such cases in which an autopsy took place, this would seem to be a sign of abscess behind the dura mater. In fact, the presence of a circumscribed exudation or extravasation may, by compression of the corresponding portion of the brain, render this anæmic and incapable of expansion during the systole. It might be supposed that this absence of pulsation is due to the feebleness of the heart's action during the agony of death; but in one of the author's cases the patient retained all his strength.

3. *The discharge of fluid from the cavity of the arachnoid is significant of superficial injury to the brain.*—When this is more considerable, a protrusion of cerebral substance, impeding the flow of the fluid, is more likely to take place. In none of his cases of considerable injury has Professor Roser observed this flow of fluid, while he has seen it continue for several days in some cases in which the injury has been obviously superficial.

4. In some cases in which the brain has been laid bare and the injury is only superficial, a kind of *fistula* may be formed analogous in its mechanism to fistula of the cornea. Just as in this there may be observed an interrupted flow of the aqueous humour, so in a case of exposed brain the author saw a similar discharge during the third and fourth week after the injury. While the wound was granulating most kindly, a bladdery projection appeared in its midst, and, bursting from time to time, spurted out clear water. This ceased as the wound cicatrized, leaving only a slightly prominent cicatrix.—*Archiv der Heilkünde*, 1866, Heft 6.

Case of Spontaneous Fracture.—Dr. Caspary relates the following interesting case, occurring in the person of a short, strong, healthy

man, twenty-six years of age:—In June, 1866, while ascending two steps which led to his dwelling, he felt a peculiar sensation in the left leg, as if unable to bear the weight of the body. He got indoors, however, and was even able to sit at table; but when he arose and attempted to walk, he cried out that he heard his leg crack, and that it was coming asunder. He could no longer stand, and was conveyed to bed. On examination, there was found to be a transverse fracture of the tibia at the junction of its upper and middle thirds, with but little mobility, and no crepitation or displacement. The patient exhibited no sign of rickets, nor did the bone itself present any abnormal character. A gypsum bandage was applied, and the limb laid on a firm mattress. The patient lay very quietly and without suffering; but when the bandage was removed, after three weeks, the limb remained precisely in the same condition. Under the advice of Dr. Wagner, assistant at Langenbeck's Clinic, the iodide of potassium was administered, that surgeon having found it of great utility in several cases of fracture of difficult consolidation. In this case it proved of no utility, as at the end of another four weeks union had not taken place. A very thick gypsum bandage, which reached up above the knee, was now applied, and the patient was directed to walk about, which he was soon able to do tolerably well with a stick. In twelve weeks, and five months after the occurrence of the fracture, bony union had taken place. The most careful investigation of this case failed to show any general or local pathological condition capable of explaining the occurrence of the spontaneous fracture.—*Berlin Klin. Wochenschrift*, 1867, No. 4.

On the occurrence of Subcutaaeous Emphysema during the performance of Tracheotomy.—Professor v. Dusch observes that although this form of emphysema has been described, the mechanism of its production, as far as he is aware, has not been explained. It is not a consequence of accidental injury to or of opening the trachea, occurring as it does before such opening has been executed, and which, indeed, becomes the means of preventing its farther extension. Although the complication may not in itself be dangerous, yet it very much enhances the difficulty of opening the trachea and introducing the canula, while the frightful appearance of the swollen face and neck causes great alarm to those who are not prepared for it. In the cases which occurred to the author, he felt convinced that the accident had not arisen from any injury to the trachea, and that the source of the air was the anterior mediastinum. He was led to this explanation by a case in which, owing to the breadth of the thyroid, he was compelled to operate very low down, and found the blood in the wound traversed by bullæ of air at each expiration. At the autopsy, considerable emphysema of the cellular tissue of the anterior mediastinum was found. Air may gain admission there if the *ligamentum interclaviculare* be injured, owing to the connection of this with the deep layer of the superficial fascia. This renders the penetration of air possible; but for its occurrence there must be an aspiration into the mediastinum during inspiration, and in order

that emphysema may be produced, an obstacle to expiration must exist. These conditions prevail in a high degree in those obstructions of the air-passages which call for tracheotomy.

In such cases the most violent inspiratory movements occur at the upper and anterior part of the thorax; the upper ribs and *manubrium sterni* being raised, while the lower part of the thorax is narrowed, having the end of the sternum drawn inwards towards the spine. The anterior mediastinum undergoes expansion, and entrance of air through the aperture ensues, this being much favoured by a considerable descent of the trachea downwards and backwards. During expiration the upper part of the thorax falls backwards, the trachea rising and approaching the anterior wall of the chest, closing, together with the coagula, the aperture against the exit of the aspired air. This escapes into the surrounding connective tissue, easily making its way to the neck and face. Immediately that the canula has been introduced into the trachea, the derangement of the respiratory process ceases, and air no longer presses into the mediastinum. The relief of the emphysema is, therefore, to be sought for in the prompt completion of the operation.

Although the production of this emphysema may in some cases be attended with no ill consequences, yet it may sometimes give rise to the production of inflammation and suppuration in the mediastinum, especially if there be aspiration of blood with that of the air; and Lissard rightly suspects that the substernal abscesses met with after tracheotomy at the Hôpital Eugénie are connected with this emphysema. At all events, it renders the most important step of the operation much more difficult, and, if this be not shortly completed, may add to its danger; while the frighful appearance the child assumes may strongly prejudice the friends against the procedure. The possibility of the occurrence of emphysema, then, should engage us to perform tracheotomy very low down as seldom as possible, or avoid doing so altogether.—*Archiv der Vereins für wiss. Heilk.*, 1866, No. 1.

On Opening Joints when the Seat of Traumatic Suppuration.—Professor Roser observes that it is remarkable that difference of opinion should still prevail respecting J. L. Petit's practice of freely opening the joints when the seat of traumatic suppuration. It has not been for the want of opportunity, seeing the numerous bloody wars which have been waged since the time he wrote. But practitioners have approached the subject with preconceived notions which have prevented them profiting by experience, while the complications attendant upon the occurrence have surrounded it with difficulties. Under the influence of these Petit's practice has often been supposed, erroneously, to lead to unsatisfactory results. Thus, 1. Hæmorrhage following the incision, its arrest by stuffing the wound with *charpie* would only aggravate the condition of the abscess. 2. In dread of such hæmorrhage, the incisions were made too small, the wound either closing again, or valvular apertures preventing complete evacuation following. 3. Pyæmia frequently fol-

lowed the incisions. At no time were *débridements* more eagerly practised than at the epoch of Dupuytren and Rust; and nowhere was pyæmia more rife than at the Paris Hôtel-Dieu and the Berlin Charité, so that the incisions came to be regarded by some as rather provocative than preventive of this complication. 4. Owing to the faulty mode of adjustment, a distorted or contracted limb was a common result. 5. Even when the operation was primarily successful, the healing of the parts was often delayed, and separation of cartilage and usure of the bones subjected to pressure were mistaken for caries.

At the present day these grounds of objection no longer exist. Hæmorrhage is avoided by the methodical exposure and incision of deep-seated parts, under chloroform if required. The production of impediments to the free exit of the discharges by the valvular character of the apertures being recognised, can be easily guarded against. Pyæmia is no longer sought to be prevented, as in Dupuytren's time, by antiphlogistics, but by the dispersion or isolation of the patients, disinfection, ventilation, &c. The improved apparatus now in use prevents the contraction of the limb; and with a more careful diagnosis, there is no longer fear of confounding usure of the epiphyses with caries, a more speedy healing of the parts being also aided by the employment of the gypsum bandage. The essential point in this mode of treatment is not the *débridement* once so considered, but the complete evacuation of the decomposed exudations. This can only be accomplished by free openings, small incisions either closing up again or giving rise to valvular apertures. In all probability, such incisions will hereafter be resorted to more frequently for all the joints, and will be found a more conservative practice with respect to gunshot injuries of the hip and shoulder than the primary excisions now practised with such indifferent success. At all events, such free incisions should be resorted to when primary excisions have not been indicated or have been neglected, and when considerable collections of matter have taken place.—*Archiv der Heilkünde*, 1866, Heft 6.

On Shampooing in Sprain.—M. Berénger-Féraud observes that it is remarkable that while some therapeutical agents are at once and even prematurely adopted, others, the efficacy of which has been well attested, remain for long period neglected. As examples of this, he instances the alcoholic dressings of wounds, the forced taxis in hernia, and shampooing (*massage*) in sprain. Confining his attention to this last, he cannot but feel surprised that it has not become generally adopted, after the testimony of MM. Nélaton and Demarquay, and of so many recorded cases, in its favour. He does not think that the uncertainty of the results, the fact that it is a practice followed by bone-setters, or our ignorance of the *modus faciendi*, ought still to be urged as reasons for its neglect. He furnishes a long array of references to cases in which the practice has proved thoroughly successful; and he argues that we should, by its scientific adoption, take a means out of the hands of charlatans

which has very often conduced to their reputation. As to the manner of procedure, that may be easily acquired. As soon as the diagnosis has been made, very gentle passes are to be executed with the pulp of the thumb or the four fingers in the direction of the periarticular tendons if a joint is the seat of sprain or in that of the fleshy fibres when this is muscular, employing from time to time some fatty body so as to prevent the skin becoming irritated. The friction must be directed from the extremity to towards the root of the limb, commencing some distance below the seat of pain and extending considerably above this. Gradually, as contact can be better borne, the pressure employed must be progressively increased, until at last considerable force is employed, the *massage* being continued for a quarter or half an hour, a whole hour, or more, until the pain ceases and the tumefaction has diminished. From time to time, slight intermitting pressure or percussions are employed, while the joint itself is moved. These movements, scarcely perceptible at first, are gradually to be increased, until at the end of the *séance* all the proper motions of the part are easily executable. In this procedure the pain must always be kept within bearable limits; and, in fact, its whole principle consists in enabling a part, for which the slightest touch is at first so painful, by the aid of carefully directed efforts, to execute all its natural motions with complete ease. The manipulations over, cold lotions are to be applied and rest enjoined. As to their repetition, this will vary in different cases, and will be indicated by the re-appearance or augmentation of the pain after amelioration. In a recent case, and when the swelling is slight, one or two manipulations of moderate length and activity are often sufficient to secure a cure; while, if the sprain date back for days or weeks, and the lesions are deeper seated, more powerful and more prolonged procedures are required. In one of the cases which M. Berenger-Féraud relates, a chronic sprain had to be brought, so to say, to the acute stage before it could be relieved.

In reply to the question whether sprain can always be thus cured, it may be answered affirmatively whenever we have to do with a simple case, unattended by severe complication, such as considerable laceration of ligaments, detachment of tendons, fracture of articular surfaces, and lesions of important vessels or nerves; and also when the manipulation is conducted with skill and patience during a sufficient period of time.—*Bulletin de Thérapeutique*, January 30.

Apparatus for Dressing Wounds.—Dr. Morton, of the Pennsylvania Hospital, believing that one great source of erysipelas, pyæmia, and sloughing observed in surgical wards is due to carelessness in the use of sponges and other means of cleansing wounds, has contrived an apparatus which has been found to work well in his hospital. His object was to dispense with all basins and sponges, substituting oakum and a constant supply of fresh running water, thereby diminishing greatly the chances of inoculation.

"I had constructed an apparatus which [consists of a table or stand 3 feet high, 3 feet long, and 17 inches broad, supported on

three wheels, two of these being 20 inches high (and noiseless), and the third smaller one, placed in front, moving on a pivot so as to allow the apparatus being wheeled in any direction. On the centre of the table is placed a tin reservoir capable of holding 6 or 8 gallons of water. To a stopcock at the bottom, a small-sized gum-elastic tube, 9 or 10 feet in length, is attached, having at its extremity a metallic tube, 4 inches long, also provided with a stopcock. On either side of the reservoir appropriate places are partitioned off for bandages, charpie, oiled silk, adhesive plaster, and jars for holding lint soaked in the various solutions commonly used for ward applications. Drawers at each end serve as receptacles for towels, instruments, &c.

"A large tin bucket, with a projecting lip, occupies one half the space under the table, into which all the refuse dressings, poultices, soiled water, and oakum from each patient are emptied. Another can, with two compartments, adjoins the bucket, into one of which the soiled bandages are thrown, while the other holds fresh oakum, cotton batting, muslin, and old linen required for the purposes of the ward. One good-sized tin bucket sufficies for the entire ward or series of communicating wards, since it serves merely as a receptacle for the water and discharges flowing from the parts during the process of dressing. A can of hot water for heating the adhesive plaster completes the apparatus. This is pushed to the foot of each bed, the length of the gum-elastic tube and the elevation of the water-supply allowing the stream to be carried to any part of the patient which requires cleansing, in either the recumbent or sitting posture. The apparatus may be considered as essentially labour-saving, since it combines, in a compact form, all the materials for washing and dressing the wounds, and removes all the refuse from the wards."

A woodcut is given of this commodious travelling dresser. — *American Journal of Medical Science*, January.

On Calculi and Diverticula of the Urethra.—Professor Grube, of the University of Charkow, states that during six years' private and hospital practice he has met with 58 noteworthy cases of urethral calculi, the fossa navicularis having been almost exclusively the portion of urethra in which they were placed. Of these, 49 were removed by some of the various forms of forceps or other extracting instruments; in 5 perineal urethrotomy was performed, in 2 lateral lithotomy, and in 2 urethrotomy of the spongy portion of the urethra. In 2 of the cases in which perineal lithotomy was performed, death ensued, both patients exhibiting symptoms of uræmia, and in both diffuse renal inflammation being found after death. Of the most remarkable of his cases the author gives some details.

1.—Boris Dolgoff, æt. 13, about three years before admission into the clinic perceived, soon after riding on horseback, a hard substance in the urethra. After much suffering, an abscess formed in the middle of the spongy portion. On examination when admitted, an urethral calculus was found under the pubic arch, accompanied by a fistulous opening in the urethra, through which the principal part of the urine

dribbled away. The boy was in a wretched state of health. The calculus was next day removed by urethrotomy with considerable difficulty, owing to its size and fragility, the patient recovering so rapidly that he was discharged within three weeks of the performance of the operation. Seen three years afterwards, it was ascertained that the urethra continued quite free, and that no incontinence of urine persisted. The stone weighed 5 ounces and 7 drachms, and measured $1\frac{3}{4}$ inch in diameter, $7\frac{3}{4}$ inches along its outer and larger semicircular border, and $4\frac{1}{2}$ inches along its lesser inner border. Of this extraordinary calculus, which Professor Grube regards as well-nigh unique, he furnishes a woodcut.

2.—The other cases detailed are examples of a much rarer affection, urethral diverticula; and of this formation four examples are given, two accompanied by and two without calculi.—*Berlin Klin. Wochenschrift*, 1867, No. 5.

Summary.

Club-foot.—Bartscher. Treatment of Club-foot with Water-glass Bandage. (Journal für Kinderkrank. October.)

Dislocation.—Meschede. Two Cases of Dislocation of the First Phalanx of the Thumb Forwards. (Virchow's Archiv, Dec. In one of these cases the post-mortem appearances described forty-eight days after the accident; in the other, reduction was easily effected by dorsal flexion.)—On Dorsal Dislocation of the Thumb. (Wien. Med. Woch., 1866, Nos. 94 and 95. A case of Lorinser's described, in which luxation, complicated with interposition of the tendon of the long flexor, was reduced by powerful extension, aided by turning the thumb outwards, so as to throw the dislocated phalanx to the ulnar side of the carpus.)—Laroyenne. Case of Recent Irreducible Dislocation of the Thumb Backwards. (Mém. de la Soc. de Méd. de Lyon, tome v, p. 276.)

Ear.—Garrigen-Desgranges. Removal of Polypi of the Ear by the Ecraseur. (Gaz. des Hôp., 1866, No. 146. With woodcuts.)—Pomeroy. Inflammation of the Cavity of the Tympanum in relation to the Exanthemata. (New York Journal of Medicine, January.)

Excision.—Van Biervelt. Statistical Account of Excisions performed in Langenbeck's Clinic, 1851-65. (Journal de Méd. de Bruxelles, January.)—Watson. Excision of the Knee-joint. (Edinb. Med. Journ., January. Describes a new apparatus for after-treatment, with woodcuts.)—Heine. Excision of the Upper Maxilla, especially with reference to the palatal arch. (Deutsche Klinik, 1866, Nos. 44 and 47.)

Eye.—Sands and Allin. Two Cases of Sudden Monocular Amaurosis. (New York Journal, November.)—Arlt. Retinitis Nyctalopia. (Wien. Med. Jahrb., 1866, Heft 6.)—Rydel. On Glaucoma. (Ibid. Report on 79 cases observed at the Vienna Eye Clinic.)—Green. Detection and Measurement of Astigmatism. (American Journ. of Med. Sci., January.)—Stellwag von Carion. On the Hæmodynamic Conditions of the Eye and Intra-ocular Pressure. (Wien. Med. Wochenschrift, 1866, No. 92.)—Oettingen. Case of

Exophthalmos from Thrombosis of the Vena Ophthalmica. (Petersburg Med. Zeit., 1866, No. 7.)—Giraud-Teulon. Mechanism of the Production of Posterior Staphyloma. (Annales d'Oculistique, November.)—Javal. Four Cases of Convergent Strabismus. (Ibid., January.)—Schweigger. On Strabismus. (Monatblatt f. Augen., January.)—Liebreich. Modification of the Operation for Strabismus. (Archiv für Ophthalm., Band xii, Abt. 2.)—Wegner. Experimental Investigation concerning Glaucoma. (Ibid. To ascertain whether neuralgia of the trigeminus is a cause or complication of glaucoma.)—A. von Graefe. On Neuroretinitis and Sudden Blindness. (Ibid.)—A. von Graefe. On Sympathetic Ophthalmia. (Ibid.)—A. von Graefe. On Conical Cornea. (Ibid.)

Fracture.—Packard. Fractures of the Upper Extremity. (New York Journal, October—January. Chiefly occupied in discussing the agency of muscular action in effecting displacement.)—Moritz. Statistics of Fracture. (Petersburg Med. Zeit., 1866, No. 10. Relates to 1069 fractures treated at the Obuchow Hospital, St. Petersburg.)—Valentin. Subcutaneous Tenotomy in the Difficult Reduction of Fractures and Dislocations. (Archives Gén., March. Two cases given.)—Duncan. Case of Ununited Fracture. (Edinburgh Med. Journal, February. Successfully treated by raising up the periosteum from the ends of the fractured bone.)

Gangrene.—Sachse. Case of Dry Gangrene of the Fingers and Toes. (Archives de Méd Navale, February. Occurring in a shipwrecked English seaman after alleged fasting for twenty-eight days.)

Hernia.—Bär. Hernia and Hydrocele with Deviations of the Testis. (Prag. Viertel., 1866, B. iv. Several interesting cases exhibiting abnormalities of position or formation of the sac. Illustrated with woodcuts.)—Ravoth. Hernia Inguinalis Externa Directa. (Deutsche Klinik, 1866, No. 44. Regarded by the author as an undescribed form of hernia which he has met with 15 times in 1077 cases of inguinal hernia.)

Irrigation.—Cahours. On Continuous Irrigation in Comminuted Fracture. (Bull. de Thérapeutique, February 15 and 28. Twelve cases given illustrating the advantage of the practice.)—Pettigand. On Mediate Irrigation. (Recueil de Méd. Militaire, December. Exhibiting the superiority of irrigation applied through the medium of caoutchouc contrivances, a great variety of which are figured.)

Laryngoscopy.—Cutter. Case of Aphonia. (American Journ. of Med. Sci., January. Successfully treated by extirpation of a fibro-epithelial neoplasm from the vocal cord. Woodcuts of the instruments employed.)—Follin. Case of Multiple Polypi of the Larynx treated by Thyro-hyoidean Laryngotomy. (Archives Générales, Feburary. Woodcut.)—Dufour. Four Cases of Polypi or Vegetations of the Larynx in Infancy. (Ibid., March).—Fournier. Extirpation of Intra-Laryngean Tumours. (Gazette des Hôp., 1867, No. 26. Woodcut of a new curved forceps.)—Navmtil. Case of Rheumatic Paralysis of the Muscles of the Larynx. (Wien. Med. Woch., 1867, No. 10.)

Nose.—Taylor. Operation to repair a Broken-down Septum

Narium. (New York Journal, December. Woodcuts.)—Bochdalek. Case of Double Fracture of the Cartilaginous Portion of the Septum Narinm, with Displacement. (Prag. Viertel. 1867, B. i. An elaborate account of an autopsy in an example of this very rare occurrence.)

Osteomalacia.—Otto Weber. On Senile Osteomalacia, and the occurrence of Lactic Acid in the Bones. (Virchow's Archiv, January.)

Ovariotomy.—Harris. Successful Case of Ovariotomy. (Madras Journal of Med., 1866, July). — Gignoux. Successful Case of Ovariotomy. (Gaz. Hebdom. 1867, No. 1.)—Krassowski. Notes on Cases of Ovariotomy. (Petersb. Med Zeit., 1866, No. 7. Seven new cases, making 15 which he has performed at the Hospital of Zarskoe-Selo, 7 proving fatal. He strongly advocates the treatmeut of the pedicle by the actual cautery.—Dubarry. Fatal Case of Ovariotomy. (Gaz. des Hop., 1867, No. 5.)

Parotid Gland. — Borelli. Tumours of the Parotid Region. (Gaz. Med. di Torino, 1867, Nos. 8 and 9. Two cases of operation, accompanied by valuable remarks on the diagnosis and cautions in operating.)

Plastic Surgery.—Gustav Simon. Contributions to Plastic Surgery. (Prag. Viertel., 1866, B. iv.; 1867, B. i. A report on plastic operations performed by him at Rostock during 1861-66, and especially relating to those on the lips and palate.)

Spina Bifida.—Caradec. Treatment of Spina Bifida. (Union Méd., 1867, Nos. 26 and 30. Three cases treated with iodine injections, two successfully.)

Syphilis. — Gerhardt. Syphilitic Disease of the Air-passages. (Deutsche Archiv, 1866, H. 6.)—Lagneau on Simple and Infecting Chancres. (Gazette Hebd., 1867, No. 10.)

Thoracentesis.—Guersant. On Thoracentesis in Children. (Bull. de Thérap., January 15.)

Tongue.—Collis. On Removal of the Tongue. (Dublin Journal, February.)

Urinary Organs.—Demarquay. On External Urethrotomy without a Conductor. (Bulletin de l'Acad. de Méd., February 12.)—Sonrier. Six cases of Successful Internal Urethrotomy. (Gaz. des Hôp., 1867, No. 17.)—Porter. Two Cases of Lithotrity. (Dublin Journal, Feb. With engravings of Crampton and Clover's syringes.)

Wounds.—Gross. Wounds of the Internal Jugular Vein. American Journal of Med. Sci., January. A useful summary of what is known on this subject, but with little original information.)—Richepin. On the Compression of Arteries. (Recueil de Méd. Mil., October. Describes and figures a new tourniquet of easy application on the field.)—Kovacs. On an Electrical Ball-Indicator and Forceps. (Wien. Med. Woch., 1866, No. 89. An ingenious instrument, described and figured, to be employed for the detection of balls when there is a difficulty, as in Garibaldi's case. The inventor has employed it several times with success.

QUARTERLY REPORT ON MIDWIFERY.

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I. The Non-Pregnant State.

1. *On Menstruation and Dysmenorrhœa.* Dr. Riedel. ('Monat. f. Geburtsk.,' Nov., 1866.)
2. *A remarkable Case of Myoma (fibroid) of the Uterus.* Dr. L. Neugebauer. ('Mon. f. Geburtsk.,' Dec., 1866.)
3. *A Case of Enormous Fibroma of the Ovary.* Dr. O. Spiegelberg. ('Mon. f. Geburtsk.,' Dec., 1866.)
4. *The Nature, Source, and Surgical Treatment of Hæmorrhage from Fibrous Tumour of the Uterus.* Dr. Matthews Duncan. ('Edin. Med. Journ.,' Jan. and Feb., 1867.)
5. *Cases of Ovariotomy performed on the plan of Extra-peritoneal Tying of the Pedicle.* Dr. Stilling. ('Deutsch. Klinik,' 1865.)

1. Dr. Riedel, in some observations upon menstruation, refers to the theory of Pflüger, who explains the escape of the ovum and the discharge of blood from the uterus as dependent upon developmental processes in the ovarium. These cause excitation of the centripetal nerve-fibres. This excitation having attained a certain height, a reflex action sets in, which induces an intense congestion, and finally rupture of vessels. In those forms of dysmenorrhœa which are independent of structural change, and are really dependent upon emotional disturbance or cold, Riedel advocates the use of a formula of Rademacher, which consists in a mixture of tincture of nux vomica and tincture of Castoreum Canadensis.—*Mon. f. Geburtsk.*, Nov., 1866.

2. Dr. L. Neugebauer relates the history of a remarkable case of *myoma* (*fibroid*) of the uterus. It was of enormous size. A fistulous communication was made spontaneously between it and the abdominal wall. Through this opening portions of the mass were at various times extracted. When first the patient came under notice, she was pregnant: she was delivered of a full-grown living child. She ultimately made a good recovery, the remains of the tumour shrinking to a mass the size of a hen's egg.—*Mon. f. Geburtsk.*, Dec., 1866.

3. Dr. O. Spiegelberg describes a case of enormous fibroma of the ovary. The patient was tapped, but only blood flowed. She died under symptoms of irritative fever. The tumour weighed sixty

pounds. No trace of the left ovary was found. The uterus lay beneath the tumour. The whole tumour was invested by peritoneum; and under this was a layer of fibrous tissue surrounding the tumour as with a capsule. The microscopical features are minutely described, and seem to establish the conclusion that the tumour really was a fibroma of the ovary. (But it may have had its origin in the left Fallopian tube or broad ligament.)—*Mon. f. Geburtsk.*, Dec., 1866.

4. *The Nature, Source, and Surgical Treatment of Hæmorrhage from Fibrous Tumour of the Uterus.*—Dr. Matthews Duncan contributes a full clinical and critical examination of this subject. After referring to the capillary form of hæmorrhage from the surface of the tumour—which has been clearly described before—he gives an example of fatal hæmorrhage from the opening of one of the venous sinuses connecting the tumour with the uterus as in pregnancy. Dr. Duncan then discusses the operative resources—incision, enucleation, dilatation of the cervix uteri, avulsion. Some instructive cases in illustration are related.—*Edinb. Med. Journ.*, Jan. and Feb., 1867.

5. Dr. Stilling continues the report of his cases of *ovariotomy*, performed on his plan of extra-peritoneal tying and healing the pedicle. In commenting upon one fatal case, he urges strenuously that ovarian cysts should never be tapped, on account of the adhesions thus produced. Ovariotomy, where there are adhesions, he says, is a hazardous operation, but a very simple one where there are none. The operation should be undertaken as early as possible in the progress of the disease.

In another case, there was great difficulty in getting at the pedicle, owing to the complication of a second tumour. It was at length secured by a Hutchinson's clamp. The patient died on the twelfth day, of trismus and partial tetanus. Of three cases in this report, one recovered.—*Deutsch. Klinik*, 1865.

II. Pregnancy.

1. *On the Maternal Placenta at the end of Pregnancy.* By Prof. Hegar. ('Mon. f. Geburtsk.,' Jan., 1867.)
2. *On the Influence of Pregnancy and Childbed upon the Vital Capacity of the Lungs.* Prof. Dohrn. ('Mon. f. Geburtsk.,' Dec., 1866.)

1. Prof. Hegar, referring to Prof. Dohrn's recent researches on the anatomy of the mature human ovum-membranes (see 'Mon. f. Geburtsk.,' vol. xxvi, and 'N. Syd. S. Year-Book,' 1867), observes that Dohrn's conclusions differing in some important points from his own previously published, he has thought it necessary to subject the matter to fresh examination. Dohrn says, the presence of a connective stroma in the decidua vera is rare. To this Hegar

answers, that Kilian, Kölliker, and Schrœder van der Kolk attest the presence of a fibrous connective tissue. But the denial by Dohrn that the maternal placenta exhibits a retrograde metamorphosis calls for more special notice. (The reporter observes that retrograde metamorphosis of the decidua of the placenta was first distinctly contested by himself, 'Proceedings of the Roy. Med.-Chir. Soc.,' 1853.—R. B.) Hegar has examined twenty mature placentas. He argues, with Dohrn, that there is an active new cell-formation in the maternal placenta at the end of gestation. He does not contend that the retrograde metamorphosis of the maternal placenta proceeds at an equal rate with that of the decidua vera; but he believes (as Dr. Druitt first maintained, 'Med.-Chir. Trans.,' 1853) that in mature labours it is always so decided as to account in a great measure for the loosening of the placenta. In the twenty mature placentas, he found nine had marked calcareous concretions, four with considerable and seven with slight concretions. From these concretions he distinguishes the simple white decolorations of the placental decidua which penetrate more or less deeply into the parenchyma, and are connected with degeneration of the villi. These are found in every placenta. Generally, the fatty degeneration maintains equal progress with the concretion-formation and the decoloration. The fatty degeneration is almost constantly most marked at the circumference of the placenta, and diminishes thence towards the centre. (This is what Dr. Druitt described.) In six placentas the degeneration was not considerable. In the tissues there were opaque molecules; many of the tissue-elements were quite free.—*Mon. f. Geburtsk.*, Jan., 1867.

[The reporter submits that these observations do not prove that fatty degeneration is a normal process preparatory to the casting off of the placenta. The specimens examined were from hospital patients, and were not sound physiological specimens. The calcareous concretions are certainly connected with depraved constitutions, are of pathological import, and not characteristic of healthy placentas. R. B.]

2. Prof. Dohrn remarks that the conclusion arrived at by Küchenmeister, Fabius, and Wintrich, that the vital capacity of the lungs after completed labour is not greater than during pregnancy, is so remarkable as to call for further examination. He has therefore examined 100 persons in the Marburg Lying-in Institution by aid of the spirometer. All sick persons and those awkward in the use of the spirometer were excluded. An increase of vital lung-capacity in puerpery was observed in 60 per cent., the mean increase being 338 cubic centimetres; there was no change in 14 per cent., and there was a diminution in 26 per cent.—the mean being 221 cub. centim. The result differed in primiparæ and in multiparæ. In 36 primiparæ the vital lung-capacity in puerpery was greater than in pregnancy in 53 per cent., equal in 22 per cent, less in 25 per cent. In 64 pluriparæ, it was greater in 64 per cent., equal in 27 per cent, less in 9 per cent. Not only was the proportion of cases in

which the capacity of puerpery exceeded that in pregnancy greater in pluriparæ, but the mean increase was greater, being for pluriparæ 458 cub. centim., against 244 for primiparæ. This result indicates that primiparæ are more severely affected by labour and the consequences, and are not so powerfully muscular as those who have already gone through a labour. The result given is not due to the younger age of primiparæ. The general conclusion is, that the majority of puerperæ have on the twelfth to the fourteenth day a greater lung-capacity than in the last weeks of pregnancy. The capacity of primiparæ ranged in pregnancy from 1800 to 3600 cub. centim., and in childbed from 1830 to 3700; in pluriparæ in pregnancy, from 1800 to 3450; and in puerpery, from 1800 to 3200. —*Mon. f. Geburtsk.*, Dec., 1867.

III. Labour.

1. *A Contribution to the Pathology of the Membranes of the Ovum.* Prof. A. Valenta. (Mon. f. Geburtsk.,' Nov., 1866.)
2. *On Maternities.* Dr. Lauth. ('Annales d'Hygiène,' Jan., 1867.)
3. *On the Value of the different Methods of Measurement for the Estimation of the true Conjugate Diameter in the Living.* Dr. Schröder, of Bonn. ('Mon. f. Geburtsk.,' Jan., 1867.)
4. *A Case of Dystocia from Displacement of the Arm.* Dr. Playfair. ('Brit. Med. Journ.,' Feb., 1867.)
5. *Fifty-first Yearly Report of the Occurrences in the Dresden Lying-in Institution.* Prof. Grenser. ('Mon. f. Geburtsk.,' Jan., 1867.)

6, 7, 8, 9, 10, 11. *Cases of Dystocia, &c.*

12. *A Cæsarian Section on account of a Fibroid in the Hinder Wall of the Pelvis.* Prof. Dohrn. ('Mon. f. Geburtsk.,' Jan., 1867.)
13. *A New Form of Forceps.* Dr. Inglis. ('Edin. Med. Journ.,' Feb., 1867.)
14. *On Cephalotripsy.* Dr. Kidd. ('Dub. Jour. of Med. Sc.,' Feb., 1867.)
15. *Contributions from the Gynæcological Clinique.* Dr. O. Spiegelberg. ('Mon. f. Geburtsk.,' Dec., 1866.)

1. The *irregularities observed in the placenta* in 2471 labours are thus classed by Dr. Valenta:—In 67 cases the seat of the placenta was near the os internum. Of these, hæmorrhages occurred in 17. In 4 more the placenta was distinctly prævious. Abnormal *adhesion* was noticed 52 times: in 25 of these the adhesion was too strong; in 27 cases the adhesion was too loose, and generally more or less bleeding. *Placentæ succenturiatæ* were observed 23 times; in one case there was *duplex placenta—i. e.* the subsidiary placenta

approached in size the main placenta; in several cases there were two or more succenturiate masses. Twelve cases of remarkable *size* occurred. Anomalies of texture were observed in 697 cases. So-called *fibrin masses* were found in 267; they were usually found on the fœtal surface; only in 23 cases did the masses extend through to the maternal surface. The so-called fibrin ring, generally combined with fibrin masses, was seen 152 times. *Cysts*, generally with serous contents, were found under the amnion, from which they were easily separated, in varying number on the fœtal aspect of 56 placentas: these, at the same time, mostly showed fibrinous degeneration. *Calcareous deposits* were found in 136 placentas, always on the uterine surface, excepting in two or three cases; but in these the calcareous masses so penetrated the placenta, that it was hardly possible to find a place where the knife could penetrate. *Recent apoplexies* were found in 13 cases. *Fatty degeneration* was seen 16 times, mostly in syphilitic mothers. *Œdema* was seen in 21 cases. *Inflammation*, as revealed in distinct purulent collections and inflammatory friability, was seen 6 times. Of *anomalies of the cord* were noticed—prolapsus, 23 times; *torsion*, 4 times; twistings, 593, —round the neck, 467 times, once; 59 times, twice; 11 times, 3; twice, 4. True *knots* were found 19 times. *Spontaneous breaking* during labour occurred twice. *Dropsy* of the cord was noticed 4 times. The *root* of the cord was eccentric 1950 times; central, 342 times; marginal, 158; velamentous, 21 times. *Anomalies of the liquor amnii.—Hydramnios* occurred in 104 labours; an *extremely small quantity* was observed in 9 cases. *Hydrorrhœa*—that is, a watery or gelatinous collection between chorion and amnion—occurred in 80 labours; whenever the liquor amnii was discolored, these collections had the same colour. *Discolored liquor amnii* occurred in 211 cases. *Anomalies of the membranes.*—These were *too tough* in 375 labours, so as to require artificial rupture when near the outlet; *too delicate*, leading to premature rupture, in 580 labours. Frequently there were fresh *blood-extravasations* between chorion and amnion, or on the outer chorion-surface.—*Mon. f. Geburtsk.*, Nov., 1866.

2. This is an elaborate memoir on Maternities, by Dr. Lauth, of Strasbourg, in the 'Annales d'Hygiène,' &c., Jan., 1867. In it are fully discussed the conditions necessary for preventing puerperal fever. He recognises, with Le Fort and most recent French authors, the sad fatality that seems almost inevitably and especially to attach to lying-in hospitals. No doubt, the logic of facts will in time lead to efforts to establish charitable institutions for domiciliary midwifery; and then the difficult problem, how to construct and to administer a lying-in hospital with safety to the patients, will be less imperative. This subject is also discussed by Trélat, Le Fort, and Tarnier, in the 'Gazette des Hôpitaux,' 1866; also by Dr. Phelan, 'Dublin Q. Journ.,' Feb., 1867.

3. Dr. Schröder, of Bonn, discusses the value of the different methods of measuring the true conjugate diameter of the pelvis in

the living. Having measured many pelves in the living and in museums, he concludes that—1. The method of Baudelocque has no particular value in the estimation of narrow pelves; for accurate measurement it cannot be used,—at least, it is only useful as a controlling method. He verifies by examples the experience of Michaelis, who showed that the deduction to be made from the external conjugate diameter in order to estimate the true conjugate cannot be assumed as a fixed quantity, but varies extremely. Thus, in 68 pelves (28 normal, 40 normal) there were 7 cases in which a deduction of 2″ 10½‴ had to be made; 8 in which the deduction was 2″ 11½‴; 11 in which it was 3″ 1‴; 6 in which it was 3″ 2‴; 9 in which it was 3″ 4‴; 6 in which it was 3″ 6½‴. The deduction necessary ranged from a minimum of 2″ 4½‴ to a maximum of 7″ 7½‴, instead of the fixed deduction of 3″ to 3¼″ stated by Baudelocque. The kind of pelvis influences the amount of deduction. Thus, in dried pelves, the flat pelvis is distinguished by the greatness, the generally contracted pelvis by the smallness. A radical error arises in this—that in Baudelocque's diameter the right measurement is evaded. The promontory in almost every case stands above the line of junction between symphysis and the spinous process of the last lumbar vertebra. Therefore, if we add up the thickness of the symphysis, the thickness of the vertebral column and of the true conjugate, we almost always obtain a greater measurement than Baudelocque's.

2. The diagonal conjugate is of great importance for the recognition of the degree of pelvic contraction. If we cannot reach the promontory with two fingers, the true conjugate is not much contracted. If we can measure the diagonal conjugate, then we obtain, in a generally contracted pelvis by the deduction of 7‴, in the flat pelvis by the deduction of 8½‴, and in the rachitic pelvis of nearly 9‴, a near approach to the true conjugate.

3. If we wish to correct the figures so obtained (which it is especially necessary to do in rachitic pelves), and to get the true conjugate as accurately as possible, then we must resort to Van Huevel's pelvimeter. Hitherto this is unsurpassed for accuracy.—*Mon. f. Geburtsk.*, Jan., 1867.

4. Dr. Playfair describes a case of that rare form of dystocia in which arrest is caused by the child's arm being bent across the neck behind. It became necessary to perforate.—*Brit. Med. Journ.*, Feb., 1867.

5. Dr. Grenser relates that out of thirty-five cases of *laceration of the perineum* treated by the immediate application of one, two, or three sutures, all healed excepting two.—*Mon. f. Geburtsk.*, Jan., 1867.

6. In a case of distortion, the external conjugate was 6¼ in.; the true conjugate was 2½ in.; the abdominal walls were so thin that it was easy, by means of Baudelocque's pelvimeter, to measure the diameter of the child's head—it was 4 in. Being in labour, at term

the forceps was applied, and a living child, weighing five pounds, was extracted. The cranial bones were much bent in, and there was irreducible exophthalmos of the left side. It died in five hours, under "brain symptoms." Numerous fractures were found in the frontal bones, involving the orbits. The woman did well.—*Mon. f. Geburtsk.*, Jan., 1867.

7. Dr. Grenser relates a case of dystocia presenting some unusual features. A woman had suffered throughout pregnancy, and for years before, from a purulent vaginal discharge. It was found that she had necrosis of the right ascending ischium. A fragment of dead bone was removed. The pelvis was obliquely contracted, the distortion telling most upon the outlet. Forceps was first tried; then perforation and extraction by Busch's cephalotribe. The patient did well.—*Ibid.*

8. Dr. Grenser reports that out of 819 labours treated in the Dresden Lying-in Hospital during the year 1865, there had been 51 forceps operations, 6 cases of turning, 9 of extraction by the feet, 3 of perforation, 1 of Cæsarian section. 8 puerperæ were transferred to other hospitals on account of illness, as tuberculosis, &c.; and 11 died. There were 69 cases of abdominal inflammation, including all forms of puerperal fever.—*Ibid.*

9. In the clinical report of the Dresden Lying-in Institution, Dr. Grenser relates a case of extreme universal œdema in a woman pregnant for the third time, associated with tuberculosis. There was no albumen in the urine. The uterus, very large, reached to the apex of the heart, causing such dyspnœa that premature labour was indicated. This took place spontaneously, and twins were born. The œdema disappeared in a few days, but the patient had to go into hospital on account of the tuberculosis.—*Ibid.*

10. Dr. Grenser relates a case of albuminuria in pregnancy which has points of interest. A primipara had extreme œdema of the thighs and labia pudendi. Urine contained much albumen. When labour had lasted seven hours, and the child had been driven down to the outlet of the pelvis, progress was arrested by the swollen, stretched condition of the labia. These were punctured with a needle, with immediate good effect; in half an hour the child was born. During the first four days, the œdema disappeared from the extremities, but some ascites continued. Pulse rose to 120; temperature, to 31·5 C. This gradually lessened, along with the albuminuria, under digitalis.—*Ibid.*

11. Dr. Grenser relates cases of Cæsarian section performed on account of extremely contracted ricketty pelvis. A living child was extracted. Blood continued to flow from the lower end of the wound after dressing. The woman died on the fourth day. *Autopsy.* —Great tympanitis; adhesions of omentum to intestines; the uterine wound gaped. The pelvis had a true conjugate diameter of $2\frac{1}{2}$ in.; a false conjugate, caused by the descent of the last lumbar vertebra into the position of the true promontory, of $2\frac{1}{4}$".

12. Prof. Dohrn reports a case in which the Cæsarian section was resorted to, on account of a fibroid tumour seated in the posterior wall of the pelvis. The subject was a primipara. Labour having fairly set in, the operation was performed. Child alive. The mother died on the sixth day. Peritonitis was found. The pelvis was contracted generally, as well as by the tumour, which sprang from the right upper part of the sacrum, and projected into the cavity. The pelvis is figured and minutely described.—*Mon. f. Geburtsk.*, Jan., 1867.

13. Dr. Inglis proposes a new form of forceps. The principal modification consists in doing away with the ordinary handles, reducing these to the shoulders or stumps. He contended that good blades would hold of themselves, and that if traction be the principal use, the handle ought to lie across the line of traction, like the handle of a corkscrew. Dr. Charles Bell and Dr. Keiller vindicate the old form of handle, showing that want of length is simply want of power, and urging the difficulty of keeping the blades *in sitû* if the handles were stumped.—*Edin. Med. Journ.*, Feb., 1867.

14. Dr. Kidd (Dublin) contributes a memoir on cephalotripsy, in which he advocates the use of this instrument. He recommends it in all cases of embryotomy, in preference to crotchets, hooks, and craniotomy forceps, for the following reasons:—1. By its use, the base of the skull, the thorax, or the pelvis may be completely broken up and reduced to the smallest dimensions without injury to the mother. 2. It holds the part to which it is applied so firmly, that it can be rotated if necessary, and extracted with ease and safety. 3. It causes no spiculæ, as the crotchet does, to tear the soft parts of the mother. 4. It does not, like the crotchet, endanger the mother or the hands of the operator by slipping or perforating the parts to which it is applied. 5. It reduces the dangers of embryotomy to a minimum, and allows of its performance in cases where it would not otherwise be possible.—(The crotchet being chiefly used in the Dublin school, cephalotripsy seems to be more especially contrasted with the crotchet. The advantages are not so absolute as compared with good craniotomy forceps.—R. B.)

15. Dr. Spiegelberg gives the histories of two cases in which pregnancy was complicated with *fibroid tumours of the uterus*. In the first there was interstitial fibroid of the right side of the lower segment of the uterus. Labour came on spontaneously at about the sixth month, a macerated fœtus being born by breech presentation. During expulsion the tumour was flattened down, and recovered its rotundity afterwards. There was no hæmorrhage, and childbed went on favorably.—In the second case, there was a large interstitial fibroid in the fundus. Here also premature labour came with delivery by breech, child dead. Ergot was given during expulsion. Flooding followed Suppuration took place round the tumour; endometritis, and death. Peritonitis was manifest on the day after labour. On the sixteenth day, endocarditis was developed. The course was this:—On the fifteenth day, a strong rigor; sixteenth,

again rigor, œdema of the face and feet, systolic bellows-murmur, slight dyspnœa; seventeeth, diarrhœa; eighteenth, pain and swelling in the left sterno-clavicular joint. The diarrhœa was persistent; œdema of face increased; pulse fluctuated between 128 and 136. Twenty-sixth day, diarrhœa continuing, conjunctivitis, keratitis, exsudation in the anterior chamber of the right eye. On the next day, hæmorrhagic injection of the left eye; the urine became albuminous; sopor and collapse set in, and death on the thirtieth day after labour. *Autopsy.*—Exsudations around the uterus; eight ounces of clear fluid in the pericardium. Heart, especially left, enlarged; on its surface small ecchymoses; aortic valves thickened, rough. Both lungs adherent. In a large arterial branch of the lower lobe of the right lung, a soft, greenish *embolus*, extending into the neighbouring branches. Both kidneys very large, containing on surface numerous white, flat, projecting spots, surrounded by a red areola. Extravasation in the conjunctiva of right eye; retina filled with small hæmorrhages, with a white centre. The uterine tumour was a fibro-myoma. Purulent masses were found in the uterus, lymphatics, and surrounding the tumour; at the lower part, one of the purulent canals had perforated into the uterine cavity.—*Mon. f. Geburtsk.*, Dec., 1866.

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NOTICE TO READERS.

THE Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him, as also Inaugural Lectures, Dissertations for Theses, Medical and Scientific Addresses, &c.

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