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"JE PRENDS LE BIEN OU JE LE TROUVE."

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ON the true Homologies of some Structures, usually accounted Epithelial. By EDWARD GEDDINGS, M.D., Professor of Physiology and Pathological Anatomy in the Medical College of Georgia.

The rapid advance of cellular-physiological and pathological doctrines in recent times, which bids fair to bring about a total reform in medical science, lends a peculiar interest and value to any attempt to trace out the natural affinities of the cellular elements, and the laws which govern their protean transformations. We have learned to refer everything essentially vital, the prime movement in every living act, not so much to the adjustment of the living mechanism as a whole, but to the individual cellular elements themselves, to regard the organism as a society of living Monads, each of which contains within itself all that gives peculiarity to its acts, while it is dependent on its association with the rest only for a supply of the physical conditions necessary to every manifestation of vitality, food, warmth, moisture, and oxygen. There is
little doubt in the mind of any real physiologist of the present day, that could we, without injury, transplant a single organic cell from one organism to another, in which it would be furnished with the necessary physical conditions, it would continue to grow, produce its kind, and perform all other characteristic acts exactly as in its original site.

Under such circumstances it is of the last importance, to the just appreciation of any physiological or pathological action, that we should comprehend, as far as possible, the actions of the individual cellular elements into which it might be analysed. We must not only endeavor to understand those characters which distinguish all kinds of cell life, but those peculiarities in which they differ one from another. We must compare them, trace their similarities and differences, assemble them in groups, according to the degree of their affinities; in a word, classify them.

In a series which, like the constituent cells of the living organism, start from a single typical form, and reach their ultimate characteristic qualities by a successive transformation and repeated divergence, it were natural to expect to find a number of sharply-defined groups, such as have been so beautifully established in the great series of animate beings, class, order, and species, in which the later divisions would contain the qualities of that which preceded them, while differing from each other in other respects. Very little has been done in this direction. We have numerous classifications of tissues, from Bichat down, all bearing more or less the impress of the peculiar tendencies of the school from which they emanated, but a real natural classification of cells has never been attempted.

Nevertheless, the researches of the great micrographists of our day; Reichert, Henle, Remak, Robin, Virchow,
Kölliker, and others, clearly indicate the limits of the first great division, the classes. We have gradually become familiar with three great divisions of animal cells, to which most of the existing forms could, without much difficulty, be referred, viz.: the Epithelial, the Connective, and the Nervo-muscular.

It is not the object of this paper to pursue this classification farther, nor to show how far such a division of cellular elements into the three great classes mentioned, is justified by known facts. But assuming it to be correct, it is intended to exhibit that certain structures, which have been usually reckoned among the epithelial, ought in reality to be referred to the connective series. Such are the serous membranes, the lymphatic ganglia, and the blood-cells.

In the classification of cells, as of other series of natural objects, we must not rely too much on one or more prominent characters, but should, as much as possible, compare all their known qualities. The failure to follow this rule has been a fruitful source of error in the history of zoological classification. I need only allude to the marsupials, the cirripeds, and others. So, in the classification of cells, the general observations that most free surfaces were covered with epithelium, and that epithelial structures consisted of closely-packed cells without interstitial substance were alone deemed sufficient to place the serous membranes among the epithelial structures. So, likewise, the general resemblance of the lymphatic ganglia to glands, and a supposititious function, led to a classification of the lymphatic ganglia, among epithelial structures. Neither of these generalizations will bear the test of closer scrutiny, and a comparison of all the known characters of such structures.

In the present state of our knowledge of cell-life, I think that characters for comparison can be drawn from
the development of cells, from their anatomical form and connections, their physiological and pathological transformations, one into the other, and their living acts under normal and abnormal conditions.

One of the earliest acts of the embryo, after the fissure of the yolk, is to divide itself into three parts, from which very different structures arise—a lower or visceral germinal membrane or layer, an upper or animal layer, and a medullary tube, the foundation of the nervous centres. Now, it was shown by Remak, long ago, that nearly all glandular and epithelial structures were derived from the lower or visceral layer. This membrane, in conjunction with the animal layer, curves downward and inward, and gradually encloses a tube or cavity, which eventually becomes the intestinal canal. Conjunctiva nares and pharynx are originally parts of this great cavity, and remain connected with it throughout life. From it are developed many homologous structures, all undoubtedly epithelial, viz.: the lachrymal glands, the salivary glands, the lungs, the thyroid gland, the thymus, the liver, the pancreas, and the genito-urinary cavity (allantois, corpora wolffiana, ductus Mülleri). All these structures are developed in exactly the same way, by a process of gemmation. One exception is usually admitted to this law of Remak, the epidermis, with its appendages, nails, hair, and horns. But there is just a possibility of error here. It is well known that, at a very early period of embryonic life, two buds or protrusions, one before and one behind, are shot off from the margin of the umbilicus, which, curving over the ends of the embryo, gradually envelop it in a double membrane, or bag. As usually described, these vesicles are developed from the animal layer alone, and the outer bag coalesces with the chorion, while the inner becomes the amnion. But
it is possible, in view of the great difficulty of investigation at this period, that the visceral layer may likewise follow the animal, in the protrusions, and that the outer bag should become the amnion, while the inner would become the epidermis. This would give the embryo a covering of epithelium, and the amnion the epithelial lining it undoubtedly possesses at a later period.

As is known, the majority of connective structures, such as the bones, cartilages, ligaments, tendons fasciae as well as the loose areolar tissue, is developed from the upper or animal layer. It may be considered an open question, whether the same distinction can be shown between these and the third group, the nervo-muscular. It would seem like a heresy to say that nerves and muscles are developed from the medullary tube, and not from the animal layer, where we usually find their first traces. But such will eventually be, I believe, the established fact. It has been well made out, that the nervous portion of the organs of special sense are developed by protrusion from the anterior extremity of the medullary tube, and it would be but a fair analogy to suppose that other nerves were evolved in the same way. In point of fact, we see about this time a series of dark lines shoot out from the medullary tube, one for each vertebra, and extending even beyond the limits of the embryo. It is impossible to say that these are the foundation of the future nerves and muscles, but it is not impossible. Again, when any part of the medullary tube, from which the organs of sense arise, is defective, the corresponding organ of sense is wanting also. Now we find the same law in regard to other nerves and muscles. When any portion of the nervous centres is defective, not only are the nerves which should spring from it wanting, but the muscles also, even the heart. This well-known law of pathological embryology does not hold the other way, for the muscle may be
wanting, while the nerve is intact, or the nerve be defective, while the centre exists. These facts furnish strong presumptive evidence that nerves and muscles are originally developed from the medullary tube, but at so early a period that it has escaped attention.

The three great types of cells are thus distinctly laid out at the earliest period of embryonal life. Subsequently they extensively interpenetrate each other, but at first they are distinct. It will not be difficult to show that the serous membranes are developed from the second or connective series.

When the division takes place between the visceral and animal layer and the medullary tube, there remain two cavities, which mark the separation. These are the great serous cavities, the pleura and peritonaeum (at first one), and the arachnoid and their walls are the serous membranes. Now if we examine the evolution of the embryo more attentively, we find that it is only the lining of the visceral and medullary tubes, which become respectively the epithelial and nervo-muscular structures. Outside of each of these remains a layer of cells, which, in one case, becomes the fibrous connective stroma of the intestine and glands, and in the other the fibrous portion of the arachnoid and neuroglia. So that the great serous tissues are developed from and among cells of undoubted connective type. The synovial capsules, sheaths, bursae, etc., are of course developed from the same source, but what is more to the purpose, they may, at any time, develop adventitiously. Wherever two parts slide over each other, there we will find a bursa make its appearance, and we can distinctly trace its formation, from the loose connective tissue, by the gradual proliferation of the stellate cells, and the secretion of mucine instead of fibrogenic material, by a partial reversion, therefore, to the embryonic type.
All this applies equally well to the blood-vessels, and their serous lining, the endangium. They make their appearance in the upper or animal layer. Some embryologists admit a special or vascular layer, where the vessels are developed, but this is incorrect. It is never truly separated from the upper or animal layer. As is well known, the first blood-cells are developed from the same cells as the walls of the vessels, and in situ. Precisely the same thing may occur at any period of life. Wherever we have a new growth, from connective tissue, there will we find a new formation of blood-vessels, and according to some of blood-cells, perfectly distinct from already existing vessels. But this is only from connective structures, never from epithelium. We may have the most massive development of epithelial cells, as the cholesteatoma, or pearl tumor of Müller, where they form large rounded masses of closely packed cells, or in horns, but never do we see such structures penetrated by vessels, unless they are, at the same time, permeated by a stroma of connective tissue. Very instructive, in this respect, is the comparison of true horns and antlers. Both are intended for the same purpose, but one is epithelial, the other connective structure. The first are never permeated by vessels, the other always during the period of their growth.

From the anatomical form we can glean but little in support of our argument. The serous cells exhibit indeed a tendency, but a tendency only, to assume the characteristic fusiform and stellate shapes of connective cells, especially in the blood-vessels, but as similar indications of a tendency to assume the fusiform shape at least are to be found among cells, undoubtedly epithelial, in the urinary passages, the sign is of little value. Even the most characteristic peculiarity of connective tissue, the presence of the intercellular cement, is wanting in the


Geddings on Epithelial Structures.

The cells are as closely packed as the pieces of a mosaic. This may be the reason why they do not send out their characteristic prolongations. Both characters, however, make their appearance when the structure hypertrophies. Something like this is normally observed in the synovial structures where the cells never form a continuous covering, but the interstitial substance is here and there uncovered. Here we not only find fusiform, but even star-cells.

Very important, on the contrary, are the anatomical transitions. If we compare vertical sections of serous and mucous membranes, as we can easily do in the intestine of small animals, where the two are in close apposition, we will find the margin of the mucous epithelium, where it strikes the connective stroma, sharply defined. Not so the serous. It is impossible to tell where the serous tissue ends, and the connective commences. As we pass outward we find the star-cells at first perfectly discreet, gradually becoming more crowded, and shortening their processes until they pass, by insensible gradations, into the cells of the serous covering. This is especially the case with the vessels, particularly those of small size. To see this distinctly, we should examine the vessels of the living animal, and I can recommend for this purpose the mesentery of the lizard. Not only are the structures more transparent during life, but when the vessel is contracted, as after death the condensed portion is drawn away from the rest, which gives an apparent distinctness to the vascular tubes, which they do not really possess. So, also, when the vessel is injected, the walls are compressed, and again appear more distinct. But in the living animal the vessels appear in their true character, as mere channels in the connective tissue. As we approach the cavity, the cells become more numerous and more crowded, and finally pass into the characters of
the endangium. I am here speaking only of the smallest vessels. In the larger the interposition of the elastic and muscular elements gives them the character of independent tubes, as well as their stratified appearance. But, if we imagine these taken away, even the larger arteries would appear as channels in the connective tissue.

The transition from connective cells to serous structures seems to have struck all observers. Thus Henle, starting from the idea that the serous covering was real epithelium, speaks of the connective cells beneath as "metamorphosed epithelium;" and Kölliker, commenting on the subject, says: "The similarity of these layers, which I will call the striated layers, with the fibre (connective) cells beneath, and the epithelium does not justify us in considering them as derived from the latter, since nothing proves that the two are genetically connected. * * *

But, it seems to me, allowed to consider them as originally of equal value, originally the same, but developed in different directions." (Kölliker Gewebelehre, p. 544.) Remak proposed to call the serous layer the cell layer, "because it, differing in this respect from other epithelium, passed into the connective structures without defined limits, so that we frequently can not tell where one commences and the other stops." (Ibid.)

It may as well be mentioned here, that our ideas of the structure of the capillaries have recently undergone a considerable modification. We no longer consider them as a network of star-cells, in the interior of which the blood circulated; nor do we regard them as formerly, as structureless tubes. They consist, like the endangium everywhere, of a mosaic of flattened fusiform, or even stellate cells closely fitted together. So delicate are they, so perfectly adjusted, that we fail to discover the limit between them; but the silvering method, as now prac-
ticed, plainly exhibits the outline of a cell around each nucleus.

The transition of serous structure to fibrous, is not the only one we can demonstrate. In every joint we can show the transition from cartilage to synovial structure. The latter, as is known, does not cover the head of the bone, the cartilage is quite bare, but where the capsule joins the cartilage, we can see every stage of transition, from one to the other. In the spleen and the lymphatic glands, we can distinctly trace the splitting up of the ends of the vessel, intima and all, into ordinary connective tissue. We will return to this presently.

In comparing the physiological characters of serous tissue with the epithelial and connective structures, it must be remembered that the two last represent classes, the physiological characters of which may be variously specialized in the different species that compose them, and it is not always easy to seize the general characters amidst the mass of detail. In general it may be said that it is the office of epithelial structures, to secrete some material, differing, more or less, from the general nutritive material from which it is made. The connective, on the other hand, performs mechanical functions, builds up the solid framework of the organism and of the individual organs, binds parts together and facilitates their movements upon one another. The distinction is marked enough if it could be carried out, but there are various difficulties. Thus, the epidermis, with its appendages, performs a mechanical part in protecting the subjacent structures, but it does so by a true secretion, the elaboration of a substance widely differing from the nutritive plasma, keratine. Again, all cells secrete, and so do the connective, but their secretion is the same everywhere; when young, mucine; when old, fibrogenic material. This would be characteristic enough if the epithelial
structures did not sometimes secrete the same materials. Mucine is secreted by laminated epithelium, but there remains the distinction that in general mucine is the secretion of ripe epithelium, but of young connective cells. More characteristic is the fibrogen material. True epithelium, sometimes secretes fibrine, or at least a coagulated proteine substance, closely resembling fibrine in its physical character, as we see in diphtheritic and croupous inflammations, and even in the ordinary inflammation of single-layered epithelium, such as lines the ultimate ramifications of many glandular structures, as the lungs, kidneys, salivary glands, etc. But too much weight must not be laid on the physical characters of the proteine bodies, for their protean transformations, under varying conditions, are but little known. Physiologically, fibrogen material and croupous exudation are two very distinct things. Never do we see the latter, like the former, undergo the transformation into fibrillated cement, never form those adhesions and indurations, so characteristic of connective structures. We may, therefore, say in general terms, that a structure which, when young, produces mucine, and when old, fibrogen material, belongs to the connective series.

Tested by these physiological characters, the serous structures clearly coincide with connective tissue. Their function is purely mechanical, to allow different parts to move freely on each other, and in this respect they assimilate to the loose areolar tissue. When under morbid conditions, two parts begin to move freely on each other which did not move before, there we will usually find an adventitious development of serous structure, and always from connective cells. When serous membranes secrete anything beyond the ordinary nutritive plasma, it is always mucine or fibrogen material, never more special secretions.
A striking peculiarity of epithelial structures is, that they are deciduous. They are being constantly cast off, and replaced by others. Not only do we find them in all secretions, but when these are retained, as in the first formation of all true cysts, they accumulate, and render the fluid opalescent or opaque. Nothing of the kind occurs with serous structure. The fluid is either transparent, or, if troubled, it is from coagulated fibrine or pus. The serous cells are not transient, but permanent structures, the duration of whose existence, like other connective cells, equals that of the organism.

As a physiological peculiarity of epithelial structures, may be mentioned their tendency to throw out protrusions, or crypts, which frequently branch, as in true glands. This we never see in serous membranes; they are always perfectly smooth, and never form glandular structures of any kind.

The above characters derived from their development their anatomical form and relations, and their physiological attributes would, I think, be sufficient to remove the serous structures from the epithelial series, and establish their homology with the connective, but the induction is still more strikingly borne out by the phenomena they exhibit under pathological processes. Herein lies the great importance of placing them in their proper class, since this at once gives us the key to the transformations they are likely to undergo.

When epithelial structures hypertrophy, either the cells, as they multiply, are cast off, or they remain attached, forming thick laminated masses, without interstitial substance. When the hypertrophy starts from the connective layers beneath, forming warty excresences, the epithelium is never broken unless by ulceration, but forms a continuous, more or less, thickened investment. With the serous membranes it is very different. Let any one
examine the vegetations so frequently found upon the valves of the heart or the pericardium, and he will find that they consist of connective tissue in various stages of development with well-defined interstitial tissue, without a trace of investing epithelium. Yet the latter has by no means been cast off, for such vegetations may cover the whole face of a serous membrane without our finding any cells in the serous exudation.

Very characteristic are the differences of the inflammatory process in the two classes of structures. In epithelium the active congestion is followed by a copious secretion of thin albuminous fluid, which soon passes into the ordinary thick glairy mucous. Simultaneously, there is a rapid proliferation of the epithelium, the cells of which are rapidly cast off from the outer layers, rendering the fluid at first opalescent, then opaque. At first the cells have all the characters of ripe epithelium, but as the process progresses, younger forms make their appearance; at first large rounded granular cells, gradually followed by smaller and smaller, until they reach the form of small round granulated cells, with tripartite or multiple nuclei, usually regarded as characteristic of pus. When the process runs off, the secretion diminishes by degrees, riper forms make their appearance, and complete "restitutio ad integrum" results, provided the process has been confined to the epithelium, and the connective structures beneath have not been laid bare. Such is the well known picture of ordinary catarrhal inflammation.

Nothing of the kind occurs in serous membranes. These structures inflame like a wound, i.e., like connective tissue, with a secretion of fibrogenic material and the formation of vascular granulations, and the result is invariably, unless the process has been confined to its earliest stages, not a complete "restitutio ad integrum," but a new growth of inodular tissue with the well known
indurations and contractions, characteristic of the inflammation of connective structures. The inflammatory vegetations of serous membranes, which are here put forward as the homologa of granulations, are larger and less vascular than these, but scanty vascularization is characteristic of serous membranes generally, and their large size is probably owing to the fact that generally all the cells ripen, whereas, in ordinary granulations, a large part are cast off as pus. It might be objected to the distinction, just tried to be established, between the inflammatory process in epithelial and connective structures, that the former may, and often do, secrete fibrine, and that the highest development of the process is identical in both, the production of pus; but, as already pointed out, the fibrine of epithelial structures (diptheria, croup, pneumonia, morbus Brightii), is not identical with the fibrogenic material of connective and serous tissue, for it, never organizes, never forms inodular tissue. In the next place, it may be doubted if the pus in these two cases is identical. The morphological characters are, it is true, the same in both. But what are they? The characters of young cells everywhere, of the young blood-cells in blood, and lymph of the embryonal cells in the egg. But do they secrete the same thing? We possess no reliable chemical analyses on this point. But if the nose might be allowed to take the place of such, I should say decidedly not. The odor of pus is so characteristic that it reveals to the practiced olfactories of the surgeon the presence of the smallest wound in any part of the body. But does any one recognize anything like this in a purulent ophthalmia, a violent coryza, a bronchitis, or a gonorrhoea? As a chemical character, every one must have observed that epithelial pus does not blacken silver instruments, or at least not to the same extent as ordinary pus. This would indicate the absence of unoxydized sulphur,
so characteristic of the pyone of Gueterbock. Catarrhal pus frequently undergoes putrid fermentation, and develops intolerably fetid odors, but they are, as everyone knows, different from those of ordinary pus.

In every respect the inflammation of serous membranes is identical with that of connective tissue. They secrete fibrogen material; a part of which forms ordinary fibrine; a part true fibrillated cement, and any cells that are developed, assume the characters of connective cells or true pus. Never do we find a thickening of the so-called epithelium, or a casting off of cells in mass, except as pus. The surface of the pseudo-membrane, or when formed, of the vegetations, is always bare, and we never find, as in croup and diptheria, the epithelium underlying the false membrane.

In general, the serous structure shows little tendency toward the production of pus, unless it is exposed to the air, or has been infected by a purulent deposit elsewhere, as in puerperal peritonitis, metastatic pleuritis, etc. Herein we have a most direct analogy with the connective tissues, for it is a fact familiar to every one, that the most severe contusions and lacerations of the connective tissue do not lead to suppuration, provided they are subcutaneous.

The formation of vascularized pseudo membranes, granulations, and adhesions, so common in serous membranes, are equally characteristic of connective tissue, for nothing of the kind is ever known to occur in epithelial structures. These never exhibit either vessels, granulations, adhesions, or contractions, unless the epithelium is broken, and the connective structures laid bare.

It might be objected that in all these cases the epithelium, as a single layered epithelium, is cast off, or degenerates (Buhl). But why does not the same thing occur in the inflammation of single layered epithelium, else
where? Why does not every pneumonia end in induration and organization of the exudation?

All this applies equally well to the endangium, for nothing is easier than to excite adhesive inflammation in the blood-vessels, and when a thrombus organizes, its newly-formed vessels communicate directly with the vasa vasorum, while the endangium remains intact, a circumstance that never occurs in any epithelial canal.

Most of the degenerative processes occur in both classes of structures. Thus the fatty and the amyloid degeneration, as well as the formation of pigment, occur as frequently in connective structures as in epithelium. All are found in serous membranes. To this the calcareous degeneration forms an exception. There is but one case where a deposit of lime salts takes place in epithelial cells under physiological conditions, and that is in the enamel of the teeth. The exceedingly rare calcareous degeneration of cataract is, perhaps, the only well-authenticated instance in pathology. On the contrary, nothing is more common than to find a deposit of lime salts, or even the formation of true bone in connective tissue. Both are common in serous membranes, and although such degenerations usually develop in the depths of the membrane, and subsequently extend to the surface, yet we can often distinctly trace at the margin of the deposit, the calcareous degeneration of the serous cells, especially in atheroma, and the osteophytes of the synovial membranes of joints.

Nine-tenths of morbid growths develop from connective structure, and all of these occur in serous membranes, but it is impossible to say, in any particular instance, whether from the serous cells or the connective stroma beneath. The tubercular process, however, offers us a case in point. A tubercle, according to the best and most recent authorities, consists of a small mass of cells,
formed by a rapid proliferation, and destined to undergo an equally rapid degeneration. Hence they never attain any considerable size, the process being carried on by a similar proliferation, and degeneration from other centres in the neighborhood. These may coalesce and form larger masses, but each tubercle must be regarded as a distinct development. Now, although nothing is more common than to find tubercles in mucous membranes and in the skin, they never take their origin from the epithelium, which may be traced in the early stages, passing over them perfectly intact. Nowhere is this more distinct than in lupus. Here the insulated tubercles extend for a considerable distance beyond the externally apparent margin of the disease; but we do not see them, because they are covered by sound epidermis. If however, we, with a probe, press somewhat rudely upon different points of this apparently sound skin, the instrument will penetrate every now and then into the tubercular mass beneath. In the tubercles so frequent upon serous membranes, it is different. The individual tubercles are upon the very surface, and we would look in vain for the serous covering. It is from the serous cells themselves that the morbid development has taken place.

Characteristic of epithelial structures are the various forms of epithelioma, of which we have several varieties, some benignant and others sufficiently malignant in their nature. These tumors, easily recognized by the epithelial nature of their elements, may penetrate any kind of structure, but they are never primarily developed, except from epithelium. This is not conceded by all, but the facts which seemed to indicate a transformation of connective cells into epithelium, under certain circumstances, such, for example, as the development of epithelial masses in the neighborhood of epitheliomata, but in the midst of connective tissue, observed by Schroeder van der Kolk,
have recently received a readier explanation in the discovery of the spontaneous movements and wandering propensities of many young cells. The fact remains that the first development of such tumors is always from epithelium. In accordance with this, we never see a development of epithelioma from serous membranes. When it occurs there, it is by extension from other parts.

Cysts are so much more frequent in connection with epithelial structures than anywhere else, that they may be almost said to be peculiar to them. If we except a few rare cysts, the origin of which is unknown, but which probably reaches back to an early period of embryonic life, and the pseudo-cysts, formed by degenerative processes in other tumors, perhaps all cysts occurring in connective tissue, and unconnected with epithelial structures at any period of their development, may be referred to hypertrophied or otherwise altered synovial bursae, normal and adventitious. This difference between the two classes of structures is also reflected in the serous membranes, for apart from the serous bursae, the rarity of cysts in serous membranes is well known. The only point in these where cysts are at all frequent, is at the broad ligaments of the uterus. But the real origin of these is to be sought in the corpora wolfliana of the embryo. They are analogous to cysts at the head of the epididymis in the male, and must be regarded as epithelial.

Thus, wherever we are able to establish a characteristic difference, anatomical, physiological, or pathological, between the two great classes of cells, the connective and the epithelial, we find the serous membranes everywhere corresponding with the former, and differing from the latter in every essential particular. In many respects, doubtless, the qualities of the serous structures seem to fade into those of epithelium; but when we review the
whole chain of phenomena, and assemble in one picture all the known qualities they present under the various circumstances, we can not escape the conviction that their true homologies are to be sought in the connective structures, bone, cartilage, and fibrous tissue, and not, as now universally assumed, in epithelium.

The value of such an attempt to establish the true relations of an extensive and important system of cells, will be very differently estimated by those who are, and those are not impressed with the great value of the discovery of the organic cell to the science of life. The first will regard it as idle speculation, while those who look upon cell-life as the future stand-point of far more general views in physiology and pathology than we now possess, will see in it an attempt to begin a labor which the science will have to accomplish, by more extended observation, and the superior resources which the future will certainly bring. The views above put forth, even though erroneous, have the advantage of connecting together and exhibiting in a clearer light many points long since empirically established in practice. We see, for example, why we can not treat an epithelial cyst like an hydrocele or a synovial bursa, by iodine injections; why it will not do to admit air into a serous cavity; why the inflammatory process leads to such widely different results in a serous and a mucous membrane; why the rheumatic process attacks and makes metastasis to every serous membrane in the body, while the numerous epithelial structures remain constantly exempt; why the erysipelatous pyemic and septic inflamations are so apt to extend to the serosae, while the catarrhal, croupous, and diphtheritic processes, peculiar to epithelial structures, never attack the serous membranes in their original form, and show little tendency to extend to them, even when in immediate proximity.
Whatever applies to the serosae generally, and especially to the endangiuni, will of course apply equally well to the lining of the lymphatics, which exhibits precisely the same structure. From the recent anatomical researches into the structure and termination of these vessels, we can gather some new facts in support of the views advanced. The smallest lymphatics, like the capillary blood-vessels, consist only of the lining membrane, which here also consists of closely-packed fusiform cells, the outlines of which can be easily demonstrated by the silvering process. According to Ludwig and Recklinghausen, each one of these vessels communicates with an irregular star-shaped vacuole of the connective tissue. Ludwig was able to inject these vacuoles, which farther communicate with one another from the lymphatics. R. found the lymphatics beginning with open mouths upon the serous membranes, and was able to follow, with the microscope, the direct absorption through them, not only of fluids and emulsions, such as milk, but also of solid powders, such as cinnabar. The absorption was frequently so rapid as to create a distinct whirl around the orifice. Chrzonzewski demonstrated, by an ingenious physiological experiment, the identity of the vacuoles, in which the lymphatics commence with the ordinary star-cells of connective tissue.

The experiment consists in tying the ureters of birds, which do not, like mammals, excrete their nitrogen, as urea, but as uric acid. The urates are then retained, and as sparingly soluble salts are soon precipitated in every part of the body. If the animal is killed a certain time afterward, every star-cell is found filled with an opaque granular mass, which may be shown by the murexide test to be uric acid. But what particularly interests us, the same granular mass may be traced through the rays of the cells into the minute lymphatics. Here, therefore,
we have the most direct transition of the endangium into connective cells. At the other extremity of the vessel a similar transition has long been known. Nearly all anatomists now agree that a lymphatic vessel on entering a follicle of a ganglion, immediately splits up lining, membrane, and all, into the connective stroma of the follicle, and that it is impossible to say at what point one begins and the other ends.

In the meshes of the sponge-like stroma thus formed, the so-called glandular elements lie closely packed, the whole mass constituting the follicle, being penetrated by a fine network of capillary vessels. The cells represent, for the most part, young, rounded, granular forms, of various sizes, and not to be distinguished from the white cells of the lymph, or from young cells generally. Besides these we almost invariably find fusiform cells, and other transition forms between the last, and the characteristic elements of connective tissue. This is particularly true of the spleen, which we must now regard as an enormous lymphatic gland. From the ultimate blending of these different kind of elements, we might at once infer that they were genetically connected, were it not for the impossibility of distinguishing the young forms of the various tissues. It is possible that we may have here connective and epithelial structures developing side by side. The permeation of the cellular elements, by a connective stroma and blood-vessels, is more characteristic of connective tissue, since it is very different from the habit of epithelial structure generally. But as this actually occurs, in at least one structure of undoubted epithelial nature, the liver, the character can not be relied on.

The generation of the lymph cells from the stellate cells of the stroma is assumed by many microscopists, and the true homology of the entire structure seems to have been more than once suspected by observers of the
first class; among others by Virchow. I translate the following passage from the most celebrated work of the great master: "I must once more call attention to the fact that the lymphatic glands differ from real glands in the ordinary sense of the term, not only in having no excretory duct, but in respect to their development. They stand by no means on the level of glands. On the contrary, their whole history assimilates them to the connective tissues, and we might be tempted to count them among those structures which arise from the metamorphosis of the connective series. But this would be at present a very bold undertaking." (Cellular Pathologic, p. 45.) To prove the homology thus hypothetically advanced, we must inquire farther into the history of the lymphatic elements. Although always regarded as secreting structures, no one has yet been able to demonstrate what they secrete. No new substance has been discovered in the lymphatics after passing through the ganglia. The constituents of the lymph are those of the serum, or rather of the nutritive plasma everywhere, and none of these constituents exhibits a marked relative increase, except fibrine, and even of this it has always been considered an open question whether it is a secretion of the structure, or whether it has passed into the lymph by diffusion from the blood. The point must however be now decided in favor of the first hypothesis. Fibrine does not pass, or passes with great difficulty, across the walls of the capillaries. We may have the most extensive dropsical effusions, we may have the blood-vessels subjected for weeks to a pressure sufficient to burst them, yet the exudation does not contain usually a trace of perfect fibrine. Wherever we find fibrine, such has not been derived from the blood, but has either been produced in situ or reached its position by open channels.

Fibrine must, therefore, be regarded as a secretion of
the lymphatic ganglia. Which of the two substances of which fibrine is now known to consist, the fibroplastic or the fibrogenic, is here secreted, or both, can not now be determined. The secretion of fibrogenic material is, however, highly characteristic of connective tissue, while it is not known to be produced by any epithelial structure: Fibroplastic material is almost equally so, but is found also in the crystalline lense, which belongs to the epithelial class.

The chief function of the lymph cells is that of serving as a matrix of the white blood-cells. Of this there can be no reasonable doubt. Indeed, if we reject the now exploded doctrine of a generatio equivoca of cells, we must either suppose these to be generated from the cells of the endangium, by direct division from the lymph cells, or in the ganglia. For the first of these methods there is no reason. The second is untenable, on account of the movement of the lymph in mass. The only fact which could militate against the third, the appearance of white cells in peripheric lymph vessels, has received an amply sufficient explanation in the recent discovery of peripheric lymph follicles in almost every part of the body. On the other hand it is confirmed by many pathological observations. Every nutritive excitement of the lymphatic ganglia causes an immediate increase of the white corpuscles of the blood, and usually a simultaneous increase of fibrine. This is notably the case in erysipelas, both the true and the phlegmonous in lymphangoitis pyæmia, etc. It is by no means as was once supposed, a general character of the inflammatory process, for there are many inflammations in which it does not occur. The most obvious connection, however, between white blood-cells and the lymphatic ganglia, is in the fatal disease leukaemia, in which the most enormous increase of white cells is found, but never unconnected with marked hypertrophy of the spleen, or lymphatic ganglia, or of both.
The lymph cells of the ganglia, and the white cells of the blood, are one and the same thing, in different situations. To prove that the white cells of the blood are connective in their nature, is to prove the same thing of the cellular elements of ganglia. This proof is given in the most striking manner in the study of thrombus. We owe very valuable observations on this point to the labors of Blandin, Lobstein, Stilling, and more recently of Billroth and O. Weber.

Very soon after the formation of a thrombus, a yellow spot appears in the centre, consisting of a cheesy mass of broken down fibrine, in which a few white cells are embedded, while the red have completely disappeared. The superficial resemblance of this mass to pus, led to the famous phlebitis theory of Cruveilhier. A little later, the detritus has in a great measure disappeared, and become replaced by numerous white cells, which might easily be taken for pus, even under the microscope. But they are not pus, as their subsequent metamorphosis proves. Virchow was at one time inclined to consider these cells as only the white cells remaining after the absorption or disintegration of the red, and to suppose that they were undergoing degeneration. But they are far too numerous, and frequently fill up nearly the whole space originally occupied by the thrombus. Their origin, by direct multiplication of the original white cells, has moreover been now distinctly observed. The next stage is the transformation of these same cells into connective tissue. The individual cells elongate, become fusiform, and ultimately send out anastomosing rays, while an indistinctly fibrillated cement makes its appearance. During the whole of these transformations, the mass is completely separated from any organized structure by a layer of coagulum, which gets thinner and thinner, and ultimately disappears. True to the law of connective tissue, blood-vessels...
soon make their appearance, and are at first perfectly distinct from the vasa vasorum, with which they subsequently freely communicate. Whether the first red cells which these vessels contain are formed in situ, or are derived from the general circulation, has not been definitely made out, but the first is, for several reasons, more than probable. When the process is complete, the walls of the vessel, the endangium, and the thrombus form one mass of connective tissue, and the vessel is completely and permanently obliterated.

These observations, if correct, render the homology of the cells of the lymphatic ganglia, and the white blood-cells with the connective series unassailable. They are moreover interesting as exhibiting what slight causes may cause one and the same cell to assume two very distinct anatomical forms. For the development of lymph cells into fibrous structure is not the only transformation they can undergo, nor indeed the normal one. Physiologically the lymph cells are the young forms of the red blood cells. This idea, although even now not universally admitted, has been steadily gaining ground for the last ten years, and for the following very good reasons: The red cells, being devoid of nuclei, are incapable of multiplication. This is borne out by direct observation, for no one has ever seen a red blood cell in process of division, which, if it occurred, could not have escaped attention in the innumerable microscopic examinations of blood, made in almost every part of the world, more especially, as it is easily demonstrated, in the blood of the embryo, previous to the appearance of the liver and spleen, when the red cells are still nucleated. Either, therefore, the red cells are permanent structures, or they are produced from other cells. It can, however, be easily demonstrated that there must be a constant production of blood cells during the whole period of existence. In the first place, the mass of
blood, and consequently the number of blood cells, increases directly with the weight of the body. Farther, the most excessive losses of blood cells from hemorrhage and other causes, are repaired within a few days. Lehmann has demonstrated a very considerable decrease in the number of red cells in the blood of the hepatic vein, and the complete identity of the bile pigment and hematoidine, the well known substance into which the coloring matter of the blood is transformed, whenever it is anywhere in the body removed from the cells, is strong evidence of the truth of his assertion. Brown Sequard injected the eliptical blood cells of birds into the vessels of rabbits, and observed that they could be found for several days performing the same functions as other blood cells, but then disappeared.

For these and other reasons, we must admit that there is a constant destruction and reproduction of red blood cells going on during the whole period of existence, and since they can not produce their kind, like other cells, and an autochthonous production of cells is no longer admissible in physiology, they must owe their origin to other cells. There is no reason why these should not be the white cells, which are poured into the circulation in such numbers and from so many sources. The splenic blood alone contains ten times as many white cells as ordinary venous blood, namely, one white for every fifty red. If, therefore, an amount of blood, equal to that of the entire circulation, should pass fifty times through the spleen, a number of white cells would be added to it sufficient to reproduce the entire number of red cells. From all we know of the velocity of the circulation, very few days would suffice for this. What then becomes of this immense number of young cells thus continuously poured into the circulation? That they are immediately destroyed without undergoing any transformation into
riper forms is contrary to all physiological analogy; yet no transformation of these structures, other than into red cells, has ever been observed under normal conditions. On the other hand, the direct evolution of red cells out of white, may be directly observed in almost every specimen of splenic blood, and frequently in the thoracic duct. The cell contracts, becomes clearer, while the nucleus becomes granular, and ultimately disappears. The cell then gradually fills with coloring matter, and becoming flattened and biconcave, exhibits all the characters of the perfect blood cell.

The origin of the red blood cells, therefore, clearly connects them with the connective series, but it must be confessed that the genetic connection is almost the only fact which points unequivocally in this direction. Their deciduous nature, and their secreting so highly specialized a product as haemato-crystalline, would indicate epithelial characters. Perhaps the valuable discovery of Alexander Schmidt, that the contents of the blood cells are fibroplastic, that is, contain one of the proteine bodies which are now known to form fibrine when made to react on one another, might be considered as additional evidence, were it not that the same substance occurs in the crystalline lense, a structure of undoubted epithelial character.

The physiological and pathological transformations which, next to their origin, furnish, as we have seen, the most valuable clue for determining the homologies of the cellular elements, are entirely wanting in the red blood cells. Never, under any circumstances, has a transformation into any other kind of structure been observed. A red blood cell, whether in or out of the circulation, is doomed to inevitable destruction. This peculiarity is probably due to the absence of the nucleus, for only young forms can undergo organic metamorphosis, and cells lose the power of generating their species with their nuclei.
The homology of the red blood cells, therefore, with the connective series, although from their origin, more than probable, can not be demonstrated with the same degree of certainty as for the other structures we have examined; but it would be far more difficult to exhibit as close a connection with any other structure in the body.

The views above advanced in regard to the lymphatic cells, are fully borne out by pathological observation. Hypertrophy of the lymph ganglia leads, not to a massive accumulation of cells, but to induration with the production of inodular tissue or a cheesy degeneration unknown in epithelial structures. All the morbid growths, peculiar to connective structures, are found arising from lymphatic ganglia, which might indeed be owing to the fibrous stroma, but on the other hand, those which are especially characteristic of epithelium are wanting. None of the forms of epithelium are primarily formed in lymphatic ganglia, and although possessing a distinct follicular structure, these never produce true cysts.

No transformation of the white cells after leaving their matrix has been observed, except those already mentioned into red blood cells and areolar tissue; but attention has been already called by Uhle and Wagner (Handbuch d. allgemeinen Pathologie, p. 203) to the probability of their giving rise to many varieties of morbid growth, when arrested in a thrombus or extravasation. This, if ever demonstrated, would throw much light on the now inexplicable metastases of many morbid growths.

Although the evidence adduced in favor of the homology of the serous-lymph and blood cells with the great series of connective cells, does not possess that degree of accuracy at present necessary to establish a scientific fact, or to raise the doctrine put forward above the rank of a mere hypothesis to be sustained or refuted
by more extended observation than the science now possesses, yet it is claimed, that it renders ripe for discussion the following important theses:

The distinction traced at an early period of the embryo between the connective and epithelial series of cells by the division of its germinal layers, is maintained throughout the whole of its subsequent evolution and through the whole range of pathology.

Epithelium is derived only from epithelium, and connective structure only from connective. The members of either group may be transformed one into the other, but never can one of the connective series be converted into epithelium, or connective cells be evolved from epithelial.

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**Protoxide of Nitrogen**, NO. \(\text{NH}_3\), \(\text{NO}_5 = 3\ \text{HNO} + 2\ \text{NO}\),

By J. P. H. Brown, Dentist, Augusta.

Protoxide of nitrogen, nitrous oxide, or laughing gas, was discovered in 1776, by Dr. Priestley; but its anaesthetic properties were not known until after the researches of Sir Humphrey Davy, in 1800, who then made use of the following language: "As nitrous oxide in its extensive operations seems capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place."

This prediction of Davy, it seems, was not fulfilled until 1844, when Dr. Horace Wells, a dentist of Hartford, Conn., conceived the idea of using this agent to relieve the pain in extracting teeth. To test the matter, he inhaled the gas himself and had one of his own teeth extracted. He was so impressed with the success of the gas as an anaesthetic, that he endeavored to introduce it into surgical practice. In 1847 he succeeded in getting Dr. E. E. Marcy, of Hartford, to allow him to administer the gas during an operation of removing a scirrhous testicle;
and in 1848 Dr. P. W. Ellsworth, of the same place, performed an amputation of a thigh on a boy. In this case Dr. Ellsworth remarks, that the gas was administered "with a success fully equal to that attained since, either by ether or chloroform." Shortly after this operation, Dr. S. B. Beresford removed an adipose tumor, six ounces in weight, from the shoulder of an adult.

After these operations, nitrous oxide was abandoned in surgical practice, until Professor Carnochan introduced it again in 1855. This eminent surgeon, in the Medical and Surgical Reporter for February 10, 1866, says: "Since my letter in December, I have performed four more capital operations on adults, viz.: one amputation of the thigh, one of the leg, the removal of a tumor from the side, and the extraction of a cataract, making in all, since last July, seven successful capital operations under the influence of anaesthesia produced by the nitrous oxide. I have also during this time used chloroform and ether in many operations, and my opinion in regard to the superiority of the nitrous oxide as an anaesthetic is still unchanged."

Since last September the writer has administered the gas some eighty odd times during operations of extracting teeth, with perfectly satisfactory results. As the effects from one administration seldom last longer than two or three minutes, it requires rapid operating when there is much work to be done. This briefness of its effects makes it not so desirable for operations in the mouth where there is much hemorrhage, particularly in the extraction of roots on the lower maxillary. In the practice of the general surgeon these difficulties can more easily be overcome. By alternating the inhalations of the gas with those of atmospheric air, patients have been kept under its influence for sixteen minutes. In one case for thirty-two minutes, with no bad effects.*

*Report of Society of Dental Surgeons of the City of New York, in Dental Cosmos for March, 1867.
The *modus operandi* of nitrous oxide in the production of anaesthesia, is not well understood. It is supposed to produce insensibility by its rapid stimulating properties, which overcome systemic excitability and cause the partial interruption of vital activity. "This power of nitrous oxide," remarks Dr. Ziegler in his Researches on Nitrous Oxide, "to produce anaesthesia by superoxidation, overstimulation, etc., is quite distinct from that of all other agents, more especially of the hydro-carbonaceous variety; for they induce the anaesthetic condition by non-oxidation and deoxidation of the system, and by directly checking chemico-organic reaction and annihilating sensibility and consciousness. The former, therefore, increases while the latter diminishes life action, with, in both instances, the same general result of insensibility of body and unconsciousness of mind, though relatively as different in character from each other as sleep is from stupor, or satiety from starvation."

When the effects of nitrous oxide pass off the system, there is none of that feeling of nausea and lassitude which usually attends the administration of chloroform and ether. This may be accounted for by the fact that the former agent increases the amount of oxygen in the blood, while the latter agents diminish it.

While I am not aware of a well-authenticated case of death from its use (though several reputed ones have been reported), I am free to admit that injury may be occasioned by its injudicious or excessive administration.

It can not be supposed that an agent capable of producing by its inhalation, in a few minutes, an anaesthetic condition of the system, can be so harmless as to be intrusted to the ignorant, and administered indiscriminately. In the first place, the gas must be pure. In order to have it so, it is not only necessary to have pure chemicals and a complete apparatus for its generation, but chemical
knowledge and the utmost care are required in its preparation, otherwise noxious gases may pass over with the nitrous oxide, rendering it impure and injurious to inhale. The use of nitrous oxide is improper in certain conditions of the system, as, for instance, where the lungs are seriously diseased; where there is organic disease of the heart; where there is a predisposition to cerebral congestion; and where the brain is diseased or any way unhealthy.

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*Ligation of the Axillary Artery.* By J. B. BAXLEY, M.D., of Richmond County, Ga., late Surgeon P. A. C. S.

Private J. W. Gomer, company B, twenty-fourth S. C. volunteers, received a gun-shot wound of the left arm on the 20th July, 1864, and was admitted into third Georgia Hospital, Augusta, Ga., on the same day. It was a flesh wound, a minnie ball having traversed the soft parts near the junction of the middle and upper thirds of the arm, internally to the humerus.

On the 29th of July, hospital gangrene, which had been prevalent in the hospital, invaded this wound, producing extensive sloughing in its vicinity, with great tumefaction of the entire limb, and a rapidly exhausting constitutional irritation.

On August 4th, the brachial artery sloughed, causing profuse secondary hemorrhage, whereupon the axillary artery was tied where it is embraced by the two heads of the median nerve. It was deemed impracticable to ligate at the point of injury on account of the local disorganization of the tissues, hence the selection of the nearest practicable point above it—the axillary region.

It is interesting to observe the result: on the third day afterward the large slough separated, leaving a healthy granulating surface, and in five or six days the swelling
of the limb had entirely subsided. The wound healed rapidly, and in due time the ligature came away. The patient was furloughed August 24th.

To hasten the separation of the dead tissues, a caustic solution of sulphate of copper was used; the general treatment was mur. tr. iron, thirty drops, three times daily, with brandy and a generous diet.

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A Paper on the Employment of Mercury in Cholera, Dysentery, and Diarrhoea, and upon the Question of its Action upon the Liver.* Read before the Morgan County Medical Society, by David Prince, M.D., of Jacksonville, Illinois, and communicated for the St. Louis Medical Reporter.

RESULTS OF EXPERIENCE.

1. Mercury is found by experience to produce changes in the function of the alimentary canal in health, increasing the amount of liquids expelled, and changing the color, apparently in consequence of the combination of mercury with sulphur, producing a sulphide or sulphuret of mercury of a green color.

2. Mercury is found by experience to change the action of the alimentary canal in watery diarrhea, so as to diminish or suspend the watery flow, and to suspend the symptoms which attend the inordinate effusion of fluid.

3. The conditions which most indicate the employment of mercury are those of acute and exhausting fluxes, most frequently occurring in hot weather, as cholera infantum in children, and cholera morbus in adults.

4. The power of mercury to correct other forms of intestinal derangements, especially those supposed to be attendant upon sluggish or obstructed portal circulation, is certainly established by experience, irrespective of any theory of its mode operating.

5. Experience establishes that, for the purpose of correcting intestinal derangements, mercury has more efficacy in acute attacks than in chronic states, in which

* This paper attempts to present a resume of the correct literature upon the question of the action of mercury upon the liver, more than to produce anything original.
inflammation has become established in some portion of the mucous membrane.

** THEORY. **

If these propositions as to the employment of mercury in diarrhoeal affections are correct, it follows that the effects are to be explained by supposing an influence produced upon the mucous membrane and the various glands connected with the alimentary apparatus, and not by a change effected in the composition of the blood.

This does not imply that the medicine may not, in whole or in part, be absorbed, and be carried into the circulation; but that if so, the action determines upon the organs where experience shows that the medicine directs its agency.

The local irritation indicated by pain, tormina, and inverted action of the duodenum, with entrance of bile into the stomach and its ejection by the mouth, is best explained by supposing a local action independent of absorption into the general circulation; and this supposition may help to explain the different effects of small and frequent doses, from those which follow the administration of large doses. In the one case a milder influence, more widely spread, might be expected; in the other, a stronger local impression, confined chiefly to the portion of the tube in which it is travelling along. This theory helps to explain why, in cholera infantum, a fourth of a grain of calomel, frequently repeated, may be more efficacious in correcting the condition of the mucous membrane than five grains given at once.

The passage of the medicine through the portal circulation necessarily carries it through the liver in its course to the general circulation; but this constitutes no more reason why mercury should be a special stimulant of the liver than holds for the salts of magnesia.

The detection of a larger amount of mercury in the liver in experiments upon animals than can be found elsewhere in the body would not prove that mercury is a special stimulant of the secretion of bile, but only that it lodged there for the time in greater quantity than could be found elsewhere. The probability of a portion of mercury being carried in the bile back to the duodenum, to be again absorbed and returned to the liver, might stand as a theoretic reason for a greater proportion of
mercury being found in the liver for a considerable time after the administration of it.

The question of the power of mercury to increase the flow of the bile rests entirely on the results of experiments, and not upon the observed effects of mercury in intestinal derangements.

If it could be proved that mercury is not at all a cholagogue, except as to emptying the gall bladder by the perturbed movements that are connected with vomiting, the place which mercury holds in therapeutics, as built upon observation, would not be disturbed.

Those uses of mercury which are inferred from the theory that mercury increases the flow of bile, should be abandoned, however, unless it can be established that the secretion of bile is increased under the administration of this agent.

In this connection it may be stated that the vomiting of bile in connection with the action of a full dose of calomel or blue mass is no evidence whatever of an increase of the secretion of bile, but only of the expulsion through an unnatural channel of that already secreted and accumulated in the bile ducts, or lodged in the gall bladder. No clinical observations can possibly settle the question for several pints of bile are supposed to be produced every day, to be again reabsorbed, after having performed its part in rendering food and medicine capable of absorption. The loss by vomiting, or by brisk purging, of a pint or more of this profuse production, would be no evidence of its more rapid formation.

Experiments upon animals prove that the amount of bile produced varies with the amount and kind of food, very much as the salivary glands obey the stimulants applied to the mucous membrane of the mouth.

Not many conclusive experiments have been performed to determine the quantity of bile which flows, and the influence of various agents in increasing or diminishing it.

Much attention has been attracted by a series of experiments performed in 1858, by Dr. George Scott, in the laboratory of Dr. Lionel Beale, in London.

A fistula was made between the skin and the fundus of the gall bladder of a dog, after having tied the common
duct so as to shut off all flow of bile into the duodenum.* The dog maintained an appetite, but grew lean.

Four experiments were made with calomel, with diminution each time of the fluid bile, and of the solids secreted.

Three grains of calomel were given on the 13th of June. The daily average of bile for two days previous had been one thousand nine hundred and sixty grains of fluid bile, one hundred and four of bile solids, and thirty-two of bile acids. The daily average for two days after the medicine, one thousand three hundred and fifty-eight grains of fluid bile, seventy of solids, and twenty-six of bile acids. The calomel in this case reduced the fluid bile and the bile solids one third, and the bile acids one fifth.

A second dose of six grains of calomel was given on the 16th of June, with a diminution of fluid bile two thirds, the solids one third, and the bile acids one sixth.

The third dose, of ten grains, was given on the 3d of July, with diminution of fluids and solids, but with increase of bile acids.

The fourth dose, of twelve grains, was given on the 7th of July, with reduction of fluid bile from two thousand six hundred and fifty-eight to one thousand seven hundred and twenty-four grains, the bile solids from one hundred and seventeen to eighty-five grains, and the bile acids from fifty-four to forty-five grains.

These experiments tend to overthrow the commonly received theory that mercury increases the amount of bile secreted, carrying with it those therapeutic indications based upon this theory, without, however, disturbing those uses of mercury based simply upon observation.

Dr. Brown, in the article from which the account of these experiments have been taken, quotes Dr. Scott as referring to three experiments of Köliker, in which calomel was given to a dog, and in two of which there was a diminution of the amount of bile secreted; and to Dr. Thadicum, who in some remarks upon this subject, before the Medical Society of London, states that in Vol. XIII of Virchow's Archives, are some experiments by Masler, proving that mercury does not make its appearance in the bile, nor increase the quantity secreted, even when given.

* Trans. New York State Medical Society, 1864, p. 267; article by James L. Brown, M.D., quoting from No. 3 Archives of Medicine, by Dr. Lionel Beale.
in very large doses, from twenty-two to fifty-two grains. Dr. Thadieum also alludes to some experiments by H. Nösse, and by H. Müller, in which it was found that the addition of calomel to food, which under ordinary circumstances produced a certain normal quantity of bile in dogs, diminished the amount secreted.

It may be claimed that the action of mercury upon healthy dogs can not settle its action upon diseased men; that while the amount of bile produced in a healthy dog may be diminished, the amount may be increased in a sick man. No direct experiment can be made to meet this point, but the analogy of cathartics, diuretics, and diaphoretics leads us to conclude that the action of medicinal agents upon healthy organs afford a strong presumption of their action in morbid states, and with some exceptional medicinal agents, that they are found to have similar effects upon man and the lower animals.

There is a surprising want of reference to the liver in the descriptions given of the results of mercurial action. The salivary glands and adjacent parts, and the intestinal canal, give signs of irritation, but the liver not.

In this connection it is proper to quote, on the other hand, the explanation suggested by Headland, in "Action of Medicine," p. 322, fourth American edition. Referring to the influence of over-stimulation of a gland to produce congestion, and a suspension of its proper function, he says: "Cantharides and turpentine increase the urine when taken in moderate doses, but when in over doses they diminish it, and may cause painful strangury, with an almost total suspension of the secretion. The explanation of such an action is obvious. Congestion is caused by the excessive action. In the same way we find that a large dose of mercury, naturally a chologogue, may produce jaundice, by causing congestion of the liver. This fact has been observed by Dr. Graves, of Dublin. But it does not follow from this, as argued by Dr. H. Jones, that mercury rightly administered does not increase the secretion of bile, and stimulate the hepatic function."

It is hardly necessary to quote the experiment of Dr. Beaumont, who gave his man twelve grains of calomel, causing "commotion," "slight nausea," and "the secretion of a white, frothy fluid, running from the aperture like fermenting beer from a bottle."

The generally received opinion of medical men with regard to the action of mercury, is expressed in the United
States Dispensatory: "In functional derangement of the digestive organs, mercurials in minute doses often exert a salutary operation, subverting the morbid action, and that, too, by their slow alterative effect, without affecting the mouth. In these cases no decided disturbance of the vital functions takes place, but the alvine discharges, if clay colored, are generally restored to their natural hue, a certain proof that the remedy is stimulating the liver, and promoting the secretion of bile. Indeed, there is no fact better established in medicine than the influence of the mercurial preparations over the hepatic system, and whether the liver be torpid and obstructed, as in jaundice, or pouring out a redundancy of morbid bile, as in melena, its judicious use seems equally efficacious in unloading the viscus, or restoring its secretion to a healthy state."

The question involved is, whether mercurial preparations may not aid in the production of stools having the color attributed to bile, but whether the effect comes through any influence upon the liver, or through the presence of bile.

A case in point is one which occurred under the observation of Dr. J. Hughes Bennett, of Edinburgh, of impaction of a gall stone in the common biliary duct. A deep jaundice existed, with dark urine, and the patient gradually declined, though brown stools were secured. Four days before death the stools were described as being green and dark. On the post mortem the common duct was found completely obstructed, and the biliary ducts greatly dilated.*

Dr. Inman states that he has seen many cases of suppression of bile, in which the feces maintained their natural color; and, on the other hand, cases in which the stools acquired a clay color without any hepatic derangement.

Dr. Ward (British Medical Journal, 1860) had himself a diarrhoea lasting six months, with clay colored stools, without any other evidence of bilious derangement.

Dr. Inman states that the green color of calomel stools is owing to the sub-sulphide of mercury, as the black color of the stools incident to taking preparations of iron is owing to sub-sulphide of iron, and that there is no evidence derived from chemical tests that calomel stools

*"Clinic Lectures on the Principles of Medicine," by J. Hughes Bennett.
contain any bile whatever, not even any of its coloring matter.

Dr. Inman maintains that fecification begins in the colon, and that it is in the colon that the brown color begins, and that the inference is that the color is the result of a secretion from the colon. A clayey diarrhoea in this light demonstrates that the colon and not the liver is sluggish.

Dr. Russell, of Birmingham, confirms the same theory of the color of the feces.

The fact is referred to, that a diet continuously of bread and milk produces a clayey color of the stools.

The inference from all this is, that the agency of the bile (if it has any) in the production of the color of the feces, is through some stimulation it produces upon the intestinal secretion, rather than through any material which it directly furnishes.

It has been ascertained in further elucidation of this subject, that the tests relied upon for detecting the coloring matter of the bile, secure the same reaction with the coloring matter of the blood, where this is in a state of disintegration, as in ecchymoses, presenting all shades of color successively from deep brown, through green and yellow, to the natural hue of the tissues.*

The inference from this is, that the coloring matter of bile is hematoidine, the disintegrating hematine which gives color to the blood. According to this theory, jaundice is the retention in the blood of effete coloring matter, which ought to be chiefly eliminated by the liver, to be finally decomposed in the intestine, so as no longer to be recognized by chemical tests.

If now it is found that calomel fails to secure a more abundant secretion of bile, it will no longer be given for the cure of jaundice, which, according to this theory, is for the most part a result of the failure of the liver to separate from the blood its coloring matter. It may be mentioned in passing, however, that the jaundice of yellow fever, and the icteric hue, resulting from bruises, and the general sallow color after great losses of blood, can hardly have any relation whatever to the function of the liver.

*See Prize Essay by Dr. S. Fleet Spier, in transactions of American Medical Association, for 1864.
With regard to the color of the feces, Mr. Inman proposes this question: "Did ever any one see the contents of the small intestine brown, deep yellow, or of a bilious color? Did ever any one see, in cases of hernia, a brown fecal matter flow from the bowel, if the small intestine only was implicated? It is not a fact that the intestinal fluid is always whitish prior to its reaching the ileo-cecal valve, and that it obtains its fecal character and color in the colon."

In view of these considerations, what must be the mode in which mercury favors a restoration of the brown color of the feces?

1st. It may produce a green color by its own combination with sulphur, which can be washed out and detected by chemical tests.

2d. It increases the amount and changes the character of the secretion of the intestinal glands, thus tending to correct the perverted functions of the passage.

It is now easy to see how careful observers may have been misled, to think that they procured a flow of bile by the administration of mercury. Thus, at the last meeting, Dr. Cassell stated that in the treatment of cholera in 1833, he was sure of the safety of the patients when, by the administration of calomel, he procured bilious stools. In this connection, Dr. Reed stated that a case occurred about the same time under his observation in which no mercury was given, but opium, capsicum, and camphor, without cathartics of any kind, and the patient recovered with this phenomenon. The first fecal evacuation, which was a consistent one, was clay colored in its lower half and brown in its upper half.

The inference from this fact is that the contents of the intestine remaining at the cessation of the cholera-flux retained its color, and that the material secreted from the intestinal glands afterward had the characteristic brown color, communicating the hue to whatever there might be of insoluble ingesta.

The fact, then, of the safety of the patient on the occurrence of bilious stools has a reversed explanation. The color comes in the stools, because the patient is beginning to get well. The stools thus contribute a most valuable aid in prognosis, and they will continue to be examined with as much interest as though the action of the liver were supposed to be the turning point in the progress of the case.
The changes which take place in the insoluble forms of mercury, as the metallic mercury rubbed into fine particles as in blue mass; oxide of mercury in ointment and calomel; to enable them to enter into the circulation, are not well understood; but the fact that they become dissolved is certain, if we admit the doctrine that only substances in solution can enter the circulation, for the presence of mercury is discovered in distant parts, to which it can only be carried by the blood.

Miahle maintained that mercury became soluble by being invariably converted into bichloride, so that this substance might, by proper management, be substituted for the other forms with the same results.

Headland claims that though this may be one of the ways that mercury gets into the system, it is probably not the usual mode.

According to this authority the bile acids form direct combinations with oxide of mercury, and the saliva and the mucous secretions of the intestinal canal take it up with promptness.

We are now prepared to appreciate the different results which may be obtained by varying the size of the dose of mercury, and the frequency of repetition.

Where corrosive sublimate is given the immediate local irritation which attends the contact of the agent, except in a very dilute form, renders the importance of minuteness of dose obvious enough.

Calomel, however, is dissolved slowly, so that a large dose may fail to secure any great degree of irritation, and the medicine may be in part expelled unaltered in the feces. There is always a risk, however, that a large dose may be retained until its complete solution and absorption may produce unwelcome results, either upon the mucous membrane or upon the general system, through its aplastic influence upon the blood or its determination to the salivary glands.

Headland remarks that "Very much smaller doses of calomel and blue pill than are usually given, will produce very much the same effects, because sufficient to exhaust the solvent power of the system. Thus, Dr. Law has shown that one twelfth of a grain of blue pill, given every hour for twenty-four hours, will produce salivation."

The efficacy of small and frequently repeated doses of calomel in cholera gets its most plausible explanation in this way: whether the medicine may be supposed to act...
first locally, or by being first absorbed and again determined upon the intestinal surfaces, as its way of getting out of the system.

I am myself indebted to Dr. Henry Jones, of this place, for a knowledge of the efficacy of small doses of calomel, from one fourth to half a grain, every ten, twenty, thirty, or sixty minutes in cholera infantum.

The grinding of calomel with crystallized sugar may be regarded as a preparation of some importance, as the medicine must be more readily dissolved by the chemical agents it meet with in fine powder than in coarse. The sugar also gives the medicine a taste agreeable to a child.

The purpose of this paper has been to stimulate inquiry and discussion rather than to settle any questions. For this purpose I have stated the grounds of the new doctrine with more fullness than those of the old, with which all are supposed to be familiar.

The new idea is not intended to revolutionize the employment of mercury, but to limit its employment to the uses established by observation, and to abolish the administration of the powerful agent in the cases in which it is inferred that the liver is too sluggish or too active, needing this regulator.

That an immense amount of harm has resulted from this theory of the control of the function of the liver by mercury is plain to every man of observation.

Comparative Merits of Incision and Dilatation of the Mouth of the Womb in cases of Dysmenorrhaea, etc. By D. Humphreys Storer, M.D. Read before the Boston Society for Medical Improvement, August 27th, 1866.

Every member of this Society must have often been impressed with the tendency which exists in the profession to be unduly influenced, I might, perhaps, with propriety, say overawed, by the opinion of those who have attained a commanding position in our ranks. This tendency I consider an exceedingly unfortunate one—it destroys self-reliance, individuality; it prevents the physician from faithfully performing his duty, inasmuch as he yields his dearly-bought and invaluable experience to the decided, oracular dicta of others. However much we should value and endeavor to profit by the instructions of
our fellow-laborers, we should never be willing to relinquish our own convictions, unless satisfied we are in error; until it is clearly shown that the course we have pursued, and are still pursuing, is erroneous. These thoughts have been suggested by the following circumstance. Since our last meeting a gentleman called upon me with his wife, who desired my professional advice. She had been an invalid for some length of time, complaining more particularly of dysmenorrhæa. I carefully examined her condition, and found she had a retroflexion of the uterus, the body of the organ being so completely bent upon the commencement of the neck as to cause almost a complete obstruction of the cervical canal—admitting the passage only of a very small metallic dilator. I told the husband what derangement existed, and the course which should be pursued to remove it; that I should advise the introduction of sponge-tents to produce dilatation, and, when this should be accomplished, the wearing of a stem pessary until the distortion should be permanently overcome. He at once told me that Dr. ——, who had seen his wife, stated that the plan I now suggested would formerly have been pursued—that it was not now, however, practiced by the profession, but that incision of the neck was the only approved method.

As the physician referred to has been a practitioner for quite a number of years, and consequently must have seen a greater or less number of cases similar to the one now spoken of, he evidently, in this instance, tacitly yielded his opinion to the weight of authority.

I think he must have read an article on "Dysmenorrhœa, Metrorrhagia, Ovaritis, and Sterility, depending upon a peculiar formation of the Cervix Uteri, and the Treatment by Dilatation or Division," which was published in the last volume of the Transactions of the Obstetrical Society of London, by Robert Barnes, M.D., President of the Society, and also that he must have coincided with the remark of Dr. Marion Simms, made at the meeting at which that communication was presented, "that that Society must be taken as the representative of professional opinion on any subject falling within its domain." Now, however willing we may be to admit the value of the transactions referred to, we are unwilling to allow the infallibility of any, even of the most distinguished of that Society. And even at the meeting referred to, it was evident that no little diversity of opinion existed between
Drs. Barnes, Baker, Brown, Greenhalgh, Routh, Savage, Hewit, Wyner, Williams, and Sims, as to the location of the obstruction in dysmenorrhœa, and the local treatment, whether by dilatation or division.

Thus Dr. Barnes considers the "seat of the obstruction," to use his own words, "almost invariably at the os externum. The obstruction is due chiefly to the small, round os itself; partly to the pointed, elongated form of the lower part of the vaginal portion, and partly to an unusual rigidity of structure of this part, which impedes the expanding action natural to the healthy formed os uteri."

Mr. Baker Brown, on the contrary, differed from Dr. Barnes as to the seat of the stricture; he believed it to be in the cervix itself, generally accompanied by narrowing, contortions, and reflexion of this canal—the results of inflammation.

Dr. Greenhalgh considered, from a long experience, that in a great majority of cases the stricture exists at the internal os uteri.

Dr. Routh coincided with Dr. Greenhalgh.

How utterly absurd to allow our judgments upon this point to be swayed by the opinions of either of the gentlemen above quoted, when the experience of every week assures us that the obstruction referred to may, and does exist at any point from the outer to the inner os uteri.

But especial reference I would make as to the manner of overcoming this obstruction, wherever it may exist.

Drs. Barnes, Baker Brown, Greenhalgh, and Simms strongly advocated the employment of the metrotome, or hysterotome; that a free incision be made; and Dr. Greenhalgh urged that the internal os should be dilated as well as the external os.

In other words, after the profession have for a series of years considered that, in the vast majority of cases, a contracted, an almost impervious os and cervix uteri may be dilated, and in many instances the suffering produced by this impediment removed by the employment of metallic dilators or sponge-tents, we are told by the President of the Society referred to, that "incision is now considered as not only justifiable, but as the only efficient and permanent remedy for dysmenorrhœa." Mr. Baker Brown, Drs. Greenhalgh, Routh, and Simms appear to have coincided with this view of the subject.
And why is this plan so strongly advocated? Dr. Barnes says: "Hemorrhage, pyæmia, cellulitis, peritonitis, have undoubtedly followed dilatation; and it is certain that in many cases, however good the dilatation effected by bougies or tents may appear at first, it is not of long duration. I suppose there is no dilatation by instruments more powerful than that effected by pregnancy and labor, yet after giving passage to a full-grown child, the peculiar cervix will sometimes completely resume its old vicious form."

Mr. Baker Brown agreed with Dr. Barnes, that "dilatation was an inefficient and only temporary remedy for dysmenorrhœa arising from the stricture of the canal."

Dr. Routh "had seen cellular abscess and death follow the use of sponge-tents."

We remember having seen, in some New York journal, a year or two since, similar remarks to have been made by Dr. Fordyce Barker and others respecting the employment of sponge-tents; that they had seen injurious results produced by their employment. It would be presumptuous in the extreme for me to doubt the statements of these gentlemen; I believe they stated the truth; I allow all they utter may occur. But is any known remedy always reliable? Is any known operation always successful? Is not an invalid sometimes made the sicker by the dose administered? the suffering one made permanently a sufferer by the surgeon's knife? May not some of the evils thus produced by sponge-tents be unnecessary? May not the time at which they are introduced, the size of the tent, the manner of its introduction, influence the effects produced? Not unfrequently, particularly in hospitals, this operation would be advised by the attending physician, but be performed by a less skillful hand, even by a nurse. Should there be an unusual excitement of the parts, such as frequently exists just preceding or following a menstrual period, it would of course be contra-indicated.

The size of the tent is of great importance. We can readily conceive that a large tent, which is capable of being dilated to a great extent, should cause much distress at the moment of introduction, and produce long continued and serious constitutional derangement. The operation itself may be improperly performed. If, instead of being carefully introduced, and the effects produced being watched, the dilator is carelessly, roughly, unfeelingly forced into the sensitive parts, suffering to a greater
or less extent must inevitably be produced. This is self-evident. From a somewhat extensive employment of sponge-tents during the past ten years for the treatment of dysmenorrhcea and sterility, I have formed conclusions different from those of the gentlemen of whom I have spoken. I have not unfrequently been disappointed in the result hoped for. The local obstruction has almost always been overcome by the long-continued, persevering employment of the dilator; but the opened canal does not always remove the condition thought to depend upon its closure—dysmenorrhcea and sterility still remain. I have, however, never seen the ill effects spoken of from the employment of tents. I can not recall a single instance where more than a few hours inconvenience have been produced; and in such cases the expanded sponge, when removed, has proved to have been originally much larger than was supposed to be—showing that he who employs these tents should be acquainted with their uncompressed dimensions. My experience has taught me, then, that these contractions, however firm they may be, may almost invariably be overcome. The physician need not feel that the part is undilatable because the application of three, or five, or half a dozen tents does not overcome it; in a case occurring in my practice about a year since, eighteen sponge-tents were introduced at intervals of two and three days before the canal was opened. My perseverance was rewarded by the perfect relief of the patient. I could point, were it necessary, to several cases where, after years of sterility, the sufferer has been relieved and borne children, and in the intervals in childbearing have suffered no dysmenorrhcea. I have repeatedly seen cases of dysmenorrhcea relieved for years, and known no return. In a word, I have relied upon dilatation to relieve these affections, and whatever opinions may be advanced by others, so long as I feel we have a remedy from which we can confidently expect relief, and very rarely observe any injurious effects, I shall feel it my duty to employ it. That cases do occur where the difficulty can not be removed by dilatation, there can be no question; but "that incision is the only efficient and permanent remedy (in most cases) for dysmenorrhcea," I unhesitatingly deny. Let us for a moment look at the method proposed. Those who advocate it should of course be satisfied that it has superior claims over the means now employed. I
have thought the ill effects produced by *distension* might be occasioned by want of care; but those arising from incision may follow the operation of the most skillful surgeon who advises it, when the metrotome cuts through the walls of the inner os; and Dr. Barnes states, to employ his own language, "there is no doubt that the surgeon has actually cut through the substance of the uterus, and wounded the plexus of vessels outside; hence, severe and dangerous hemorrhage has ensued, and the inflammation of the peritoneal tissues." And even supposing the operation should be successfully performed, it is acknowledged by Dr. Routh, one of its advocates, "that such an amount of contraction frequently exists as to render it necessary to have a dilating substance worn for a considerable length of time to prevent its perfect occlusion;" and Dr. Williams observes that "oftentimes no relief is afforded. He had seen a patient whose cervix uteri had been slit up on both sides, forming two large protruding lips, without affording any relief to the sufferer." Where the external os has been almost cartilaginous to the feel, I have overcome the obstruction with the hysterotome; but I have never attempted to divide the internal os. I can not, however, recall the instance where it was required.

Fortunately for those who object to unnecessarily experimenting upon the os and cervix uteri, there were those at the meeting when Dr. Barnes read his paper, whose opinions coincide with ours upon this subject. Thus, Dr. Savage, physician to the Samaritan Hospital for Women, who was in the habit of treating the severest cases of the character I have spoken of every week, assures us he never failed to remove the obstruction with the sponge-tent; and Dr. Graily Hewitt observed that where the cervix uteri was not hard and tense, he preferred to employ the tents as dilators. With these opinions Dr. Williams also coincided.

Enough has been said, I trust, to prove that the profession generally do not advocate the indiscriminate incision of the cervix uteri in cases of dysmenorrhea; that the physician should yield his scientific opinion only when convinced of its error; that carefully attested facts are of infinitely more value than the dogmatic teachings of the highest authority.—*Boston Medical and Surgical Journal.*
Monthly Period of Infecundity.

We have received from Dr. Avrard, a physician at Rochelle, an interesting little work, printed at Bordeaux, by Gounouilhou, entitled "Generation and Duration of Pregnancy in the Human Race." The object of this work is to determine with almost mathematical precision, "when fecundation is possible in woman, and to assign a limit of time in the menstrual cycle to the generative faculty."

The determination of this law forms the subject of the first part of the pamphlet before us; in a second part the author treats of pregnancy, and inquires into the possibility of recognizing its commencement, of determining its duration, and of assigning to its termination a physiological period.

The theory of M. Avrard, concerning the moment when fecundation takes place, is no other than that of M. Pouchet, verified, completed, and determined in its modes and phases. "Fecundation," says M. Pouchet, "presents a constant relation with menstruation; also, with the human race, it is easy to determine exactly the intermenstrual period when fecundation is physically impossible, and that when it can offer some probability." By observation he endeavors to gain a confirmation of this assertion; to establish upon a solid and scrupulously exact basis the duration of the intermenstrual period, during which fecundation can alone take place; and to fix, as well as possible, the limits of this period.

M. Avrard, after having learnedly related and discussed the facts which seem to him calculated to throw light on the question, arrived at the following conclusions:

1. The cycle of generative functions lasts twenty-eight days. It is divided into three periods of unequal length, which the author calls menstruous, generative, and hypnotic.

2. Menstruation returns normally every twenty-eight days, starting from the accession of the courses. Its duration is indefinite.

3. A certain time elapses, most frequently, and perhaps always, between the end of the courses and the beginning of the generative period; this time the author calls the interperiodic phase.

4. The generative period ends always the fourteenth day after the beginning of the courses.
5. It has been shown, by an observation of fifteen years, and resting to-day upon thousands of facts, with proof and counter-proof, that woman is physiologically barren during fourteen days in twenty-eight, that is to say, after the fourteenth day, commencing with the appearance of the courses, till the end of the following period.

M. Avrard does not admit, as does the Professor of Obstetrical Clinic, at Paris, the possibility of impregnation during the period of the courses.

In the second portion of his work the author maintains, contrary to the opinion of M. Mattei, that parturition, at natural term, coincides neither with the ninth or tenth catamenial period; but is effected always two hundred and seventy days after impregnation, whatever be the moment (often difficult to determine) of the generative period when the woman was impregnated. This normal limit can be exceeded, which is rare, or not be attained, which is common enough.

We regret our inability to analyse more at length this very attractive work of a distinguished observer, where are treated with so much taste and talent questions of the highest interest as regards midwifery, legal medicine, and hygiene, and also in a still more important respect. In short, far from considering the popularization of the physiological fact of which he treats as necessarily involving immoral results, a very learned theologian, to whom the author had submitted the question of temporary infecundity, has thought, on the contrary, that, man being free to use marriage, if not as he pleases, at least when he pleases, many men being prevented on prudential grounds from cohabitation, through fear of a too numerous progeny, will hereafter be able, thanks to the doctrine of temporary infecundity, to allow themselves, in all security, complete normal, and consequently lawful intercourse; without which, in the opinion of the moralists, economists, and physicians, domestic happiness can not exist. — Jour. de Med. et de Chir., and New Orleans Med. and Surg. Journal.
A New Remedial Agent in the Treatment of Insanity and other Diseases.

The following is an account of a remedy which after several experiments Dr. S. Newington asserts he has found most useful in the treatment of insanity. It is a remedy which appears to him to afford a powerful and valuable means of withdrawing the blood from any diseased organ to which there is an abnormal determination; and, at any rate, it is often most efficient in subduing the excitement of mania and in inducing sleep.

"It is not known," he says, "that during sleep the quantity of blood in the brain is less than during wakefulness, and that the active circulation of much blood through the brain is incompatible with healthy sleep. When the cerebral functions are disordered from excess of activity, mental anxiety, or other cause, there is a determination of blood to the brain, sleeplessness ensues, and the effect in its turn becomes the cause of further mischief. Maniacal patients have been frequently brought to me who have been for six or seven days without sleep, and when repeated doses of morphia and antimony have proved worse than useless. Indeed, the frequent disappointments from the administration of narcotic drugs during an experience of twenty-two years in the treatment of insanity, have led me to try various experiments for the purpose of obtaining some simpler and more certain method of calming excitement and producing sleep.

"While staying at Matlock Bath, I was induced to try the effects of being wrapped up in cloths steeped in mustard and water, and applied to the whole legs and to the lower part of the abdomen. After the removal of a wet towel which had been applied round the head and was very uncomfortable, I began to experience the most soothing effects, and gradually passed into a dreamy semi-conscious state, which lasted during the half hour I was under treatment. On getting up, I felt very lively and joyous, the liveliness lasting the whole day; and for nearly twenty-four hours there remained a pleasant tingling sensation in the legs, which were affected in no other way than by redness. It occurred to me at once that this kind of application might be very serviceable in certain cases of insanity, and immediately on my return home I
set about making experiments for the purpose of testing its value. The first experiment was upon myself.

On retiring to rest I ordered a large basin of linseed meal and mustard (ten parts of the former to one of the latter) to be made into a paste, and spread upon a sheet of brown paper sufficiently large to cover the whole abdomen, a piece of muslin being interposed to keep the skin clean. In a short time I fell asleep, and was conscious of nothing till eight in the morning, when I was partially roused by persons about me; but I was unable to speak or move. One of my medical assistants was thereupon sent for, and he pronounced me in a state of stupor from some narcotic. Though I was unable to speak, I heard the whole of the conversation, and was in a dreamy semi-conscious state. On the administration of some stimulant I presently recovered.

Another form in which I use the mustard is this: two handfuls of crude mustard are tied in a cloth and placed in hot water, then squeezed in the hand until the strength of the mustard has been extracted. A thick towel, long enough to reach round the loins, is then wrung out of this infusion, wrapped round the body, and covered with a large piece of macintosh. In one case a patient suffering from acute mania, who was restless, sleepless, and refused food, was thus treated with the greatest benefit. Before the application the pulse was one hundred and eight, but after two hours of this treatment it had fallen to sixty in the minute, and the patient was in a quiet, semi-conscious state. Afterward he took his food regularly, and in a short time left, perfectly recovered.

A third form in which this derivative treatment may be applied is as a mustard bath; in other words, an ordinary warm bath, into which have been thrown five or six handfuls of crude mustard. In some cases the deep hip bath only may be used; but in severe cases of mania the whole body of the patient, with the exception of the head, should be placed in the bath. A lady so treated, who had during the last year four attacks of violent mania, each lasting for five or six weeks, has now for twenty-two weeks had no further attack, although the symptoms usually forerunning the seizure have on several occasions occurred; the mustard bath appears to have warded off the recurrence of the excitement. In this case the bath was used once every twelve hours, for half an hour at a time, during a period of ten weeks; so that
the skin was kept in a constant state of redness. It may be hoped that the habit of diseased action has now been broken, and that this patient, after due probation, may be discharged as recovered.

"Mr. W—was brought to me in a strait-waistcoat, and as many as six people had been, it was said, necessary to control him before his arrival at Ticehurst. Notwithstanding repeated doses of opium, he had not slept for six days and nights; and through the night after his admission he was excited, restless, and talkative. On the following night he was placed in a mustard bath for half an hour, so that he was perfectly red on being taken out. During the next eight days he had six of these baths, and at the end of a fortnight after admission, he returned home on trial.

"A lady who, notwithstanding repeated doses of morphia, had not slept for seven days and nights, was admitted in a state of mania, extremely incoherent and excited. After being in the mustard bath for half an hour, she became calm and comparatively rational, and expressed herself as feeling much more comfortable. She was then wrapped up in a blanket and put to bed, where she soon fell into a sleep that lasted for seven hours; and in the morning she awoke free from excitement. The treatment was continued for six nights, and no further excitement occurred, although, as she had been insane for two years, her mind remained unsound.

"These instances, with others that I might quote, suffice to prove that in the proper use of these derivative measures, we have a valuable remedial agency in the treatment of insanity. As nature, aiming to restore the nervous element of the brain wasted by the day's labor, diminishes the activity of the circulation through it, and allows the process of repair to go quietly on, so we, imitating nature, strive in this treatment of insanity to withdraw the excess of blood from the disordered brain, and thus to favor the restoration of the natural equilibrium and the return of healthy function. And as when a morbid action continues for some time, a habit of it is apt to be formed, and the habit to become a 'second nature,' so, on the other hand, whenever the morbid activity is interrupted, the tendency to revert to its sound type, which exists in all organic elements, fails not to assert itself, and, if sufficient time be allowed, to restore the normal function. We perceive
then, how exceedingly important it is to produce natural sleep in the early stages of insanity.

"In using the mustard bath, it is necessary to protect the privates with a folded dry towel; and it is, of course, desirable to have the bath placed near the bed, so that the patient may pass directly from it into his bed. If a little constraint is required on the first occasion of its use, it will rarely be found necessary on any subsequent occasion."—Half-Yearly Abst., vol. xlii., from Lancet, June 10, 1865.

Amputation at the Knee.

Professor Syme bears strong testimony (Edinburgh Med. Jour., April, 1866) in favor of the advantages of Mr. Carden's method of performing amputation at the knee, which the Professor regards as one of the greatest improvements in modern surgical practice.

"When I began," says Professor S., "to amputate at the ankle, and found the great advantage of dividing the bone through its cancellated texture, it naturally occurred that the same consideration was applicable to the knee, and that, when circumstances permitted, amputation should be performed here rather than through the thigh, with its dense shaft and medullary texture. But, unfortunately, not being then aware of Mr. Carden's plan, I formed a covering for the bone by cutting it from the calf of the leg, which proved very inconvenient, and so counterbalanced the benefit anticipated, that this operation soon fell into disuse. Mr. Carden, pursuing quite an opposite course, made a semi-lunar incision in front, from side to side, with its convexity nearly over the tuberosity of the tibia, and reflected the flap of skin thus formed, so as to expose the muscles above the patella, where what remained of the limb was divided transversely. The popliteal artery, and any of the small branches that required ligature having been tied, the ample covering of integument was brought down to its place, where, being secured by sutures, it lay without any tendency to retraction, or requiring the restraint of bandages, while the dependent opening afforded a free vent for the discharge of matter. No trouble was experienced in the after-treatment, and the stump proved entirely serviceable, since
the skin over the bone, instead of becoming thinner, acquired additional thickness, so that the patients could rest upon it just as they do after amputation at the ankle.

"But the advantages of this operation are not limited to its facility and satisfactory results in the event of recovery, since its great claim to respect and confidence is the safety that attends its performance."

On the 19th of September last Professor S. saw, with Mr. Annandale, a patient in the hospital "who had been admitted with both of his legs completely shattered by a large mass of iron falling upon them. It was obvious that he must die if the limbs were retained, and no less so that amputation of both thighs would in all probability prove fatal. I therefore suggested that Mr. Carden's operation might be performed, which was accordingly done by Mr. Annandale with the most satisfactory result."

Soon after this (October 23) Professor S. saw, with Dr. Mackenzie, of Kelso, "a young farmer whose life was in great danger. It appeared that while on horseback, during the race week, he had been struck by the shaft of a cart in the crowded street, with such violence as to cause a fracture of his leg. There was no wound, but the limb suddenly swelled and became cold, with dark discoloration. Inflammatory symptoms succeeded, with corresponding constitutional disturbance, and on the fourth day it was generally supposed that the case must prove fatal from spreading gangrene. But Dr. Mackenzie thought that amputation might still afford a chance of escape; and although the prostration was extreme, with a pulse hardly to be felt, so that cutting through the thigh must have been almost fatal, I proposed to operate at the knee, and did so without delay, when it appeared that the posterior tibial artery had been ruptured at the seat of injury. The patient was no sooner relieved from the mortified limb than he began to improve, and, through careful nursing, made a good recovery with an excellent stump.

"On the 4th of November, Dr. Hislop, of North Berwick, requested me to see a clergyman who had been confined to bed for more than twelve months, by disease of the knee-joint, with no prospect of improvement, and constantly increasing weakness. It seemed that skin and bone naturally predominated in the constitution of his frame, and that from long-continued exhaustion little else of it remained. I should, therefore, have regarded am-
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Amputation of the thigh as a most unpromising procedure, but with my recently acquired faith in the knee operation, felt no hesitation in performing it. Everything went on favorably afterward, and the reverend gentleman speedily regained his health, with a comfortable stump.

"On the 1st of January, W. M., aged thirty-two, was admitted into the hospital on account of a compound fracture which his leg had sustained on the railway. An attempt was made to save the limb, apparently for a time with some prospect of success; but on the 9th, from the amount of discharge and the extent of shattering which was revealed by the ulceration and sloughing that had taken place, it became obvious that amputation afforded the only chance of escape. I therefore operated at the knee; and although the circumstances were most unfavorable for recovery, I had the pleasure of seeing the patient gradually improve in health, with the prospect of a good stump.

"A boy, aged six, was admitted into hospital on the 20th of January, with mortification of the foot, from a railway injury, and threatening of gangrene extending up the leg. I amputated at the ankle, in the hope of arresting the mischief in progress, but with only partial success, since inflammation affected the periosteum, so as to cause necrosis of the whole tibia, and establish profusely discharging sinuses above as well as below the knee. The patient then became so extremely weak that his case appeared to be hopeless, and would, I believe, have proved to be so, had we not possessed a milder alternative than amputation of the thigh. I removed the limb at the knee on the 14th of this month, and the operation was followed, instead of sinking, by such an improvement of condition as encourages us to look for the most satisfactory result.

"Soon after his double amputation at the knee, Mr. Annandale had a patient, in private, suffering from disease of the knee-joint, who was so exhausted by hectic bed sores, and profuse discharge, that amputation of the thigh seemed to afford no prospect of recovery. He, therefore, amputated at the knee on the 25th of October, with the almost unlooked for result of complete recovery.

"Dr. Joseph Bell, who takes charge of the surgical clinical wards in my absence, admitted a patient on the 26th of January, who had suffered a compound fracture of both legs on the railway then in progress of construction at Queensferry, whence he had been brought all the
ten miles in a cart. One limb was shattered beyond the possibility of recovery, the other being less seriously injured. The former was amputated at the knee, and the latter so successfully treated that the man is now able to walk on it with the assistance of crutches.

"From what has been said, I trust it will appear—

1st. That Mr. Carden's operation is less dangerous to life than amputation of the thigh.

2d. That the execution, ligature of vessels, and after-treatment, are simple and easy.

3d. That the resulting stump is comfortable and serviceable.

"These considerations will, I trust, meet with due attention, and tend to promote the adoption of a procedure destined, I feel assured, to supercede amputation of the thigh, which, notwithstanding all the attempts to prove it, has so long remained an opprobrium of surgery."

On the Trichina and Trichinosis. By M. DELPECH. (Annales d'Hygiène Publique, Julliet, 1866.)

In an elaborate report on various papers on trichinosis, communicated to the Academy of Medicine, Paris, and from a review of the whole subject, M. Delpech arrives at the following conclusions:

"Although the symptoms and gravity of trichinosis had been fully known only since the year 1860, still the disease was by no means a recent one, and its existence in Germany at a remote period, in an epidemic form, could be readily demonstrated.

"It was then confounded with various other affections, and was more especially looked upon as a peculiar and exceptional variety of typhoid.

"The disease has since given rise to much arduous research, and can scarcely in future escape detection, when it has been attentively watched in every stage of its development.

"Disturbance of the digestive organs followed by edema of the face, and subsequently by severe muscular pain, and by a degree of dyspnoea which may even end in asphyxia on account of the impossibility of the movements of respiration, is an aggregate of symptoms not to be met with in any other affection. These morbid manifestations correspond with the successive birth in the
digestive tube, and of the passage into the muscular structures of trichinae in numbers sometimes enormous, but in general proportionate to the quantity of parasites which have been swallowed. Their presence can be demonstrated during life by the microscopic inspection of a minute particle of muscle removed from the patient's person with peculiar instruments, and by an innocuous and almost painless operation. In doubtful cases, the diagnosis can; therefore, at a certain stage of the disease, be confirmed by direct inspection.

"In general, one tainted animal will infect many persons. Hence more or less widely-spread and severe epidemics, according to the condition of the animals, the variable quantity of the flesh consumed, and the mode of cooking adopted.

"Certain animals are, as well as man, liable to trichinosis. In carnivora and omnivora the complaint occurs spontaneously, and herbivora may also artificially become affected, but only by the intervention of the human subject.

"In man the disease arises from the consumption of raw or insufficiently cooked pork flesh, tainted by the presence of trichinae.

"In pigs the propagation of the parasites is referable to several causes. They eat trichinized animals, especially rats, dead or alive, or abandoned on dunghills or in fields. They feed on human excrement, or on the dejecta of pigs which have recently consumed trichinized flesh, and which excrete, with the contents of their intestines, fecundated female trichinae. Moles, earthworms, the larvae of flesh-flies, the beetroot worm, have nothing to do with the transmission of trichine.

"When the disease occurs spontaneously in pigs, it seldom gives rise to characteristic symptoms, and microscopic inspection alone leads to the knowledge of the parasites. In the human subject, the cyst, when encrusted with calcareous salts, can easily be discerned with the naked eye, in the shape of white patches, and the microscope affords further conclusive evidence. In the countries where trichinosis prevails, this mode of examination has become a general precaution, whether carried out by individuals or by order of the Government.

"Merely optional microscopic examination, although doubtless useful, can give no absolute security, on account of the necessary absence of regularity and supervision.
Compulsory examination alone can yield any seriously beneficial results. Two objections are urged against it, viz.: the difficulty of carrying it out, and the uncertainty of the information supplied in cases in which the animals are but slightly affected. These are, it is true, serious considerations, but nevertheless the advantages derivable from compulsory microscopic inspection are such that the measure should unhesitatingly be adopted in all countries contaminated by trichinosis.

"France appears hitherto to have escaped the contagion, and no cases have yet been adduced of acute or encysted trichinosis, nor have any records been brought forward of former epidemics, as in Germany. The rats of the slaughter-houses do not seem to have been infected; at least, not habitually. The immunity is to be traced to the different customs of both countries, and to the more complete boiling to which the meat is submitted in France, which checks the development and propagation of the parasites.

"A temperature of 75° Cent. (167° Fahr.) alone can secure the destruction of the trichinae. The same result may be attained by thorough and protracted salting, or by a hot fumigation of twenty-four hours' duration. Cold smoking does not destroy the worms."

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The general conclusions which Dr. Compton has come to have been arrived at after a careful study of some one hundred and twenty-five cases taken by himself during the last two years, at St. Bartholomew's Hospital, and also of some seventy-five other cases taken in the same hospital, during the same period, by Dr. Warter.

The total number of cases in which the temperature and general symptoms have been watched and recorded daily throughout their course, amounts to two hundred, of which sixty are typhus, thirty typhoid, twenty pneumonia, fifteen scarlet fever, and the remaining seventy-five comprise cases of febricula, acute rheumatism, erysipelas, cholera, acute tuberculosis, etc. The total number of observations in these cases, and in others in which only
one temperature has been recorded by Dr. Warter or Dr. Compton, probably exceeds five thousand. Dr. Compton states what he considers to be approximately the average normal temperature of an axilla in a healthy adult. A temperature of 98°·4 Fah. is the point generally settled upon by the majority of authorities on the subject; but this Dr. Compton believes to be too high, as although he has not at present taken a sufficiently large number to decide the question to his own satisfaction, yet he can state that he has very rarely found such a temperature present in a healthy adult under normal conditions. "I have," he says, "every reason to think such a temperature to be nearly up to the maximum, consistent with health, and to be only met with occasionally, just as one comes across, now and then, a healthy adult with a temperature below 96° Fah. I consider the healthy range to be somewhere between 95°·5 and 98°·5 Fah., the most common temperature met with, being probably 96°·4 Fah., i.e., one degree less than the temperature hitherto most generally received as the normal one."

Dr. Compton seeks to establish the following propositions from his observations:

1st. That a continued daily temperature of 99° Fah., and upward, indicates an unhealthy condition, and occurs in every case of acute disease.

2d. That any one observation of a very high temperature (such as 105° Fah.), in any case in which the general symptoms do not appear of any particular severity, should lead to a very attentive reexamination, and suggest a very careful watching, especially if occurring in a non-diagnosed case; such a temperature being present only in severe forms of any disease.

3d. That the thermometer is of great use, as a means of diagnosis in those cases which frequently present themselves, of general malaise, often accompanied by a history of rigors, loss of sleep, etc.; such symptoms being due either to the commencement of one of the specific fevers, or merely to some gastric or uterine disturbance of a temporary character.

4th. That the temperature in every disease has a tendency to run a peculiar course, and has a certain range of altitude, a knowledge of which course and range is of great value as an assistance to us in diagnosis and prognosis.

5th. From the last proposition it follows, that the same
altitude of the thermometer attained at one period of any disease is not of the same importance as the same height reached at another time in the same disease.

Thus, in typhoid fever, a temperature which has been rising for two or three days, reaches perhaps 104° Fah. between the seventh and fourteenth days, without causing any anxiety; whereas, should the same phenomenon occur about the twenty-eighth day, a fatal termination may probably be expected.

And again, the actual altitude attained on a certain day in one disease is not of the same importance to our prognosis as the same height reached on the same day in another disease. Thus, a temperature of 104° Fah. in erysipelas is very common during the first week, and need not give rise to any alarm; but should such occur at the same date in acute rheumatism, Dr. Compton would consider it of much more importance.

6th. That although, in all diseases, a high range of temperature generally indicates a severe case, with a slow convalescence, and a low range usually occurs in a mild case, and is followed by a rapid convalescence; yet there is no actual temperature in any disease which necessarily foretells a fatal termination.

7th. That in the majority of cases a rise of temperature is contemporary with a rise of pulse, although such is often not a proportional one, and may not take place at all unless the alteration in temperature by as much as 1°F or 2°F Fah.

8th. That where the temperature and pulse together do not coincide with the general symptoms, the two former may be generally relied on as to the actual state.

9th. That where the temperature and general symptoms agree together, but do not coincide with the state of the pulse, the two former may generally be relied on as to the actual state.

10th. That in those cases in which the pulse and general symptoms remain the same, a moderate fall of temperature on one occasion is not to be relied on; but should such a fall continue in a moderate and gradual manner, for some days, and at such a period when a fall was to have been expected, the temperature may then be depended upon. Severe cases of typhus, toward their close, often give examples of this sort.

11th. That in those cases in which the pulse and general symptoms continue the same, being the one frequent
and the other severe, a continuous rise of temperature for some days, occurring at a period of disease at which some improvement might generally be expected, is usually the precursor of a fatal termination.

12th. That although it is possible that the state of the temperature alone in acute disease may, perhaps, hereafter prove to be the one safest symptom to rely upon if taken by itself (and I believe it is at present, at least, equal to the state of the pulse, and of greater value than this certainly, if only its frequency be taken into account), yet the temperature must be considered merely as an aid, and all other symptoms must be carefully examined into, as it is on comparison with these that its greatest value is always to be found.—Dublin Quarterly Journal of Medicine, August, 1866.—Ranking's Abst.

On the Treatment of Scabies by Oil of Petroleum. By Dr. Decaisne.

From a report published in the Archives Médicales Belges, we learn that Dr. Decaisne has used the oil of petroleum successfully in upwards of six hundred cases of scabies. In the great majority of the subjects the disease was completely cured after a single friction, in several after two, and in a very few instances were three or four inunctions required. The method failed in two or three cases only, and sulphuret of lime was necessary to effect a cure.

It has been objected that oil of petroleum is an irritant and produces rashes, but M. Decaisne remarks that the remedy applied with proper precautions seldom causes this unpleasant result.

"At first the frictions were performed with rough towels and brushes, and probably, in order to lacerate the sulci, the oil was rubbed violently into every part of the skin more particularly affected. The inevitable result was the exposure of the dermis, and rashes consequent on the mechanical irritation. Military surgeons have, however, found from experience that this is unnecessary, and now the inunctions are more gently performed. But even this plan was open to improvement. It may be a matter of indifference when the skin is healthy to use a brush, a rough sponge, or a hard towel, but in the case of scabies the vesicles are often broken, and the cuticle destroyed,
and the softest aquarelle brushes should be used to spread
the oil on the integument.

"Since brushes of this description have been used in
barracks, the secondary eruptions have all but ceased, and
when any have appeared they were the result of an error
of diagnosis which can not always be easily avoided in
case of some standing. Prurigo, eczema, impetigo, are
often mistaken for scabies, and in these affections the evil
effects of repeated and inopportune frictions are readily
accounted for."

M. Decaisne also adverts in his report to the disinfection
of the clothing. Experiments instituted in the military hospital and garrison at Antwerp have shown the
utter inutility of the measures in habitual use. Since they
have been discontinued, relapses have become less fre-
quent, and the inutility of disinfection is, therefore, now
fully demonstrated, and this expensive procedure, founded
on routine and not on scientific experience, should hence-
forth be abandoned. If it be even conceded for the sake
of argument that the acarus can continue to live elsewhere
than in its natural habitat, the operation would still be un-
necessary, because in resuming his wearing apparel the
patient exposes to the action of the petroleum with which
his person is saturated, the few sarcoptes which may re-
main in his clothes.

The treatment with petroleum oil thus combines with
its great efficacy the additional advantage of economy,
because the process of disinfection is dispensed with, and
the entire cost of the medication does not exceed for each
case three or four centimes.—Jour. of Practical Medicine
and Surgery, Jan., 1866.—Ibid.

On Morbid Conditions and Injuries of the Spleen in the Preg-
nant and Parturient States. By Sir James Simpson,
Bart.

In a paper read before the Obstetrical Society of Edin-
burgh, Sir J. Y. Simpson referred to three cases of fatal
rupture of the spleen, which had occurred respectively in
the pregnant, parturient, and puerperal states. He pointed
out the circumstance that, during pregnancy, there is
often, if not generally, an increase of the white particles
of the blood, or, in other words, a kind of normal or
physiological leucocythemia. As in states of morbid leucocythemia, the spleen was often enlarged; so was it also occasionally in pregnancy. Perhaps it would be found in practice much more common than the silence of authors on the subject might lead medical men to suppose. It sometimes recurred in successive pregnancies. In one patient of his, the spleen became enlarged to a very marked degree in a series of successive pregnancies, and this splenic enlargement disappeared always after delivery. Her youngest child is now about ten years old, and during that time there has been no recurrence of the splenic hypertrophy in the mother. A certain amount of softening very frequently accompanies the hypertrophy of the spleen, and predisposes to the laceration of the organ under strong exertion and muscular effort, blows, etc. The first case of rupture of the spleen in a child-bearing mother which he saw was a patient of Dr. Husban's. She began to show symptoms of fatal sinking shortly after premature labor set in, about the sixth or seventh month. On opening the body after death, the enlarged spleen was found lacerated, with effusion of blood into the peritoneal cavity. Shortly afterward a patient of Dr. Wilson's who had been delivered a week or two before, after making some unusual muscular exertion, complained of abdominal pain and sinking, and died. Rupture of the spleen and effusion of blood were found on dissection. The late Dr. Cunningham, of Currie, delivered a patient in Edinburgh, using the forceps. He left very shortly afterward to catch the railway train. The patient sank and died within an hour or two. An inspection of the body was ordered by the law authorities, when rupture of the spleen, and consequent effusion of blood, were found to be the immediate cause of death.—*Edinburgh Medical Journal, Sept., 1866.*—*Ibid.*

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**On the Therapeutic Uses of Oxygen. By M. Demarquay.**

M. Demarquay, who has devoted much attention to the use of oxygen inhalation in medicine, says, in speaking of its therapeutic indications, that, in the early stage of phthisis, when there is no fever, and no fear of exciting local action, when the patient is becoming emaciated, and the emaciation is increased by persistent dyspepsia, oxygen may have a salutary influence in modifying the state
of the constitution and sustaining the organism. Asthenia is the disease in which oxygen has been given by preference; of twenty-two patients treated by Beddoes, ten were cured, and nine relieved. But the employment of oxygen in asthenia meets with numerous contra-indications. Oxygen renders incontestable service in essential anæmia. It is specially indicated in that form of chlorosis of young girls which is characterized by obstinate anorexia; in the anæmia of convalescents, and in the anæmia, often severe, of newly delivered females. The inhalation of oxygen is also successful in anæmia arising from hæmorrhage or from fatigue, and is also a very energetic remedy in the debility produced by prolonged suppuration; it stimulates the appetite, sustains the powers of the patient, and enables him to attain to recovery. In diabetes, under the influence of oxygen inhalation, the quantity of sugar contained in the urine is remarkably diminished. In surgery, oxygen stimulates weak and ill-conditioned ulcers, and accelerates the production of granulations in cicatrizing wounds. In senile gangrene, as long as the circulation continues in the artery of the foot, oxygen is, according to the observations of MM. Laugier, Demarquay, and Maurice Raynaud, the only remedy which in advanced cases affords a chance of recovery.—Gazette Médicale de Paris, 13 Avril, 1866.—Ibid.


Peritoneal friction-sounds, Dr. Seidel states as the result of his observations, have been noted on a level with nearly all the abdominal organs. Their signification is very variable. They indicate the existence either of a chronic or an acute malady, but more rarely the latter. It is not necessary for the production of a friction-sound that the peritoneum should be supported, as it were, by a solid part. In the majority of the cases the friction-sound was somewhat rhythmical, under the influence of respiratory movements, which were perceptible even in the hypogastrium. The perihepatic friction-sound, particularly when occurring over the convex surface of the organ, might easily be confounded with a pleuritic friction-sound. To distinguish the one from the other, let the patient make a forced inspiratory movement, the glottis being closed, a
movement similar to those which accompany vomiting. In this movement the inferior border of the lung is not sensibly displaced whilst the liver is notably elevated. If the friction-sound remains under these circumstances, it is almost certain that it is of peritoneal, not pleural origin. In no case has a non-rhythmical sound, arising from the peristaltic movement of the intestines, been noted. Peritoneal friction-sounds are observed of every grade, from an extremely slight rustling to a veritable rasp.—Schmidt's Jahrbücher, 1866, No. 4; Archives Générales de Mèdecine, Juin, 1866.—Ibid.

EDITORIAL AND MISCELLANEOUS.

INFANTILE FEVERS.

It has long been a rule with me, and I have endeavored to impress the precept upon my friends and patrons, never to allow a child to have a second paroxysm of fever, if it be possible to prevent it. Children in this country are subject to the same class of fevers as we find affecting adults. That is to say, they very often have intermittent and remittent fevers, which can be arrested after the first paroxysm by the use of sulphate of quinine, and which, if not thus timely treated, may prove fatal, by the supervision of convulsions, coma, or other unmanageable complications. If a child or an adult have a paroxysmal fever, he will most probably have a repetition of it a little earlier on the following day, and sometimes one day later; and although the first paroxysm may be comparatively light, the next may be very serious. A sufficient quantity of quinine administered some hours before the expected return of fever, will make the child safe. It is true that children not unfrequently have a paroxysm of ephemeral
fever which would have no sequence even if left to itself. But there is no way in which we can distinguish these fevers from those that will recur. It is therefore always better to use the quinine, which may save the child's life if the attack be of a malignant character, and which can do no harm whatever if it be ephemeral. One grain of the sulphate of quinine for each year of the child's age will be found usually quite enough to prevent the return of fever, and it may be given in divided doses or all at once, care being taken to have the system under its influence at the hour of the apprehended attack. Many persons in as well as out of the profession give quinine in much larger quantities; but I feel satisfied that while the proportion above recommended is enough to obtain the desired effect, larger quantities are very trying to children. They certainly do not tolerate this remedy as well as adults do. It affects their head unpleasantly and makes them very nervous and fretful. It may not be amiss to state that many children will reject quinine as often as it may be given. In such cases it should be administered per rectum; the same quantity being used and given, all at once, in a tea-spoonful or two of thin starch or mucilage.

L. A. D.

A Manual of Medical Jurisprudence. By Alfred Swaine Taylor, M.D., F.R.S., Professor of Medical Jurisprudence and Chemistry in Guy's Hospital, etc.; with notes and references to American Decisions, by Clement B. Penrose, of the Philadelphia Bar.

This is the eighth English and sixth American edition, and a glance at the table of contents shows that the perfection of the work is only limited by the imperfections of the science. The arrangement of subjects in the present volume is a decided improvement upon former
editions for, to make it a "convenient guide to medico-legal practice" by the unskilled, it is eminently proper to devote the opening chapters to instructions as to their duties and responsibilities, and the nature of medical evidence. Besides this change new subjects have been introduced, with amplifying notes by the American editor, thus making it one of the most valuable text-books of the science known to the profession. Of these, the most important are trichiniasis and life-insurance: the former is a new and fatal malady connected most prominently with the ingestion of poisonous animal food, and of such peculiar interest to the profession at present, that we will presume to offer the following extract touching its history and medico-legal relations:

"From these researches," says the author, "it is now clearly established that the trichina is a viviparous parasite, which passes the greater part of its existence in the chrysalis state in the muscular system, until, by the consumption of this muscle as a food, it finds in the stomach and intestines of another warm-blooded animal a favorable medium for its full development into an intestinal worm. According to Virchow and Henker, the trichina not only frequently presents itself in the human organism, but this organism is most favorable to its development. The period of incubation of the chrysalis in the stomach and bowels of man or of warm-blooded animals, is from six to eight days; and during this time it there thrives and propagates to an almost incredible extent. Dr. Keller states that in three or four days the females produce one hundred or more young ones, which begin on the sixth day to leave the parent animal; and he estimates that in a few days after the ingestion of half a pound of meat, the stomach and intestinal canal of a person may contain thirty millions of these minute worms. M. Herbst found the muscles of two dogs which had been fed upon parts of a badger containing worms, to be loaded with these parasites. When once introduced into the stomach and intestines, the worms leave their capsules, become free, and produce young, which migrate through the walls of the intestines into the muscles; there they become encysted, and are ultimately found appropriating and destroying the muscular substance to a greater or less extent. The sudden liberation of a large number of these worms causes irritation and inflammation in the bowels, attended by peculiar symptoms, resembling in some respects those of poisoning.
It is worthy of note that trichina are more frequently found in pork and articles of food derived from it, than in any other kind of meat, measly pork appears to be something of a trichinous character. Further, the vitality of the parasites is not destroyed unless the meat or other substances in which they are located, has been subjected to a temperature equal to that of boiling water for a sufficient time to insure that every particle has been exposed to this degree of heat. Salting and smoking, or partial cooking, is not sufficient to destroy the worms in all parts of the food, and they have even been found living in putrefied meat. This may serve to account in some cases for the serious symptoms which have followed the use of pork as food, also of bacon, sausages, and German sausages, which are generally made of raw ham.

The symptoms produced by the use of such food are, in the first stage, those of intestinal irritation, loss of appetite, sickness, pain, general weakness of the limbs, with diarrhoea, swelling of the eyelids and of the joints, profuse clammy perspiration, and a wasting fever, sometimes of a typhoid character. Death is either the result of paralysis (from destruction of the muscular fibres) or of peritonitis and irritative fever. During the perforation of the coats of the intestines by these worms, the mucous membrane becomes irritated and inflamed; pus is formed on its surface, and bloody evacuations are sometimes passed. No case is known in which trichiniasis, after having once declared itself, was arrested by medical treatment."

In the medico-legal bearings of the subject, the author continues:

It is probable that some unexplained cases of death from irritation of the stomach and bowels, simulating chronic irritant poisoning, may have been the result of eating food containing trichina spiralis. Medical men have been unable to group the symptoms under any known form of disease, while the marks of irritation and inflammation in the mucous membrane of the bowels have given strength to the supposition that poison must have been taken by the deceased, although chemical analysis had failed to show the presence of any ordinary poison in the fluids and solids of the body. In the course of many years’ practice, I have met with several cases of this description, and there has been sometimes manifested a disposition to doubt the accuracy of chemical analysis. Dr. Lücke has related a series of fatal cases which occurred in 1845, attributed at the time to poison, which, as he suggests, were most probably caused by the use of trichinous food. (Casper's Vierteljahrschrift, January, 1854, p. 102.)

As means of distinction from irritant poisoning may be pointed out the long time which commonly elapses between the taking of the food
and the commencement of the symptoms. The pain, vomiting, and purging are comparatively slight; the pain is in the bowels rather than in the stomach, and peritonitis, pneumonia, and fever are not commonly the results of the action of irritant poisons, while they appear to be constant symptoms in trichinosis. The absence of ordinary poison in the food, in the urine, and the evacuations, at any stage, may also be taken as conclusive evidence against irritant poisoning in its usual form.

In suspected cases, a new method of research must be added to those already in use. If any of the food can be obtained, this must be examined for the parasite by the aid of the microscope. If the case prove fatal, the voluntary muscles of the deceased must undergo a similar examination.

The American editor appends at this point the following note, which will serve to awaken a lively personal interest with our readers. It is evident that our only safeguard against its epidemic prevalence in this country is, as has been suggested, in the system of thorough cooking to which our food is subjected:

A committee was appointed some time since by the Chicago Academy of Sciences to ascertain, first, whether trichina actually exist in the hogs of this country, and in those of the Northwest in particular; and, secondly, should they exist, to determine the extent of the danger thereby incurred, and to ascertain the best means of averting it. For the attainment of the first object, portions of muscles from one thousand three hundred and ninety-four hogs in the different packing houses and butcher-shops of Chicago have been examined, and the results presented in tables.

These tables show that the committee have found trichina in the muscles of twenty-eight hogs out of one thousand three hundred and ninety-four examined. We may, therefore, conclude, that in the hogs brought to Chicago, one in fifty is affected with trichinosis in a greater or less degree. We must confess our surprise at arriving at this result, which indicates, with little doubt, the startling fact that trichinosis in pork is even more common in this country than in Germany, where it caused so much suffering and death. For instance, in the city of Brunswick, where a most careful inspection of nineteen thousand seven hundred and forty-seven hogs was made in the years 1864-5, only two were found to contain trichina in their muscles, the proportion being one to ten thousand against one to fifty as before stated, in our country. The comparative immunity from disease which our own people have enjoyed, undoubtedly results from our habit of cooking
meat before eating it, while in Germany it is eaten raw by the poorer classes, on account of the high price of fuel. The committee have found the spinal muscles more liable to be infested with the trichina than other muscles. (p. 163.)

The closing chapter of the volume upon the subject of life-insurance is very instructive and interesting: we can not notice it at length, but simply direct attention to it, as a novel subject, of great importance to all classes of the community.

W. H. D.

INFLUENCE OF MARRIAGE ON LONGEVITY.

The Boston Medical and Surgical Journal of March 7, 1867, contains an interesting article on "The Influence of Marriage on Longevity," by James Stark, M.D., of Edinburgh, of which the following is an epitome:

What is the effect of marriage on male and female life? Is its influence, in so far as the death-rate is concerned, favorable or not? Is its influence limited to the female, or has it, also, a marked influence on the duration of life in the male? These are important questions. Table II includes married and unmarried men of each quinquennial period of life in Scotland in 1863, deaths at the same ages, and the percentage of deaths to the living at each age, and shows results quite startling as to the immense difference between the mortality of the married and unmarried.

It appears that from every age, from twenty to eighty-five, the death-rate of married men is much smaller than that of the unmarried. Reading the tables without decimals, so as to make them more intelligible, of every hundred thousand bachelors between the ages of twenty and twenty-five years, one thousand one hundred and seventy-four died annually; but of the married men, only five hundred and ninety-seven, or just one half. As the age
increases, the death-rate decreases, but slowly and regularly. Thus, at the ages from twenty-five to thirty years, when the number of bachelors and married men in Scotland is pretty nearly equal, of every hundred thousand bachelors one thousand three hundred and sixty-nine died annually, but of the married only eight hundred and sixty-five. Without going in detail over every separate age, but to mention one more—even at the extreme ages of eighty and eighty-five years, while nineteen thousand six hundred and eighty-eight of the bachelors died, only seventeen thousand four hundred married men did so. These facts are rendered still more striking if we calculate the mean age at death of the married and the unmarried men. The result is, reckoning from the twenty-fifth year to the close of life, that of the married was 60'2-10 years, while that of the bachelors was only 47 7-10, giving twelve and a half years in favor of the married.

This is a remarkable fact, and apparently a special provision of nature to protect the father of a family, in order that he may provide for his offspring and super-intend their rearing. It is quite true that this special protection from death is based on fixed laws of nature, by which we see that the generally quiet and regular life of the married man secures him from falling a victim to diseases, to which the more irregular, and often more dissipated life of the bachelor renders him prone. To Insurance Companies statistics like these are invaluable, because they point out to them an unsuspected source of danger, whose influence for evil is as great as vicious habits, or the existence of organic diseases, or descent from a scrofulous or consumptive family.

Similar tables, referring to women, are given, showing valuable information, embracing the two consecutive years of 1861 and 1862. When the mean annual percentage of deaths in the married and unmarried female at each quin-
quennial period of life is compared, it is noted that the married die in a higher ratio during the three periods of fifteen to twenty, twenty to twenty-five, and twenty-five to thirty years; but, that during the next two periods, viz.: thirty to thirty-five, and thirty-five to forty years, during which half the children are born, the married die at a lower rate than the unmarried. At the age when the usual "change of life" occurs, viz.: between forty and forty-five years of age, the mortality of the married slightly exceeds that of the unmarried woman—a result which might have been expected, seeing that the fatigues of child-bearing, and nursing, and the harder labor connected with the rearing of her family, somewhat weakens the system. From forty-five to seventy-five years of age, the married die in smaller proportion than the unmarried woman.

It will be seen that at every quinquennial period of life, the difference between the death-rates of the married and unmarried women is very much less than that between the married and unmarried men. It is thus demonstrated, for the first time, that marriage exerts a much more powerful influence on the male than the female; for, whereas, the influence of marriage on the female death-rate is comparatively trifling, it is the most marked and potent on that of the male. The common belief has always been the reverse of this—it being, that marriage, by adding to the female the dangers of child-bearing, would be found to increase her mortality; but it was never once suspected that it would make any difference in the mortality of the male. These facts, however, whose correctness there is no denying, disprove all this, and show that marriage exerts a much more powerful influence on the mortality of the male than all imagined sanitary improvements could ever hope to effect.
At the three quinquennial periods of fifteen to thirty years, married women died at a somewhat higher rate than the unmarried. From fifteen to twenty years, in every hundred thousand married women eight hundred and sixty died annually, whereas, of the unmarried, only six hundred and ninety-two; from twenty to twenty-five years, of the married nine hundred and eleven died, of the unmarried seven hundred and eighty-three; from twenty-five to thirty years, of the married nine hundred and forty died, and of the unmarried eight hundred and sixty-six. During the next two periods, however, married women died in a lower ratio than the unmarried. From thirty to thirty-five years, nine hundred and fifty-six married died, and nine hundred and ninety-five of the unmarried; and from thirty-five to forty years, one thousand one hundred and eighteen married died, and one thousand two hundred and six of the unmarried. It is an interesting inquiry, to ascertain why it is that the mortality of the married, under thirty years of age, is higher than that of the unmarried women. Every man knows that the risk to the mother is far greater at the birth of her first child than at any subsequent delivery; and it is extremely probable that the whole extra mortality of the married female under thirty years old, may be caused by the greater dangers which attend the birth of her first child.

Table V includes the number of mothers in Edinburgh and Glasgow in 1855; the number of these confined with their first child, and the proportion per cent. of mothers who bore their first child. The results are: between fifteen and twenty years of age, eighty-seven per cent. of mothers gave birth to their first child; from twenty to twenty-five years, fifty per cent.; from twenty-five to thirty years, twenty per cent. It was only, then, at the ages when a very large proportion of the married women were giving birth to their first child, that the death-rate.
rose higher than that of the unmarried women; but the moment that age was attained, when the great majority of the women had got over the birth of their first child, viz., thirty years, the mortality fell even below that of the unmarried women. This seems clearly to prove that it is bearing the first child which causes the higher mortality of mothers between fifteen and thirty years of age.

Seeing these things are so, there is nothing to prevent the higher mortality of the women under thirty years of age being quite arrested. Medical men all know whence the dangers of the first birth arise. The causes are almost entirely removable: they are almost wholly due to our civilization and faulty habits, which produce an overexcitable, unduly-stimulated, yet worn-out frame, where health and vigor ought alone to exist.

The Eighteenth Annual Meeting of the American Medical Association will be held in Cincinnati on Tuesday, May 7, 1867, at 11 o'clock, A.M.

Secretaries of all medical organizations are requested to forward lists of their Delegates as soon as elected, to the Permanent Secretary, Wm. B. Atkinson, M.D., 215 Spruce street, Philadelphia.


The principles and practice of physic by the distinguished Sir Thomas Watson, have long been received favorably in America, and we feel assured that the abridgment by Dr. Meylor will be popular among students, as a valuable aid to them in their medical studies. We recommend it.

We commend this small volume, as containing valuable and interesting matter. The author has labored with great zeal in making such a large collection of cases, and suggests rational plans of treatment which are worthy of note.

F.

GEORGIA MEDICAL ASSOCIATION.

This Society met at Griffin, Ga., April 10th, at twelve o'clock, and was called to order by its Presiding Officer, Dr. A. Means.


The hospitalities of the city were tendered to the Association in a neat and appropriate manner by Alderman Nunnally. After which, the press and the city authorities were invited to seats upon the floor during the session.

Next followed the recording of names and calling the roll, when it appeared that about forty members answered to their names.

The minutes of the last meeting were then read and confirmed, and the Society adjourned until 2½ o'clock, P. M.

AFTERNOON SESSION.

The first business was the election of officers for the ensuing year, which resulted as follows:

President—Dr. Chartres, of Savannah.
1st Vice President—Dr. T. S. Powell, Atlanta.
2d Vice President—Dr. DeS. Ford, Augusta.
Corresponding Secretary—Dr. Myers, Savannah.
Recording Secretary—Dr. L. H. Orme, Atlanta.
Treasurer—Dr. J. D. Fish, Savannah.
The Valedictory of Dr. Means was then delivered in his usual peculiar and forcible style of eloquence.

Dr. Chartres, the President elect, on assuming the Chair, made a few brief but eminently practical remarks, when the Society commenced the regular routine of business.

The following resolutions were then introduced by Dr. L. H. Orme, of Atlanta:

Whereas, According to Article 1, Code of Ethics of the American Medical Association, “Every individual, on entering the profession, as he becomes entitled to all its privileges and immunities, incurs an obligation to exert his best abilities to maintain its dignity and honor, and to exalt its standing;” and

Whereas, According to Article 4 of said Code of Ethics, “a regular medical education furnishes the only presumptive evidence of professional abilities and acquirements, and ought to be the only acknowledged right of an individual to the exercise and honors of his profession;” therefore,

Resolved, That while the fact is recognized that there are in our midst medical practitioners worthy, talented, and useful, who, from lack of means, or other cause, have failed to obtain a diploma, yet as the earning of the Degree of Doctor of Medicine furnishes the only presumptive evidence of a regular medical education, the Georgia Medical Association, fully alive to the honor, dignity, and true interests of the profession, deems the admission, in future, of non graduates to membership a violation of the spirit which governs the Code of Medical Ethics.

Resolved, That hereafter no individual shall be entitled to membership in this Association who has not received the Degree of Doctor of Medicine from some medical school of known and acknowledged respectability, and as such recognized by the American Medical Association.

Resolved, That the portion of the constitution which provides for the admission to membership in the Georgia Medical Association of State Licentiates be stricken out.

After some discussion, action was postponed until the morning session.

Adjourned till 9 o’clock to-morrow morning.
SECOND DAY.

9 o'clock, A. M., April 11.

Meeting called to order by the President.
Minutes read and adopted.
The order of business was then suspended for the admission of new members, when Dr. L. Strozier, of Albany, was presented, vouched for, and elected.
Several resolutions were introduced and adopted.
Order of business was suspended, and R. V. Reid, M.D., was presented, vouched for, and elected.

On motion of Dr. Westmoreland, a committee was appointed to revise the constitution.

On motion of Dr. Griggs, a committee was appointed to report the Medical Topography of the State of Georgia.

Also, a committee to report on the medicinal properties and uses of the various unofficinal indiginous plants of the State of Georgia, and other States that they may be acquainted with.

Several resolutions were then introduced and adopted, among which were the following by Dr. Simmons, of Atlanta:

Resolved, That the members of this Association highly appreciate the cordial welcome they have received on the part of the city authorities of Griffin, and that their thanks are due and are hereby tendered to the citizens, and especially to the ladies, for their kind offices in contributing to the pleasures of this body during its session in Griffin, in furnishing such entertainments as are ever agreeable, and which are esteemed as evidences of kind feeling and good will to the profession; and

Resolved, That the thanks of this body are tendered to the vestry of the Methodist Church for the use of their Lecture Room for its deliberations.

Reports from the different committees were then called for and received.

On motion of Dr. Ray, of Atlanta, Dr. Thomas, of Savannah, was elected orator for the next annual meeting.
The rules were then suspended, and Dr. W. H. Touchstone elected a member.

The following resolution was presented by Dr. Banks, and adopted:

Resolved, That the thanks of the Association be tendered to the following Railroads of Georgia, which have kindly made concessions in favor of members of said Association, viz.: Georgia Railroad, Central Railroad, Macon & Western Railroad, Augusta & Savannah Railroad, Western & Atlantic Railroad, Albany & Gulf Railroad, and Macon & Brunswick Railroad.

Upon motion, Dr. DeS. Ford, of Augusta, was appointed Chairman of the Committee of Arrangements for the next annual meeting, to be held at Augusta, Ga.

On motion, the report of the late Treasurer was received, and ordered to be placed on the minutes.

On motion of Dr. W. F. Westmoreland, it was

Resolved, That in the opinion of this Association, there is no breach of the code of Medical Ethics which governs the profession, in physicians contracting with the owners or agents of plantations for the treatment of freedmen in their employ. Provided, that in each city, county, or neighborhood, uniformity of charges be observed, and underbidding avoided.

On motion of Dr. Word, Dr. H. L. Wilson, of Atlanta, late Treasurer, was called upon for a full report.

On motion of Dr. Holt, of Macon, a vote of thanks was tendered to the Recording Secretary and Treasurer for the prompt and efficient manner in which they discharged their duties.

Upon motion of Dr. Crawford, of Atlanta, the proceedings were ordered to be printed in the Southern Medical and Surgical Journal of Augusta (the organ of the Association), and all other medical journals in the State.

There being no further business before the Association, upon motion it was adjourned to its next annual meeting on the second Wednesday in April, 1868.

L. H. ORME, Secretary.
Works Received.

Watson Abridged; a synopsis of the Lectures on the Principles and Practice of Physic. By Thomas Watson, M.D., F.C.P., etc., etc.; abridged from the last English edition, with a concise but complete account of the properties, uses, preparations, doses, etc., with other valuable additions, by J. J. Meylor, M.D. Philadelphia, 1867; 12mo., pp. 276.


Cerebro-Spinal Meningitis; being a report made to the Illinois State Medical Society, June, 1866. By J. S. Jewell, M.D., Professor of Anatomy Chicago Medical College, etc. Chicago: G. H. Fergus, 1866; pp. 68.

Two Cases of Esophagotomy for the Removal of Foreign Bodies, with a history of the operation. By D. W. Cheever, M.D., Assistant Professor of Anatomy in Harvard University, etc. Boston: D. C. Clapp & Son, 1867.

Researches upon Spurious Vaccination; or the abnormal phenomena accompanying and following vaccination in the Confederate army during the American civil war, 1861-1865. By Joseph Jones, M.D., Professor of Physiology and Pathology in the Medical Department of the University of Nashville, Tenn. 1867.

Tableau of the Yellow Fever of 1853; with Topographical, Chronological, and Historical Sketches of the Epidemics of New Orleans since their origin in 1796, illustrative of the quarantine question. By Bennet Dowler, M.D., etc., etc. New Orleans, 1854.

Remarks upon Compound Fractures of the Thigh from Gunshots, treated at Chimborazo Hospital, Richmond, Va. By S. E. Habersham, M.D., of Augusta, Ga. 1867.

A Letter of the Corresponding Secretary of the New York State Inebriate Asylum, to Hon. E. D. Morgan, Governor elect of the State of New York. 1858.

An Appeal of the Trustees of the Inebriate Asylum, to the Churches of the United States and the American public, in behalf of that Institution.


Boston Medical and Surgical Journal. Edited by Drs. S. L. Abbot and Luther Parks.

Buffalo Medical and Surgical Journal. Edited by J. F. Miner, M.D.
FACULTY OF MEDICAL COLLEGE OF GEORGIA, AT AUGUSTA.

I. P. GARVIN, M.D.,
Emeritus Professor of Materia Medica, etc.

L. D. FORD, M.D.,
Professor of the Theory and Practice of Medicine.

JOSEPH A. EVE, M.D.,
Professor of Obstetrics and the Diseases of Women and Infants.

L. A. DUGAS, M.D.,
Professor of the Principles and Practice of Surgery.

H. F. CAMPBELL, M.D.,
Professor of Operative Surgery and Surgical Anatomy.

G. W. RAINS, M.D.,
Professor of Chemistry and Pharmacy.

EDWARD GEDDINGS, M.D.,
Professor of Physiology and Pathological Anatomy.

DESAUSSURE FORD, M.D.,
Professor of Anatomy, general and descriptive.

WM. H. DOUGHTY, M.D.,
Professor of Materia Medica, Therapeutics, and Medical Jurisprudence.

JOHN S. COLEMAN, M.D.,
Demonstrator of Anatomy.

L. A. DUGAS, Dean.