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CANADA

MEDICAL JOURNAL.

ORIGINAL COMMUNICATIONS.

Variations in the Great Arterial Blood Vessels. By GEORGE DUNCAN GIBB, M.D., LL.D., Member of the Royal College of Physicians, London; Assistant Physician and Lecturer on Forensic Medicine, Westminster Hospital, London.

Variations in the origin and distribution of the great arterial blood vessels in the human frame are sufficiently infrequent to invite the consideration of the biologist, more especially when there is a striking and well-marked deviation from the standard normal type.

Although I have seen several instances of variation in the vessels arising from the aorta preserved in many of the museums, and indeed elsewhere, I am content on the present occasion to bring forward two striking examples in adult white individuals, which I had the opportunity of carefully investigating, and I am especially induced to do this, inasmuch as in one of them the peculiarity observed, so far as I am aware on careful investigation, is quite unique.

It must be remarked that deviations in the origin of the great vessels from the aorta are seldom—indeed it may be said never—recognised during life, that is to say when they occur congenitally. And as they are only discovered when life has ceased, they do not constitute a condition of morbidity any more than what is observed in the rare examples of transposition of the viscera. Whether they exert any disposition or liability to morbid action, I think is extremely improbable, as nature so beautifully regulates the compensating forces as to leave nothing to be desired in the completion and easy working of the great processes going on within the human machine, the circulation to wit, amongst others. As far as I know these peculiarities occur in persons, not subject to any external manifestation of abnormality, for in reality they constitute deviations from a normal standard, and are not deficiencies.

The subject has been ably considered by some of the greatest anatomo-

mists, and especially by one who, I am happy to say, lives to see how much his labours in this field of inquiry are universally appreciated by men of science: I allude to Mr. Quain, whose truly magnificent work on the arteries is a monument that will endure for ages yet to come.

In the first of the two examples which engaged my attention, the aorta gave up four branches, instead of the usual three. These were the left carotid and subclavian arising in the usual manner, and the right carotid and subclavian, each arising direct from the arch of the aorta by a distinct and separate trunk, there being an absence in consequence of the innominate. The two vessels on the right side were larger than on the left; the left carotid was the smallest vessel of the four. The course of these vessels was the usual one, but the laryngeal branch of the superior thyroid artery of the left side, perforated the thyroid cartilage, instead of passing inwards through the thyro-hyoid membrane in the usual manner. On the right side however this vessel did perforate that membrane. This peculiarity has been described and figured by Mr. Quain, and my diagram of it, recently exhibited before the British Association at Nottingham, has been mainly framed upon his.

Both femoral arteries varied in their division—that on the right side divided into two branches at Poupart's ligament, the anterior taking the usual course of the femoral, whilst the posterior continued downwards and inwards, giving off its different branches irregularly, and different from usual. The left femoral on the other hand, divided into three branches—some might call it four, from the internal circumflex originating near the origin of the profunda. I do not purpose considering them. The left great ischiatic nerve divided into two branches on its emergence from the great ischiatic foramen, whilst the right great ischiatic nerve was normal.

The singular feature was noticed in this individual, that the left side had remarkable peculiarities, which were not observable on the right; for example the left femoral artery and great ischiatic nerve differed from those on the right side in their general division, and so did the left laryngeal branch of the superior thyroid from the right as already described.

It is not my intention to say anything of the morbid changes that were noticed in the person, a man of sixty, who died of phthisis, who possessed these peculiarities, as they had nothing to do with them. But it may be stated, that all the arterial trunks with their ramifications, both of the upper and lower extremities, and noticed during life, were in a state of firm calcification, the vessels had become converted into hard cylinders, and could not be compressed without fracture. This

condition was absent in the four vessels that sprang from the arch of the aorta, and did not involve those of the neck, but commenced in the axillary on either side. It likewise commenced in the iliacs and extended downwards. The age represented by the person thus circumstanced was 100 years, although not in reality more than sixty, and he may be said popularly to have had his arterial blood vessels converted into bone. The muscles were normal in their arrangement.

The comparative infrequency of four separate trunks being given off from the aorta—without crossing one another or going to opposite sides,—and excluding the vertebral which sometimes arises from the aorta—is perhaps not very great, as in the instance under consideration. But taken with some peculiarities in the vessels on the left side of the body and more especially in the left great ischiatic nerve, the deviation becomes one of general interest.

In the other and second instance, the aorta is *almost* disposed to confine itself to the well known usual type, but on careful examination it was seen that the main trunk had divided into *two great branches*, the *first* of which subdivided into the innominata and left carotid, the latter crossing the trachea obliquely upwards to the left side; the innominata divided into the two usual branches of right subclavian and carotid.

The *other* subdivision of the arch was into the left subclavian and descending aorta, both vessels taking their usual course.

If my interpretation of this peculiarity is correct, then we have a unique instance of division of the aorta into two branches, which in their subdivision give off the proper trunks.

No similar example has been found, even in Mr. Quain's great work, and the inference is that it is unique. Differing from the first case considered, there were no peculiarities noticed in other parts of the body.

So striking was the appearance presented by the aorta in its division, that when first seen, it immediately attracted special notice. (The specimen is preserved, labelled B. C. 4).

Mr. Quain gives three examples of reduction of the primary branches to two, as in figures 6, 7 and 8 of his 6th plate. The nearest approach to my example is his No. 6; but a comparison of the two figures will show that in mine the parent trunk is even larger than that part of the aorta going to the left, and I may say that I was *particularly* careful to give a correct rendering of this peculiarity of division, on the spot, at the time of its discovery, so as to avoid any possibility of error.

The only example of a dichotomous division of the aorta in a normal condition which exists in ourselves is at its termination into the two

iliacs. But in the example under consideration we have the occurrence of such a remarkable phenomenon—for so it may be called—at the very commencement of the great arterial system. It is possible that others may have met with a similar instance, although as stated before none have been recorded.

Besides the desire of contributing something decidedly new to our biological knowledge, I have believed that the examples brought forward might stimulate others to work in a field of enquiry which, though well worked, still affords now and then something that has not been before witnessed; and a multiplication of valuable facts on the subject of the present paper cannot but prove eminently useful to science.

With regard to these peculiarities of the arteries of the aorta, can we draw any inferences of value explanatory of their occurrence, or do they possess any bearing upon the physical structure of the economy?

There is no explanation of their occurrence afforded, beyond the fact that such peculiarities as occur rarely in the human subject are common in some of the lower animals, to wit, the quadrumana; and therefore they merely point to a connecting link in the chain of progressive development in the animal creation. It has been asserted that the great arterial vessel known as the aorta gives off in many instances four great trunks in the Negro; but as it has been noticed probably as often in the white race, we are not justified *at present* in drawing any conclusions, that the black race are necessarily of an inferior type to their white brethren from an approach to the quadrumana in the arrangement of the aortic blood vessels, nor indeed am I aware that it is common to the black race.

Nor can we assume that these peculiarities exert any special influence over any part of the animal economy, unless that their co-existence in other parts of the body may give rise to morbid conditions demanding surgical interference.

This short paper with several diagrams, was brought before the biological section of the *British Association for the Advancement of Science* at the Nottingham meeting in August last, and I am induced to send it for publication in the *Canada Medical Journal*, inasmuch as the second and more remarkable variation described in the paper is preserved in the museum of the French School of Medicine at Montreal, marked B. C. 4, and where I trust it may still be seen.

1 Bryanston Street, Portman Square.

London, November, 1866.

A Case of Triplets. By GEO. D. SPOONER, M.D., Clarke, C.W.

ON March 21st, 1864,—I was called to attend Mrs. D.,—the mother of several children, in her then expected accouchement. On my arrival I found the child born and in the arms of a nurse, but the patient, as I observed on approaching the bed, did not appear as if she had given birth to a little one. Had some slight pains, and an examination revealed another presentation (head), and in half an hour the second child was born. I was again surprised to find that that made no apparent diminution in the size of the abdomen. Made another examination and found another child, breech presenting, which in another half hour was brought into the world. I had considerable difficulty in getting the last to respire; but after diligent perseverance, with hot and cold water alternately applied, and inflation of its lungs by direct application of my own mouth, I had the satisfaction of hearing the little thing cry very vigorously.

There were three placentæ and two sets of membranes: the cords were remarkably short, the longest being fifteen inches, the shortest eleven. The mother made a good recovery; and now, thirty-three months after, "the three" are as well-grown and healthy looking as any in the country. At their birth they weighed $5\frac{3}{4}$, $6\frac{1}{4}$ and $6\frac{1}{2}$ lbs., one being a girl, and two boys.

December 7th, 1866.

CANNIFF'S PRINCIPLES OF SURGERY.

To the Editor of the Canada Medical Journal.

Would I trespass too much upon your space in making a few remarks respecting the very kind notice you have been pleased to give of my Principles of Surgery?

I do not come as a fault finder, for you have accorded me no little credit. But I would beg permission to demur to your statement that you "cannot commend the book as containing original teaching, the result of extended observation." The same thing has been stated by a Medical Journal in the United States, although the Medical Press there has bestowed upon the work praise of the most decided kind.

This broad statement at the commencement of your remarks, beyond which some readers might not go, I fear may leave an erroneous impression upon the minds of some, as to your opinion of the work. The credit you subsequently grant me is most gratifying, and would almost be sufficient to satisfy any one; but an author, when he thinks he has given to the public original thoughts in connection with the thoughts of

others (which he has in many cases digested) is unwilling that his own should be quite obscured by the latter. Such it seems to me, is the position I occupy; while in my prefatory remarks I candidly acknowledge the several sources from which I had drawn knowledge, I was under the impression that from time to time I had thrown some light upon the matter under consideration. You will therefore, I hope, pardon me; and the profession I trust, understand my motives if I undertake to indicate a few of the points wherein I believe I have said something original.

Upon the first page is the following declaration: "I shall endeavour to show that *healthy* inflammatory action is a step—a first step—in the process of healing; that, while healing more expeditiously takes place when inflammation is absent, yet, under certain circumstances, the inflammatory process is necessary to prepare the way and to supply the means for successful healing. When, however, the inflammatory action is *unhealthy*, when there is presented some one of the several *varieties of inflammation*, then there is a *de facto* disease. The distinction between the two will be found to be the same as that which exists between a healthy ulcer and unhealthy one; in the former, healing kindly takes place in accordance with physiological laws, and in the latter it fails to take place." Whether I have succeeded in showing that such is the case, may be questioned, but if any writer has ever declared the same thing, I have been so unfortunate as never to see it.

Again, on this point, there appears on page 65: "There are two things which ought to be constantly borne in mind by the surgeon in the treatment of this disease (inflammation); the first of which is that in the inflammatory process itself, and at each step of the process, nature seems to be making efforts to cure, and that many of the phenomena of the disease are the results of those efforts. To this subject I beg leave to direct especial attention. Writers on surgery have been accustomed for so long a time to speak of the phenomena of inflammation, and indeed of all surgical affections as purely *symptoms of disease*, that it is next to impossible to disassociate the indications therefrom, and to regard them instead as exhibitions of the admirable workings of nature to stay disease, to remove the cause thereof, and to effect a cure."

But it is with respect to fibrin I wish to refer particularly. It was my privilege to teach that fibrin was not the pabulum of the tissues when such was regarded as heterodox. But in my work I have essayed to show, not alone that fibrin is not thus employed, but what its office in the economy really is. Others had distinctly shown that it could not be the nutritious juice. I made bold to say that it had another and a distinct duty in the physical economy. Upon page 30 will be found stated

“ that the exact chemical constituents of the nutritious juice which transudes (through the coats of the capillaries) in health and which is taken up by the cellular structure, according to Virchow, and becomes tissue, has not been determined. The opinion long prevailed that fibrin constituted this material; but there are insurmountable difficulties in the way of continuing to regard it as so high a product. Rather must it be conceded that it bears evidence of being a material which is in the decline of life.”

“ Now may it not reasonably be supposed that the fibrin thus placed without the capillaries (that is, effused in inflammation) and unable to enter into the formation of tissues either on account of its own beginning metamorphosis, or the inability of the tissue to appropriate it or perhaps both, and having thus been turned aside from the high position for which it had been intended, assumes a new position in respect to its vitality and its capability of acting any part in the operations of life in the system. Although it has, on account of circumstances apart from its own development, failed in fulfilling its primary destiny, it constitutes a no less useful material; although it may not act a part in the ordinary repair of tissue, it serves a purpose quite as important and indispensable in effecting *extraordinary* repair. The elements of the blood which would, had not the natural functions been disturbed, have become tissue, deviate from this natural course of development and become the agent of the *healing process*. The nature of this change is not uncertain; there is not only a change but a degeneration direct from one stage of existence to another. It is a retrograde development. I will here simply venture the statement that the object for which fibrin exists in the blood is to heal, or effect extraordinary repairs. That it is a material limited in quality which “ is kept in store for that purpose, and which is always found to increase rapidly in quantity, and become more efficient in quality, when a demand for it is made.” Again on the 49th page “ the tissues of the body are constantly exposed to the vicissitudes of life—to disease, and to accident—and nature would fail to maintain her reputation as a conservator, and as wise to provide all things necessary for life, did she not have in readiness an agent to meet the sudden requirements of disease, and of the various disturbances to which the tissues are constantly exposed. Herein, I would venture to assert, consists the importance of fibrin, This is the special object for which it exists; it is by this that the healing process is carried on.”

Upon page 175, under the distinct heading, “ *The Special Agent for Repair*,” may be seen as follows: “ It is the fibrin which performs this most important duty, second only in importance to nutrition and

growth. Whatever other purpose the fibrin may serve, this is the principal, seemingly, for which it exists, and it is a function sufficiently high, so that those who have been wont to regard fibrin as the pabulum of the tissues. have no reason to think that in losing that status it has sunk into insignificance. Can it be thought unlike the doings of nature to suppose that she specially prepares a material for *extraordinary* repair—for restoring parts more palpably destroyed? Judging from numerous facts and data in the possession of our profession, one does not hazard much in venturing the opinion that this is the principal, if not the sole, use to which this element of the blood is intended to be put.

“ This view has forced itself upon my mind, and it is in accord with physiological facts. The fibrin is an element of the blood, which primarily was capable of entering into the process of nutrition; but not being required, it is left to undergo a change by which it is made to occupy a grade somewhat lower in the scale of vitality. We cannot suppose that the supply of food, and that part of the blood, which is the most highly vitalized, and intended for the nutrition of tissues, will always be just coequal to the demand; nor can we admit that the supply in health is ever *below* the requirements of the body. Consequently, we may reasonably suppose that there is often, or always, found in the blood of the body more material for nutrition than can be well made use of. The elements of the blood, like the tissues of the body, have their day of growth, development, maturity, decline, and death. When the tissue's food has reached its highest state of vitality, it is fitted to form a part of the most delicate organ in the frame. But should it not be required,—and we have seen that in all probability there is commonly more than the tissues do want, then degeneration sets in, and that portion will have passed the meridian of its life, and thenceforward cannot possibly take part in ordinary repair; but may, if required, take part in extraordinary repair—not, it is true, to be assimilated to the tissue, but to constitute a temporary structure—a scaffolding, whereby the permanent and the natural edifice shall be built.

“ I would liken the fibrin to refuse timber, which can always be seen strewn about a building in the process of erection. It has been rejected not only because better were available, but because other and enough were more convenient. This material, thus declined, is regarded as a waste substance; and, in consequence of its being thus regarded, is subject to deterioration. Now, this timber, which has failed to form a part of the edifice for which it was prepared, may yet discharge an important duty, by being employed to construct a temporary stage, or scaffold, by which the permanent structure shall be completed. So the fibrin. Yes—

terday, under another form, it was perhaps qualified to enter into the formation of tissue. To-day, in the decline of life, it is no longer fit to accomplish so high a purpose—yet not so degenerated but it may answer a temporary use in the work of extraordinary repair.

“Several facts seem to support the view advanced; two particularly: first, the very small quantity of fibrin in the blood when the body is in a state of health; second, the well-known fact that the fibrin increases in a part as soon as the inflammatory process commences—that it is elaborated in excess immediately upon a prospect presenting itself of its being required to restore the part. And there are other facts which serve to corroborate the theory, which will become apparent as the healing process, in its several forms, is duly considered.”

Now, if the foregoing has been previously taught, I was, and am, oblivious to the fact. The physiological facts upon which this theory is based had been of course discovered, but their application I must claim for myself.

Upon page 180, under “*Organization of the Fibrin*,” it is taught that fibrin, upon the surface of a wound, while manifesting a certain amount of vital power, will fail to continue a living structure, unless blood-vessels had formed in it prior to coagulation, and the discoveries of Paget are pointed to where the vessels become elongated and sacculated, as indications to form such vessels. Then it is elsewhere taught that by these vessels the coagulated fibrin is gradually removed as it dies particle by particle, and by these is substituted the natural structures.

The terms *ordinary* and *extraordinary* repairs of tissue, and the differences, although foreshadowed by Paget, have not, as I am aware, been pointed out by him, or any other writer.

While fibrin is set up as the *great healer* of tissues, when palpably destroyed, the various modes by which it acts are mentioned. Thus speaking of the continued fluidity of fibrin when transuded in inflammation, I have said on page 53:—“The length of time which elapses before it (*Liquor Sanguinis*) thus separates into its component parts—serum and fibrin—varies; it may take place at once, or it may not for some length of time; or a portion of the fibrin may become solidified, while another portion remains in solution. In each event it is not difficult to observe an attempt to arrest disease, or restore to health. Perhaps further observation will enable us to say there is always an early tendency in a portion of it to stiffen around the small vessels, and thus, by *external* support, assist to prevent over-distension and paralysis; and fixing tissues, as it were, to secure a degree of *rest*, which will favour recovery.”

“Coagulation is the last act of life in fibrin when separated from the body, and in the main it must be regarded the same when only without the blood-vessels; hence the unwillingness of nature, so to speak, to take a step by which the fibrin becomes in most respects a foreign body. While in solution, or only solidified in a limited quantity, it can readily be absorbed; but after coagulation, the process of absorption is necessarily slow. This property, therefore, possessed by inflammatory lymph, of remaining in solution for a longer time than natural fibrin does, is one of salutary importance, in so far as recovery is concerned.”

Page 58 has the following statement:—“I wish here to refer particularly to the interesting and important fact, that in each tissue and organ of the body, such lymph is elaborated, in connection with the inflammation, as will best serve a curative purpose.”

In speaking of the healing process; by pointing out the several modes whereby it is accomplished, and then the departure therefrom, and giving to such departures the new term *diseases of the healing process*, it is respectfully submitted that the whole subject is not only simplified, but placed on a new basis. Instead of each of the several ulcers being considered as an individual disease, it is presented as a disease of a natural process.

In discussing the question of duality of syphilitic poison, I have said on page 153: “Now, it is an important fact that syphilization rarely, if ever, follows as a result of the soft chancre, although it cannot be questioned that it is quite as much due to a specific disease as the hard is. From these facts it has been inferred that there are two kinds, at least, of syphilis; that one kind is characterised by a hard chancre, followed by constitutional symptoms, and the other kind by a soft chancre, which is not followed by further disease. But the correctness of these conclusions may certainly be questioned. There are three circumstances, as just above stated, which may control the changes, which took place in lymph subsequent to its effusion. We have seen that after the application of the poison, the inflammation may be either adhesive or suppurative. Now, it would seem that the poison so acts upon the part that the inflammatory lymph exhibits a decided tendency to congregate, and probably this is the natural effect of the poison in every case; but in certain cases, may it not be (and I offer it only as a plausible view,) that another controlling influence, adverse to the first, comes into operation, very likely arising from the state of the blood, by which lymph is made to degenerate into pus? The conclusion which I wish to draw from the above is, that the syphilitic poison is always the same; but its effects upon the system may be modified by the state the system is in at the time the poison is brought in.

contact with the body. *That it naturally causes adhesive inflammation, but under certain circumstances as this tendency is counteracted, that when it acts alone, and causes the hard chancre, inoculation is the result, but when from any other cause the soft chancre is produced, the poison is ejected and no syphilization ensues.* When, therefore, a Hunterian chancre is met with, it cannot be inferred that the constitution is already affected, and the hard sore is the effect of constitutional disease; but that there is imminent danger of such constitutional affection—that the poison is being held in the most suitable state to secure syphilization.”

“In connection with this question, it requires to be mentioned that swelling of the glands of the groin, and the formation of buboes, is more generally an attendant of the soft chancre than those of the hard. This fact, although at first it may seem to oppose the view advanced, in reality supports it. It is not at all irrational to suppose that, although venereal poison is mainly expelled by the suppurating sore, yet the lymphatic glands may be called upon to stay the entrance of a very limited portion, a duty which they faithfully perform, even at the expense of their own integrity. The poisonous material even here causes inflammation, followed by suppuration, and so the pus and the poison escape together, while on the contrary, in the case of the hard chancre, the syphilitic poison has only the effect of causing induration of the glands, and thereby incapacitating them for their natural duty, so that the system becomes contaminated. To repeat: in the case of the soft chancre, the most of the syphilitic poison is cast out by the suppurating sore, and what little may be taken up by the absorbents is arrested at the first system of glands; consequently these glands take on a similar suppurative action, whereby is disposed of that limited portion which had so far obtained an entrance to the general system.”

Whether the foregoing reasoning is correct or incorrect it, at least is my own.

In considering the diseases of the bones, I think that I have pursued a plan not previously taken by writers. Especially in caries I feel there is to be found much original thought. Also in the pathology and treatment of fractures, I am certain of having taught, what I had no where else seen. Respecting the treatment of fractures, I must maintain that in the principles ably laid down by Hamilton, and received by all surgeons, I had taught them in the class room before I ever knew of such a person as Hamilton.

In conclusion, allow me to give the opinion of the gentleman who did me the favour to read the proof-sheets. Dr. Moffatt, physician-in-chief to one of the hospitals at New York, says, in a note to the publishers:

"I cannot forego the pleasure of bearing testimony to the excellence of the book, which is in many respects superior to any yet published on this side of the water. Some of the views advanced in reference to the processes involved in inflammation, as well as the reasoning based upon them, have the merit of originality. Whatever else may be said of them, it cannot be denied that they are very ingenious. The careful reader cannot fail to be impressed with the conviction that the author of the work is a profound student and a close observer. His love of the subject, as evinced in the enthusiasm which glows on almost every page, gives promise that in succeeding editions he will supply what is wanting, and thus render year by year the work more and more complete and valuable."

But I fear I have encroached too much upon your space. I must say that I felt a great unwillingness to thus obtrude my own work upon your attention. I trust, however, that the reasons given above will be deemed sufficient. I repeat I do not complain of your notice of my work, for it is written in the kindest spirit, and has bestowed no little credit. I only desire to indicate, what was no doubt an oversight, wherein there is, according to my own knowledge of surgical writers, to be found something original.

W. C.

Belleville, 26th November, 1866.

REVIEWS AND NOTICES OF BOOKS.

Orthopedics: A Systematic Treatise upon the Prevention and Correction of Deformities. By DAVID PRINCE, M.D., Svo. pp. 240, Philadelphia: Lindsay and Blakiston, 1866.

(Continued from page 201.)

The author discusses in the third section of the work the pathology and treatment of articular diseases. The forty-four pages devoted to the subject might have sufficed for a thoroughly competent pen to give a brief and comprehensive digest of this very interesting topic. But the author is obviously not at home in this matter, and has certainly failed to redeem his pledge (perforce) to render "the advance in knowledge on this subject, gained within the last twenty years, accessible to the mass of the profession." In fact, this section is made up of quotations from Barwell's treatise "On the Joints," in the style of Braithwaite's *Retrospect*, with a few insignificant comments. This is certainly a very com-
modious way of book-writing, though of questionable literary value, and

hardly commendable for imitation. However, our author is of but modest pretensions, and a comparative novice in the arena of science; we must, therefore, look with leniency upon his first literary effort. Nevertheless, we believe that a candid and well sustained criticism is not only compatible with kind intentions, but even preferable to the sloppy and stereotyped literary notices in which some journals indulge, and which they keep on hand for the most contemptible products of the pen.

We are not surprised to find the author kneeling at the shrine of that pathological chameleon, otherwise known by the appellation of strumous disease. Neither he, nor his fountain-head, show any disposition to define that malady, or furnish reliable anatomical details for differential diagnosis. They simply take it for granted that there is such a disease old enough to be known and understood by every reader of their respective works. We must forego the pleasure of discussing this point at length. In order to let the author know that the question of the existence of strumous disease is not as yet settled beyond dispute, we beg to remark that no less an authority than Virchow denies it *in toto*; the modern ophthalmologists have expelled scrofulosis from their precinct; and Louis Bauer has disputed the strumous congestion of joint diseases. The author may not choose to accept of their opinions, but then he should say so, and disprove their arguments and clinical facts. To ignore them may be convenient, but it exposes the author to the charge of ignorance or temerity in grappling with them. Withal it seems that Dr. Prince has not a very serious apprehension of that *deas ex machina*, when he pronounces (page 49) the indications for treatment very similar with those inflammatory diseases that arise from wounds and injuries of the joints. And, indeed, the treatment adopted by the author, and commended to the reader, precludes the supposition of constitutional causation at all.

The pathological details do not invite close analysis, for they are but fragmentary and ill-digested. Thus, for instance, he favours the opinion that the synovial membrane is the most susceptible structure of the articular components, notwithstanding that they are deprived of nerves and vessels; and to the bones, and especially to the epiphyses, though so richly endowed with them, he allowed, as it were, a back seat. The description of strumous synovitis borrowed from Barwell is so general as to apply to any joint disease, traumatic or constitutional. Next it is self-evident that violence must expend its force much more upon the bones than upon a yielding membrane protected by integuments and fat. However, we cannot bring to bear upon the author's views a more

elaborate criticism, space prohibiting, and must defer doing so to another occasion.

About the treatment of this class of diseases we have to say but little. The author has certainly and most diligently compiled nearly all that is known, without being complete or in every respect comprehensive. Nevertheless, we feel grateful for many historical notes.

Like other good surgeons, the author insists upon absolute rest of an affected joint, omitting, however, to indicate the proper means to achieve it. In the beginning of these diseases rest alone is sufficient to prevent the advance. He deems recumbent posture and extension the best. Neither insures rest. The former removes the weight from the joint; the latter elongates the muscles, and prevents spasms and contractions. With both the patient can move the joint. The simplest and cheapest way to procure rest in a certain position is a firm bandage around the joint, extending to the next articulation, and thoroughly impregnated with dextrine, plaster of Paris, or silicate of potash, the last being in favour with the school of Vienna. In some joints splints of leather and sheet iron may be substituted. In incipient hip-disease, the "wire breeches," introduced by Louis Bauer, secure complete rest, and proper position of the affected extremity. These important improvements in modern surgery, Dr. Prince has partly underrated, partly ignored. Extension is to be resorted to when spastic oscillation commences; but the ordinary method of applying it by means of adhesive strips, pulleys, and weights, is certainly inefficient, because it enables the patient to accommodate himself to the traction. We have to avail ourselves of a fixed point. The perineal strap does not answer the purpose. One apparatus, which keeps the pelvis in a steady position, is the mode indispensable when the muscles have already commenced to retract; and the author would certainly have insisted upon its use, had he acquired an extensive experience in the treatment of this class of diseases. We must take issue with the author on the efficacy of extension in muscular contractions. In a few cases of forcible, a persistent gradual extension may suffice to overcome the organic resistance of contracted muscles, but it mostly fails. Experience has decided this fact. The division of the resting structures becomes then indispensable. The author has utterly failed to appreciate the therapeutical action of myotomy and tenotomy. They not only annihilate resistance, and thus facilitate a proper position of the joint, but they act as a powerful antiphlogistic, contributing more to the arrest of the disease than the balance of remedies in vogue. We have practised both extension and division long enough to have an opinion on the relative efficacy of either.

A great advancement was wrought by the introduction of portative extension inasmuch as it enables the patient to enjoy the desirable open air exercises. And Dr. Davis is entitled to the full share of credit for this improvement. There remains, however, a vast field for mechanical perfection of those portative apparatus heretofore in use. When Davis's splint became known, the profession was in ecstacy, and indeed it has a conditional value. Experience has however decided that it cannot cope with aggravated cases of hip diseases, cannot render dispensable the use of the knife, nor prevent the progress of the disease. The same objections apply to Sayre's, Barwell's, or Vedder's respective splints. Dr. Andrew's pelvic crutch is evidently an improvement in the right direction. We are truly glad to have seen it. Sayre has lately published the like apparatus for the knee, and ankle joints. They are both ingenious and effective, and should have found a place among the numerous illustrations of less value in this work.

In effusions of the joints, the author thinks that the use of the trochar would do no harm. We think it will, unless the joint is properly prepared for the puncture, or such movements are made with the joint as to favor the exit of the liquid, and prevent the entrance of air. Barwell has informed the author to whom surgery is indebted for this operation, but the author thinks so little of it as to mention it merely in passing.

The author is still inclined to favour counter-irritation, and doubts that remedies of which our professional ancestors thought so much, should have become entirely worthless to the present generation. The same logic applies to indiscriminate bleeding, and yet it has been almost entirely abandoned for very relevant reasons. Dr. Prince may tell a nice little story of a boy who submitted to the hot iron, under the promise of being taken to the theatre, but it proves very little to the point. Counter-irritation has been indeed thoroughly tried and assuredly too much so for the good of the respective patients. But having disappointed the expectations of both surgeon and patient, and rather aggravated the suffering of the latter, it has been given up by the wiser portion of the profession. We have seen cases in which the fly-blister instantaneously produced spastic contraction, in diseases of the knee and elbow joints. The author may not have had the same opportunities, but then he is disqualified for counselling others on this point.

On page 58, the author commends the bold and free incisions of the late Dr. E. S. Cooper "to give free outlet to the offending fluid in violent inflammations," and on page 61, he is inclined to the advice of South who "thinks it preferable not to meddle with abscesses of the hip joint!" Every sound surgeon will follow the contrary maxim. That is to say, he

may be forced to relieve an inflamed joint of its effused material by puncture, but articular abscesses by free incisions, for reasons which require no further proof.

The 4th division on lateral curvature of the spine, is so flippantly compiled as to be rendered useless in every particular. The subject is yet, involved in some obscurity, but Dr. Prince renders it still more obscure. Lorinser of Vienna is the only author who ascribes lateral curvature to morbid changes of the spine, and its ligamentous apparatus; whereas the greatest pathologists, including Rokitsansky, emphatically deny it, nor is the opinion of Lorinser borne out by facts. Yet our author enumerates under II, as one of the common causes, and links it with violence to the spine in general. The case by which he attempts to substantiate his opinion on page 125, is certainly a most singular one. His patient, a lad of 15 years, fell from a house, three years previously, striking upon his head and was nearly helpless for a few days after. "This is the only known cause" of the deformity that occupied the superior half of the spine. The author informs us that after ten days' use of his apparatus delineated on page 107, "the patient had increased half an inch," after two months $1\frac{1}{2}$ inch; and after five months $1\frac{1}{2}$ inch; and he thinks he might have gained still more if he had kept the patient in the horizontal posture. A competent reader might feel inclined to take the whole, as an opportune story and might apply the remark of Solly; that but "knaves and ignoramuses" could assert the straightening of a curved spine tantamount with the increase of height; but we do not choose to deal so uncharitably with Dr. Prince as to question his veracity, though we cannot save his diagnosis of "softening of the spine." The only commentary we have to offer is, that the patient, in consequence of the violence sustained, suffered from local paralysis of the muscles concerned in the deformity, and that the doctor succeeded in re-establishing the proper innervation, for his apparatus is too ineffective to bring about the claimed result.

Under II. Dr. Prince enumerates spasmodic contraction of dorsal muscles as another of the causes of lateral curvature. This opinion has been started by Jules Guéin, but Malgaigne, has so thoroughly exploded it as utterly groundless, that we are rather surprised to find the author still possessed of it. It is well known, and should not have escaped his notice, that the former extensively practised myotomy upon the supposed contracted dorsal muscles, and had the coolness to promise to the French Academy of Sciences ocular demonstrations by his patients he pretended to have restored to their normal form and height; but unfortunately his patients, on examination by a committee, proved the very contrary. In the treatment of this deformity, he reproduces the accepted opinions and

sustains them by respectable authority. He illustrates quite a number of spinal braces, of which his own seems to be the most useless of all. Mr. Broadhurst's lever apparatus, certainly one of the best in existence, has found no place in the work.

The next article on Pott's disease, is evidently compiled with diligence. The deficiency of the author in practical experience is however noticeable. Under the causes enumerated we find constitutional trouble very preeminent, whereas injuries with their direct and remote effects are slighted. The author seems not to be aware that in many instances fractures of vertebral bodies initiate this deformity. That fractures occur much more frequently in this connection than seems to be generally supposed, has been proven by the experiments of Bonnet, by pathological examinations, and by clinical cases. We miss, likewise, whooping cough and syphilis, as casual causation of posterior curvature; last, and not least, softening of the intervertebral cartilages and periostitis of the spine come in for their share.

Generally we accept the treatment of Dr. Prince, as well sustained with the exception of a few propositions. Medication is rarely needed in these cases, and least of all should we feel inclined to purge the patients once a week. Next we deem counter-irritants not only dispensable, but absolutely objectionable. Lastly, we look with the eye of Mr. Solly upon Dr. Taylor's plan of straightening the curved spine by braces, acting like a lever upon the superincumbent portion. Anybody at all acquainted with the pathological condition of Pott's disease must pronounce suggestions of this kind as reprehensible folly. The efficacy of mechanical appliances in the cases, is altogether questionable. As long as the part of the spine is softened, disintegrated, and physically unfit to sustain the superincumbent weight, the patient is better left upon a water bed, and in the horizontal position; and when the disease has terminated, the affected bones have regained their firmness, and perhaps been strengthened by newly formed bone splints (osteophytes), then the patient is actually recovered, may leave his bed, resume the erect posture, and needs no mechanical support whatever. The cuirass of Bonnet, and the dorsal splint of Bauer may be useful in protecting the patient from jarring the spine whilst he is carried, or drawn about in the open air, but nothing more is wanted in the line of appliances.

Passing over some rather indifferent topics, as corns, bunions, absence of toes, etc, we proceed now to the subject of talipes. An attentive reader cannot fail to notice that Dr. Prince has given to this subject thought and diligence; and that portion of his treatise which considers the deformity of the feet, is without a doubt, the best portion of the

work. We would have little to say to mar our approval and commendation, had the Doctor relied on his own judgment and experience; but inasmuch as he has chosen to become the special pleader of Mr. Barwell in this as in another divisions of the treatise, we feel impelled to give him the benefit of our disagreement.

Dr. Prince discriminates six varieties of talipes, viz: tal. equinus, dorsalis, varus, valgus, plantaris, and calcaneus, with their respective intermediate combinations. Why the species of tal. plantaris should have been set up as a new form is not intelligible, since the author by his own illustration gives it as a minor grade of tal. valgus, to which species it therefore belongs. What he pleases to define as tal. dorsalis has very properly been termed tal. plantaris by others (Bauer), because the abnormal arching is the result and not the cause, which latter rests undeniably with the shortened plantar, aponeurosis and muscles.

To Barwell the author pays the unmerited compliment of a better anatomical understanding of talipes, unless he justifies it by the new name for the intertarsal articulation which the former has originated.

Although Dr. Prince attempts a wide range for the etiology of talipes, he nevertheless settles quietly down upon "permanent spasm and paralysis" as the usual cause of these malpositions. The physiological character of permanent spasm is somewhat problematical. We cannot understand a tonic spasm of years' duration, because the muscle loses its contractile anatomical elements and is almost entirely converted into a cord. If the definition of contraction given by Barwell has any meaning at all it applies precisely to such a condition. But it evidently did not suit the author to dispense with spasm, which, as a special pleader for elastic extension, he needed as an indispensable premise.

We need not state that the author, being in favour of extension, is a strong opponent of tenotomy in the treatment of talipes, and he therefore fortifies his position by all the arguments that have been raised against it. Among others the Doctor is very apprehensive that the divided tendons will not re-unite, and that the muscles will lose their function commensurate to the intermediate scar-tissue, if such should be formed at all. An extensive experience on this subject entitles us to a vote. Now while we do not want to question the credibility of the statistics adduced on page 1767, we can say this much of our experience that we have had but one case of non-union of a tendon in our practice. Perhaps the after-treatment which we observe by keeping for some time the fragments of the divided tendon in close approximation by an appropriate dressing, may have something to do with our results. Thus far we at least have no reason to oppose tenotomy. Next we do not see how

the author can overcome the resistance of contused muscles by gentle, however persistent, traction by means of elastic webbing. Out of five cases of varus four cannot be reduced by the most powerful hand, and division of the resisting structure seems to be imperative. Moreover such powerful extension or elongation of the latter is by no means without danger as we have shown in another place. It is evident that difficulties which cannot be reduced by the hand of a powerful man are not likely amenable to minor remedies. Yet on page 184 the author thinks that "in cases of obstinate resistance to reduction by extension" force may be legitimate. To what extent the author wants us to apply force may be inferred from his own statement which we quote. "The most tense ligamentous fibres and the investment of the muscular fibres in shortened muscles are torn." In other words, the author likewise adopts division merely by a different process. Whether this is preferable to a harmless division by the knife, experience alone can determine. It seems a folly to commit one's self to any plan unconditionally. There are cases of talipes in which the knife has to be employed, and there are others in which extension may have been advantageously resorted to; and we concede with pleasure the author's merits in prominently urging the method of extension.

We close herewith our criticism without having exhausted all our strictures on the views and statements of the author that might have been submitted without fastidiousness. On the other hand the author has handled the subject of his treatise with some ability and literary tact. His language is fluent and rather agreeable when not spoiled by quotations. We hope that this book will find a ready sale, and thus enable the author in his second edition to correct the errors into which he has been led by implicit faith in some authorities more plausible than correct. We beg to express the hope that the author as well as the reader will accept the interpretation of our purpose to serve science, and its progress.

A Manual of Medical Jurisprudence. By ALFRED SWAINE TAYLOR, M.D., Fellow of the Royal College of Physicians, and Professor of Medical Jurisprudence and Chemistry in Guy's Hospital. Sixth American from the 8th and revised London edition, with Notes and References to American decisions. By Clement B. Penrose, of the Philadelphia Bar. Svo. pp. 776. Philadelphia: Henry C. Lea. 1866.

This work has for years held the place of being the standard authority on the subject of Medical Jurisprudence. Dr. Taylor has recently published a large work entitled, "The Principles and Practice of Medi-

cal Jurisprudence. In this, the eighth edition of his Manual, considerable alterations have been made. These consist in the omission of the full details of cases, a synopsis only being given, sufficient, however, to be of practical utility to students of medicine and law; while the more elaborate work above alluded to can be consulted and cited in courts of justice. There are two chapters on medico-legal evidence and the duties and responsibilities of medical witnesses. These are of great use, as affording information and guidance to medical practitioners, who are too often subjected to unnecessary cross-examination by counsel. These chapters are deserving of special and earnest study. They are clear, concise, and give a fair and defined idea of the rights and privileges of a medical witness. The author has introduced into this edition of his Manual numerous wood-cuts, illustrating the crystalline form of poisons under microscopic observation, and also the apparatus used for their detection. The subject of wounds and personal injuries is a most important section, interesting and valuable. In this section will be found a brief notice of spectral analysis as applied for the detection of blood-stains. In this, the American edition, the editor has thought it advisable to restore some portions of the matter omitted by the author; he has also introduced some material from the Principles and Practice of Medical Jurisprudence, thus rendering the volume more complete, and affording an opportunity of reference on certain points, to those to whom the larger work is not accessible. These additions are in the articles on noxious animal food, trichiniasis, sexual malformations, insanity as affecting civil responsibility, suicidal mania and suicide, and life assurance. The notes of the former editor, Dr. Hartshorne, have been retained, and there will be found several decisions by American courts of law which add increased value to the work. The typography is excellent and the paper superior, the publishers having done their part most creditably.

An Introduction to Practical Chemistry, including Analysis. By JOHN E. BOWMAN, F.C.S., late Professor of Practical Chemistry in King's College, London. Edited by Charles L. Bloxam, F.C.S., &c., &c., with one hundred and seven illustrations. Fourth American, from the fifth revised London edition. Svo. pp. 351. Philadelphia: Henry C. Lea. 1866.

This little book is what its title declares, a hand-book of Practical Chemistry. It is a work intended simply for use in the laboratory, hence it will be found that all theoretical explanations have been excluded.

The editor has thought proper to omit all symbols and equivalents, with a view of inducing the student to work out the equations for himself, and not rest satisfied by finding them on the page before him; also, because of the limited space which he has allotted himself, which would, in many cases, prevent the explanation being thoroughly given. The article on the mouth blow-pipe has been carefully re-written. There will likewise be found a systematic course for examining unknown substances with the aid of the blow-pipe. In the appendix will be found tables to serve as a guide for the rapid analysis of simple substances. This little work is an admirable companion for those desirous of following a course of Practical Chemistry. It is illustrated throughout with wood engravings.

PERISCOPIC DEPARTMENT.

Surgery.

CASE OF A LARGE CYST IN THE NECK, WHICH OPENED INTO THE PHARYNX.

By WILLIAM S. SAVORY, F.R.S., Assistant-Surgeon to St. Bartholomew's Hospital.

On the 2nd of last March, I saw, with Mr. Pettifer and Dr. Savory, a tall, slim man, with light hair and sandy whiskers, sixty-two years of age. A large tumour occupied the whole of the right side of the neck. It extended from the mastoid process above to the sternum below. It passed back into the posterior triangle, and forward across the mesial line, pushing the larynx and trachea far over to the left. It was not very tense, and resisted pressure unequally in different parts. About its central portion, which was most prominent, it was soft and fluctuated, and there percussion elicited tympanic resonance, so that it obviously contained air. The cavity of the tumour did not seem to be far from the surface; but it was clear that it extended very deeply into the neck. It moved with the larynx, as in swallowing, and when grasped by the hand, could be drawn to and fro.

The patient told us that it had existed for many years—for more than thirty at least. It was first noticed by his wife, whose attention one day was attracted by the swelling in the neck. Its origin was therefore obscure. There was nothing to show that it had arisen either in connexion with any special natural structure, or upon any previously existing abnormal one. Lately it had increased rather rapidly, and embar-

ressed the respiration. He told us that it had been tapped many years ago, and some fluid let out. About a week before I saw him a considerable quantity of abominably fetid puriform matter passed into his pharynx and mouth, which he said almost choked him, and after this discharge the tumour became unusually flaccid. But in a short time it seemed to refill, and again and again a profuse discharge of frothy, fetid, puriform matter recurred. For the last day or two he had been frequently hawking up this stinking matter, which poisoned the whole room. His breath, too, was intensely fetid. He could only lie on the right side.

He had been very deaf for many years, but was an intelligent man, and gave a very clear account of the case. Until lately his general health had been good, and he cared little for the tumour until it began to embarrass his breathing: but since the fetid discharge his strength and spirits had given way, and he then felt very ill, and began to despair. In fact, at this time his health was not only being rapidly destroyed by the poisonous discharge, but his life was directly threatened by the sudden way in which, from time to time, it escaped into the fauces.

The tumour was undoubtedly one of those large, simple, watery cysts which have been called hygromata or hydroceles of the neck. It had evidently opened into the throat, probably into some part of the pharynx, perhaps into the side of that cavity, just above the glottis. It could hardly have communicated directly with the windpipe, because, although when his mouth was suddenly filled there was much distress and a sense of choking, yet the symptoms were not so urgent as they must have been had the matter passed directly into the larynx or trachea; and moreover the matter was spat out without much cough or evidence of expectoration. At this time he could not bear the introduction of the laryngoscope.

We all agreed, however, that something must be done to relieve him, and that the case admitted of no delay. Removal of the cyst by dissection was out of the question, and even had it been practicable such an extensive operation would not have been, in my opinion, justifiable, because it appeared to us that a very fair prospect of success was offered by a much simpler measure. I proposed first to puncture the tumour, to be quite sure of its nature, and then to lay its cavity freely open by a simple incision. In this way the discharge would escape through the wound instead of into the pharynx, and thus the most urgent and distressing symptoms would be at once relieved, and then there was every chance that the cyst would gradually contract and disappear.

So on the 6th of March the cyst was laid open. Our patient had lost ground considerably since we had last met—poisoned, I suppose, by the

fetid fluid which formed in and escaped from the sac. I cannot describe the horrid stench which at this time accompanied the now almost constant hawking-up of the matter. He had spat up much blood too, as well as pus; sometimes as much as a quarter of a pint at once, of the two mixed in about equal proportions, and then the tumour, though in some parts only, for a short time seemed shrunken. This suggested the possibility that the cyst might be multilocular.

I first punctured the cyst with a grooved needle where the wall seemed thinnest and it was most resonant—that was in its most prominent portion near the centre; but only some dark, tenacious, evidently stale blood escaped. Then I made a cut, an inch and a half long, over the spot, and dissected down cautiously to the cyst. It lay at some distance from the surface: not only the platysma, but also the sterno-mastoid, was spread over it. The sac being exposed, I punctured it with the knife, and immediately black grumous blood and air flowed out. The aperture was enlarged, and gave vent to a considerable quantity of stinking black blood. The odour was almost intolerable. I introduced my finger into the cavity, and found it to be very extensive and irregular. It occupied nearly the whole of that side of the neck, reaching upwards and backwards to the mastoid process, downwards to the sternum, and forwards, across the neck, to the larynx and trachea. Far on the opposite side at one spot in front and above I felt what I fancied was the aperture which communicated with the pharynx. Much more rotten blood and pus were turned out, and then, separating the edges of the wound, which was enlarged upwards and downwards for another inch, we obtained a fair view of the interior of the sac, which was gently wiped out with small sponges. On the opposite wall, portions of a dark slate-coloured and shrunken membrane appeared. This was cautiously drawn forward with a pair of forceps, and recognised as a dead and putrid portion of the cyst. Only a part of this could be drawn out by very gentle traction—for, doubtful of the structures to which it might be attached, we avoided all violence,—and this was cut off. When the cavity was cleansed, nothing more could be detected than the remains of the cyst-wall. One or two arteries bled freely in the tissues in front of the cyst, but they were readily secured. Some strips of dry lint were introduced into the cavity, and the patient—who had sat in a chair, and borne the operation, without chloroform, with remarkable equanimity, becoming, however, rather faint once for a moment—walked back to bed, and lay on the right side with the aperture dependent. It was observed that after the operation the trachea and larynx returned at once almost to the mesial line,

All went on well without any interruption. He felt instant relief from the diversion of the course of the discharge, and rallied rapidly. The horribly offensive odour quickly diminished, and was further restrained by the use of Condy's fluid, with which the lint was saturated before it was introduced. The rest of the dead cyst-wall came away in two or three large pieces. The discharge gradually became odourless pus. The cyst contracted, and at length (a month after the operation) closed, leaving only a linear scar with a very limited amount of thickening about it, which could be covered by a florin. He is now quite active, and his health is excellent. Nothing abnormal can be discovered by an examination with the laryngoscope.

This case is remarkable, not only for the great size which the cyst attained and the consequent displacement which it effected of the trachea and larynx and other adjacent structures, but most of all for the communication which had been established between the cavity of the cyst and that of the pharynx. So far as the cyst itself only was concerned, and had the patient's life not been threatened by suffocation or poisoning, this would probably have led in time to a natural cure. It was certainly a strong hint and encouragement to make a counter opening in the manner described. For while by this means there was every prospect of averting immediate danger, there seemed, considering the changes which the interior of the sac had already undergone, little or no risk of producing either local or constitutional disturbance by the operation; on the contrary, there was the best chance of reducing both.

These cysts in the neck have occasionally given way spontaneously. Thus Mr. Phillips* has related a case in which "a small crack took place in the covering of the tumour, and above three pints of a reddish serous fluid escaped." This was followed by a sero-purulent discharge for many weeks; but "a small fistulous communication with the sac remained." A case is also related by Wernher,† in which a congenital multilocular cyst opened externally by several apertures, and, gradually contracting, was at length obliterated.

But among recorded cases, I can find only one which opened in a similar manner to this. Fortunately, the specimen is preserved in the museum of St. Bartholomew's Hospital. It is thus described:—

"A larynx, pharynx, and adjacent parts, with the thyroid gland. The right lobe of the gland is enlarged by the formation of a cyst, of more than four inches in diameter, in its interior. The walls of this cyst

* Medico-Chirurgical Transactions, vol. xxv.

† Die Angeborenen Cysten-Hygroeme. Giessen, 1843.

appear to be formed by the distended tissue of the gland; its interior is rough, and has a large quantity of lymph deposited upon it, some of which hangs in it in loose shreds. At its upper part, the cavity of the cyst communicates with that of the pharynx by a narrow ulcerated aperture (indicated by a piece of glass) near the arytenoid cartilage. The isthmus and left lobe of the gland are healthy.

"The patient was an elderly woman, and the enlargement of the gland had long existed. The cyst at first contained a fluid like serum, which, when withdrawn, spontaneously coagulated. After being twice emptied, the walls of the cyst inflamed, and it was rapidly filled with pus and lymph; its wall ulcerated, and the ulceration extending through the adjacent part of the pharynx, the patient was suffocated by the sudden discharge of its contents, and the passage of some of them into the larynx."

It is probable, from the reasons already given, that the cyst in my case opened somewhere about the same spot. Such an occurrence must be very rare.—*Lancet*.

Brook-street, Nov., 1866.

REMARKS ON THE USE OF THE ENDOSCOPE.

By HENRY DICK, B.A., M.D., Surgeon of the National Orthopædic Hospital.

There are some remarks in *The Lancet* of October 20th on the use of the endoscope, by Henry Thompson, which left an impression on my mind, and very likely affected others similarly, that Mr. Thompson is not an admirer of the endoscope, nor very sanguine as to its future usefulness. I can pretty well understand the uncertainty which Mr. Thompson seems to feel on the subject, because I myself gradually passed through the same state of mind.

About fourteen years ago, I began to work with the endoscope, and I had several conversations on the subject with the late Mr. Avery. With M. Desormeaux's instrument I did not succeed at first, because I used spirit of wine instead of gazogene. When I used gazogene the light was better. At the same time I found another inconvenience in the great pain which was produced in the urethra by the sharp borders of the tube, but when I used blunter tubes that inconvenience was obviated.

The results of my endoscopic researches nevertheless were not always satisfactory. Sometimes I could very well distinguish the colour of the urethra, and diagnose its condition, but at other times I could not get a good light at the desired spot, except by moving the instrument in different directions; and after many trials I came to the conclusion, that the essential thing for success in endoscopic researches is to get the rays of light well reflected on the spot we are desirous to examine. I there-

fore prefer Dr. Cruise's instrument to Desormeaux's, not only because the light is more brilliant, but because by his ingenious contrivance we can either raise or depress the tube, or give it a lateral direction. If we succeed in getting the light on the proper spot—even the light of an ordinary sperm candle is sufficient—a distinct view of the object will be obtained by means of the endoscope. A precaution I think it necessary to take is to have the endoscope placed on a stand, the height of which should be capable of alteration, as the involuntary motion of the hand renders the light uncertain. In fact, there are many precautions which it is necessary to observe, the omission of any one of which, though apparently of little moment, is quite sufficient to frustrate the desired result. Amongst them I found it desirable to have tubes constructed, with their side-openings capable of being closed, so that, when required, all the light can be concentrated on one spot, without any loss of light through the side-openings.

I quite coincide with Mr. Thompson's remark about *chronic inflammation* in the bulbous portion of the urethra. That affection was known long before the endoscope was in use; but we were in the dark as to the *extent* of that inflammation. Besides it is not only the bulbous portion of the urethra which can be affected by chronic inflammation, but also the fossa navicularis can be so affected; and in three cases I found chronic inflammation two inches and a half down the urethra, and which I was only able exactly to diagnose with the aid of the endoscope. The touch alone is not sufficient for the diagnosis, because some parts of the urethra may be very sensitive when no chronic inflammation exists; and I have found, in *old cases* of chronic inflammation, pain in sounding is either absent or very trifling.

The term "granular inflammation" of the urethra does not seem to me one which we ought to accept; my reason for which I have stated in the preface and notes in the second edition of my monograph "On Gleet."

The orifice of a stricture can certainly be seen by the endoscope. It presents itself to the observing eye as a very small black spot; and may be overlooked by a *tyro*, or not found at all, because the end of the tube is directed towards the walls of the urethra. To be successful, we must by a little manœuvring bring the opening of the stricture into the centre of the tube, as we have sometimes to do with the speculum in order to get a good sight of the opening of the os uteri.

I consider the endoscope a very useful instrument in impassable strictures. In a case of Civiale's, where no instrument could be passed, Desormeaux with the help of the endoscope succeeded. That the orifices

of the ejaculatory ducts cannot be seen is to be ascribed to their anatomical position. If we could expand the tube at that particular spot, and dilate that portion of the urethra, I have no doubt the ducts could be seen ; but I fear the sudden dilatation of that particular spot would produce orchitis.

In all diseases of the bladder, the future of the endoscope is bright, and I am in great hope that the mysterious sac called the "*irritable bladder*," into which so many affections of the bladder have been thrown, and still is thrown, will be well lit up by the endoscope, to the great benefit of humanity and science.

There is not the least doubt that a foreign body can be seen in the bladder by the endoscope. Dr. Cruise has proved it in the presence of witnesses, and Desormeaux further actually gives exact drawings of stones in the bladder, as seen through the endoscope. The "*tactus eruditus*" of Mr. Thompson is certainly necessary, but the *oculorum acies* is still better in the diagnosis of disease. In fact, it is my conviction that the endoscope will be more useful in disease of the bladder than in that of the urethra, because a larger space for observation comes under the range of the instrument, and sooner or later lenses will be constructed to improve its practice and extend its usefulness.

Mr. Thompson's opinion about treating strictures by flexible bougies is very valuable, coming from such an authority ; but it does not agree with my own experience, as I have found the metallic bougie more serviceable. But my own experience would be of little weight if it could not be proved that the pathological anatomy of stricture of the urethra shows conclusively that the metallic bougie is the one indicated.

Lest I should be misunderstood, I should state that for passing a difficult stricture I prefer the finest and most flexible bougie ; but for *treatment* I adhere to my above-mentioned opinion.

The presence of the tube of the endoscope cannot be more irritating than the sound ; indeed I have always found it much less so, because the tube only penetrates a short distance into the bladder, and does not come in contact with its walls when in its injected state.

Mr. Thompson is mistaken when he says that we cannot make use of the endoscope in operating in the urethra. Desormeaux cuts strictures with the help of the endoscope, and I have removed a small polypus through its instrumentality ; and in cases where only a very small portion of the urethra is affected by chronic inflammation I cauterize by its assistance.

But the last remark of Mr. Thompson about the use of the endoscope in disease of the rectum is very encouraging, and I am sanguine that the

time will come when he will discover its equal usefulness in diseases of the bladder and urethra; because it cannot be logically admitted that if the endoscope can reveal a deep seated affection of the rectum, it should not render equally valuable services in diseases of the urethra and bladder.—*Lancet.*

ON A CASE OF LATENT PLEURISY; EMPYEMA; PARACENTESIS THORACIS; INTRODUCTION OF A DRAINAGE-TUBE.

By ANDREW DUNLOP, M.D., Resident Clinical Assistant, Consumption Hospital, Brompton.

As the employment of the drainage-tube in the treatment of empyema is not much resorted to in this country, the following case, which was under the care of Dr. Hamilton Roe (who has kindly permitted its publication) may be found interesting. To the kindness of my predecessor, Dr. Powell, I am indebted for notes of the case during the first two months and a half that it was in the hospital.

J. S—, aged twenty-four, was admitted into the Consumption Hospital on Jan. 29th, 1866. He had enjoyed excellent health until December, 1864, when his illness commenced with pain in the right side, and cough. On admission, his symptoms were, cough, dyspnœa on mounting a stair or on walking more quickly than usual, and general weakness. On examining his chest, it was found that the right side bulged laterally, and that it was almost motionless, while its circumference was $20\frac{1}{2}$ inches that of the left being 18 inches. Vocal fremitus was absent all over the right side except at the apex, and there was intercostal fluctuation. There was absolute dulness on percussion all over this side, anteriorly and posteriorly, extending, in front, slightly beyond the right border of the sternum. The respiration was distant all over the right side anteriorly and posteriorly, and the vocal resonance was diminished, though not markedly so. On the left side there was supplementary respiration, and the percussion note was good. The apex of the heart was beating in the seventh left interspace, about three inches outside the line of the nipple. The impulse was diffused, and at the apex there was a soft, blowing murmur.

As the effusion was so great, and, so far as could be ascertained, of such long standing, Dr. Roe considered that paracentesis was necessary, and on Feb. 9th the operation was performed by Sir William Fergusson, who withdrew 170 ounces of serous fluid mixed with a considerable amount of pus. After the operation the apex of the heart was found beating half an inch outside the mammary line; and there was comparative dulness all over the right front and lower half of the right

back, with soft distant respiration. The circumference of the right side was now $19\frac{1}{2}$ inches.

By the 27th of February the fluid had again accumulated in large quantity, and as the patient was suffering from great dyspnœa, Dr. Roe once more had recourse to paracentesis, and removed 150 ounces of sero-purulent matter.

For a week or two after the operation the patient did extremely well, and hopes were entertained that no further accumulation would take place; but it soon became evident that fluid was once more being poured out, and in the beginning of April the signs of extensive effusion were as strongly marked as when he was admitted. Dr. Roe now resolved to consult with Dr. Goodfellow, who has had extensive experience in the use of the drainage-tube, as to the advisability of adopting that mode of treatment in this case, as it seemed the most likely means of affording permanent relief. When the consultation took place, on April 9th, it was agreed that a tube should be introduced, and that the upper opening should be made at once, and the lower one a few days later. Accordingly a medium-sized trocar was introduced, and on its withdrawal a few drops of thick purulent matter escaped by the canula. A gum-elastic catheter was then passed into the pleural cavity, and after some flakes of tenacious lymph had been withdrawn which were blocking up the tube, about 60 ounces of purulent fluid escaped in a slow and frequently interrupted stream. The canula was then left in the wound, its aperture being closed by a wooden plug. A day or two after this operation an attack of bronchitis came on (from which he did not recover for about three weeks), so that the introduction of the drainage-tube had to be postponed. Meanwhile the plug was removed from the canula every day or two, allowing at first only a few ounces of sero-purulent matter to escape, but the discharge gradually increased, so that in two or three weeks from ten to forty ounces were withdrawn nearly every day, and there was besides an almost constant and sometimes copious flow from the wound, around the canula. At first it was nearly inodorous, but it soon became extremely fetid, tainting the air of the whole ward. To destroy the fetor an injection of four ounces of tepid water, containing a little of Condyl's fluid, was thrown into the pleural cavity every other day.

In the beginning of May the bronchitis had almost entirely disappeared, but marked hectic had set in. His complexion, when not coloured with a bright flush, was sallow and dingy; his eyes were sunken and surrounded by a dark areola, and he had copious night-sweats. The introduction of the tube had still been put off, as it was thought he might

improve somewhat, and be better able to bear the operation; but it soon became evident that he was becoming exhausted, and that further delay would be inadmissible. On May 24th he was placed under the influence of chloroform by Mr. Clover, and Sir William Fergusson passed in the drainage-tube.

From this time he gradually improved; the hectic diminished, his appetite returned, and the discharge lost its fetor and decreased in quantity till it amounted to little more than five ounces in the twenty-four hours. The injection was still continued. In the end of June he began to get up for an hour or two each day, his appetite became excellent, and he felt himself getting rapidly stronger. In another month he felt himself so much better that he resolved to leave the hospital and go home to Devonshire; and, as it was thought that he would be benefited by the change, he received his discharge on the 1st of August.

When he left, the discharge was almost entirely free from odour, and amounted to about four ounces in the twenty-four hours. The right side of his chest was hyper-resonant on percussion, and the respiration was inaudible; on the left side there was a full-toned percussion note, and exaggerated respiration. The left lung extended nearly to the right border of the sternum.

It is probable that in this case the effusion was gradually becoming purulent when the patient was admitted, for the first fluid that was withdrawn contained only a small proportion of pus, and had it formed earlier doubtless its presence would have been indicated by hectic and constitutional irritation.

It is generally in cases of latent pleurisy, where there are few, chest symptoms and little constitutional disturbance, that we find the most extensive effusions, and it is in these cases that paracentesis is specially indicated. To Dr. Roe* and to Dr. Hughes† is due the merit of having revived this practice in England. The advantage of the drainage-tube in ensuring a free exit for the fetid, purulent fluid, was well shown in this case.

BOILS AND CARBUNCLES.

By TILBURY FOX, M.D. Lon., Physician to St. John's Hospital for Skin Diseases.

The points involved in Mr. Startin's letter are of so much pathological interest, that I hope I may be allowed space for a few comments. Mr. Startin's therapeutical experience is entitled to the profoundest respect;

* Medico-Chirurgical Transactions, 1844.

† Guy's Hospital Reports, 1844.

but the explanation which he has given of the cause of boils and carbuncles is scarcely that which *modern* pathological observations would seem to indicate. Unfortunately, empiricism of the rankest and most tyrannical kind has held its sway for many a long day over cutaneous medicine; and no one (since Carswell's day) specially conversant with the facts of general pathology has thought it worth while to study the subject; yet, unquestionably, the philosophical study of skin-diseases is pregnant with results of great general significance, and the case of carbuncles is fully illustrative of this fact.

Mr. Startin views boils and carbuncles as having "frequently or constantly a parasitic origin;" and he bases his belief upon the facts, (1), that they are sometimes contagious; and (2), the success and efficiency of the practice in the cure of these ailments, rather than on microscopic verification: in other words, on the occurrence of *occasional contagion*, and the *beneficial action of acid nitrate of mercury*. One word will suffice in reference to the second argument. Acid nitrate of mercury, in virtue of its *caustic* properties, removes a host of ills—lupus, acne, warts, cancerous masses, and other diseased structures the most dissimilar. Are they then parasitic? Mr. Startin's chief ground for his belief in the parasitic nature of boils and carbuncles is the occurrence of contagion. This, however, is only occasional; and, considering the absence of all relative proportion between the amount and kind of the local diseased action and that observed in parasitic maladies; the absence of parasitic growth in the vast majority of cases; the fact that fungi will but very scantily develop in purulent fluids; the absence of any aperture by which the fungus-germs could enter from without into the cellular tissue; the non-access of air; and the want of relation between the amount of tissue-change and that of the fungus when present,—the unlikeliness of its parasitic nature is evident. And, if we seek amongst the parasitic diseases of animals, of human beings, or of plants, we shall not find any analogical grounds (nay, just the contrary) upon which to rest such a belief. Again, the constitutional conditions antecedent, accompanying, and following the local changes, in relative proportion to the extent and character of the latter, are not seen in any parasitic disease. The occasional presence of vegetable parasites is common to all diseases. Parasites are essentially ubiquitous, and they may be found in almost all skin-diseases; it is only when they *luxuriate*, that they give rise to *special* mischief. It is fashionable to ascribe too many diseases to the influence of parasites.

How, then, explain the contagion of boils? for they seem to be occasionally contagious. What mean we by contagion? The labours of all

pathologists seem to show that it is essentially connected with the growth of living particles of matter, detached from living bodies, and carried to others—of course, under favouring circumstances. Occurrences of the kind are universal in the vegetable kingdom, and there seems no reason why animal cells should not be transplanted and grow as well when isolated as in masses; and they do so. The cells in the secretion from a leprous sore, from Egyptian ophthalmia or the mucous surface of a rinderpest cow, cancer-cells, the pus-cells of syphilis and of small-pox, and, from recent observation it seems likely, tuberculous cell matter, all possess this faculty; and, to take another example, in the case of molluscum, the cells found in the little "varioliform" tumours are the means by which, being transplanted from person to person, the occasional contagion of molluscum is to be explained. One feature that is necessary in all these cases is the presence of free proliferation on the part of the cell-growth, and an adapted state of nutritive fluid (blood). In the active and early stage of boils, the cells of the enlargement may, no doubt, be removed from one body to another, and, growing under favourable circumstances, reproduce the original disease. Why not? What law would this contradict? Dr. Laycock's cases of contagious furunculoid are explicable upon the same ground.

Contagion is scarcely a distinctive feature of any one disease; the degree of contagion no doubt is. If it were possible to transplant an alphos scale, and it were to grow and produce alphos on a second subject, there would be no great mystery in it; it would harmonize (though an unusual occurrence) with true pathological facts, and be contagion in one sense of the word. There is nothing improbable, but probable, to say the least, in the supposition that the cell-growth in a boil may be the means by which the disease is rendered "contagious." In carbuncle, there is a good deal of superadded inflammation, and a tendency to gangrenous change, which, implying a tendency to the death of the cell-tissues, is accompanied by a very much less likelihood of contagion.

But what is the pathology of boils and carbuncles? We may assume that in kind it is the same; the difference between the two diseases is dependent upon (1) variations in the vigour of constitution, (2) the state of the nutritive fluid (the blood), and (3) the activity of the local tissues. In the central part of boils and carbuncles are one or more pieces of dead tissue, sloughs, or cores. How is the tissue killed?—by arrest of the circulation, or failure of nutrition? What has been noted about the blood? Three very important sets of facts: 1, bacteridia oftentimes in great amount; 2, excess of urea in the urine, and uric acid in the blood; 3, diabetes. Bacteridia, however, seems to be developed only secondarily.

and to be unable *per se* to produce furuncle. The excess of urea, and uric acid can scarcely be said to be the cause of carbuncle and boils; and we come to the third condition, noticed by Cheselden, Prout, Latham, Landouzy, Marchal de Calvi, and others—viz., a tendency to, or actual, diabetes. Dr. Wagner has given details of fifty-two cases of gangrenous inflammation, including carbuncles and furuncles, in which a diabetic condition existed; and M. de Calvi has confirmed Wagner's observations. My own observations on this point are small; but I am convinced that, if we would clearly understand the true pathology of carbuncle we must carefully investigate the matter in connection with the production of sugar in the system. The existence of a diabetic habit explains satisfactorily the fatality of carbuncular disease, and the serious constitutional disturbance. Nothing is more common than for carbuncles to arise in the course of diabetes; and it will be remembered that Cardinal Wiseman suffered for no less than four years before his death with carbuncles. More recently Dr. Fonseca, of Pernambuco, has investigated this subject; and he tells us that in Pernambuco anthrax is very common, and that one of its forms is regarded as diagnostic of diabetes. Kuchenmeister, Menestrel, and Jordan of Lisbon, have also given similar evidence.

And at this point Mr. Startin's therapeutical experience comes in to confirm the theory I have briefly sketched. He finds successful treatment in the use of *aperients*, *animal* diet, *tonics*, and free stimulation without malt liquors. The avoidance of all saccharine and amylaceous matter, is an essential point; but I venture to affirm that, of all drugs, opium, judiciously used, is the most important. Clinically, I know that it has cured, and does help to cure, carbuncular inflammation, when other things fail: and therefore, if we add to Mr. Startin's recommendations the use of opium, we shall be in possession of a plan of treatment which is not only empirically dictated by the largest experience, but consonant with the most recent truths which pathology has taught us. The acid nitrate of mercury acts well, of course, as a *caustic*.

There are many other points—the origin of the local mischief especially—that I would like to notice; but I have only attempted to indicate that there is a much truer explanation than the "parasitic" hypothesis as to the cause of carbuncle, involving very wide pathological considerations. Skin-diseases have been so long handled from a *surgical*, that it is a novelty indeed for any one to investigate them from a purely *medical* point of view, and to trace connection between them and such a profoundly subtle disease as diabetes; but I again reiterate the remark I have elsewhere made, that "the physician must be possessed of all that general

medicine can teach before he can become the successful dermatologist."—
British Medical Journal.

Medicine.

SUGGESTIONS FOR THE TREATMENT OF CHOLERA.

By ARTHUR ERNEST ŠANSOM, M.D.

ACCORDING to my views, cholera is the result of a poison which manifests its effects in two different ways, accordingly as it is in immediate relation with the intestinal canal or absorbed into the circulating blood; as a primary intestinal irritant it may cause the "immediate" phenomena of vomiting or purging, and as an absorbed poison it may ultimately manifest the "deferred" phenomena of central irritation of the great sympathetic nerve, and cause the combined symptoms of true cholera-diarrhœa and collapse.

I. ORDINARY CHOLERAIC DIARRHŒA AND THE PREMONITORY DIARRHŒA OF ASIATIC CHOLERA.

I think every grain of evidence is in favour of this being caused by a direct irritation of the intestinal canal, whether due to the contact of irritating organized material with the mucous membrane, or whether arising during the elimination of the germs from the circulating blood.

It is one thing to suppose that the germs of cholera in the process of their elimination by the intestinal surface excite diarrhœa; it is another to suppose that this diarrhœa has a directly curative tendency. The amount thus eliminated can bear but a small proportion to the amount diffused throughout the mass of the blood. We cannot stop fermentation by diminishing the bulk of fermenting material; nor can we arrest the course of a zymotic disease by draining the poison which must exist in every capillary of the body through the mucous membrane of the alimentary canal. These observations, of course, apply only to the absorbed poison; if the poison merely exists as a local irritant on the mucous surface, the purgative plan must be directly curative. But even then is there a better course? I think so. It is more feasible to kill the germs than to remove them, for in their removal the elements of nutrition must, *pari passu*, be eliminated.

We come next to the consideration of those bodies which have the power of specifically altering organic or destroying organised matter. Let me insist on the essential difference between these classes of agents. The first exerts no especial action upon living matter other than that

which it manifests upon organic. Permanganate of potash oxidizes every organic body with which it comes in relation. It would be hopeless to administer permanganate of potash with the idea of decomposing the organic poison existing in the body, for its action would likewise be exerted in the intestinal tube itself as well as its contents, whether organized or not. The researches especially of Mr. Crooks show carbolic acid to be a type of the other class—a class which does not destroy or chemically alter organic bodies, which does not interfere with chemical processes, but which destroys organized bodies, animal and vegetable, and stops zymotic change. Is it not more hopeful to render inert the *materies morbi* by such an agent as this than to attempt the herculean task of sweeping away the germs which mingle with every drop of blood to

“Cleanse the stuff'd bosom of that perilous stuff
Which weighs upon the heart?”

Mr. Crookes's researches show that carbolic acid destroys the vitality of animal or vegetable cells, and a large amount of evidence is brought forward to the effect that it has had a directly destructive effect upon the poison of cattle plague. If we glance for a moment at those medicines which have been reputed efficacious in cholera, we find that many of them have a powerful action on organic matter. Calomel is antiseptic as well as purgative, and its tendency is to induce a flow of bile, which is Nature's own disinfectant. Creosote has been recommended, and that is an undoubted antiseptic. The compounds of sulphurous acid (*e. g.* the sulphite of lime, to which attention has been lately called) have a power of disinfection which it is not necessary here to recapitulate. Turning to the analogy of another disease, we find that there is no more successful plan of treating diphtheria than by the sulphites recommended by Professor Polli. I have just under my care a case in which sulphite of soda has been most demonstratively successful. Again, we cannot help noticing the success of oxidizing agents in this disease. Nitrate and chlorate of potash are directly oxidizing agents; perchloride of iron is an oxidizing agent—that is to say, in the presence of organic matter it gives up its chlorine, which unites with the hydrogen of water and liberates oxygen. Chlorination in the presence of water is oxidation. I have adduced these considerations to show the probability that some of those medicinal agents which have been used empirically may have been in reality actual destroyers or decomposers of the *materies morbi*.

The treatment of cholera, by doses of carbolic acid has been tried, and though it is as yet *sub judice*, the reports from the *Belleisle* Hospital

hip states that it would appear that "the carbolic acid system has been most successful." The advantage of carbolic acid is that it may not only be administered by stomach and bowel, whereby it acts upon the organic matter in the alimentary tract; but it is most undoubtedly absorbed into the blood, and, being a volatile body, its vapour can easily be introduced by the lungs. The castor-oil treatment, though I do not think it possesses the merits of the disinfecting plan, has done much for the treatment of cholera. Dr. Johnson has enticed Medical Practitioners from the beaten track of astringents, and chalk, and opium—measures which check the process of elimination, and which cause retention, with all facilities for active fermentation and decomposition, of vitiated matter. Whether there were hypersecretion of bile or suppression thereof, the treatment used to be the same—opium and brandy. Opium, which may arrest the perhaps already feeble flow of bile; and brandy, which may contract the narrowed arteries, and force the current of blood back upon the veins.

From the foregoing facts what shall we educe as the most rational treatment for choleraic diarrhœa during the prevalent epidemic?

1st. Thoroughly disinfect the chamber and the house with carbolic acid or McDougall's powder.

2nd. Let the patient be placed at perfect rest, horizontal, with draw-sheets, so that the discharges may be frequently removed. All soiled linen should at once be placed in a vessel containing carbolic acid solution.

3rd. Commence the treatment with the following:—℞ Acidi carbolic. gtt. ij., chloroformi miiij. mist. acaciæ ℥j.—to be taken every two or three hours.

The carbolic acid used, which at ordinary temperatures is a white crystalline solid, should be liquefied by the addition of a few drops of water. The presence of a very minute amount of water is sufficient to reduce the solid to a liquid. A piece of carbolic acid will melt down under the influence of the moist breath.

The objection to carbolic acid is its odour, which resembles that of tar. The presence of the chloroform (which acts locally on the nerves of taste, masks this in great degree, but it may happen that in certain cases this remedy cannot be applied. Under these circumstances, and especially in cases of children, it will be well to use sulphite of soda.*

I think sulphite of soda is better than sulphite of lime, as the latter so soon becomes oxidized to sulphate of lime, and it is well not to administer the lime salts until the germs be, if it be possible, rendered inert.

* ℞ Soda sulphitis ʒ ss.; æquæ ℥j. M. Every two or three hours.

4th. The diet, while the absorbing powers are yet active, should consist of beef-tea thickened with isinglass or arrowroot. By being thickened, irritation of the stomach by it is much lessened, and small quantities may be retained in spite of vomiting. Stimulants *purs et simples* should never be given, but mingled with food they may be administered in small quantities—thus, a teaspoonful of brandy with a wine-glassful of milk and the white of an egg may be given every three or four hours.

II. TRUE ASIATIC CHOLERA—COLLAPSE.

Supposing that the fore-mentioned means have been tried, and yet the special symptoms of cholera set in—supposing coldness and cramps and the signs of incipient collapse occur—what are the indications?

The cause, I firmly believe, is an union of the poison with the sympathetic. It may be that the system is so overburdened that nothing can lift the load; * but what plan of treatment offers the best chance?

1. *Counter-irritation of the Epigastrium.*—I have often seen the value of heat employed to the epigastrium in relieving the symptoms of collapse. I have employed it in cases of chloroform administration wherein there have been signs of syncope. Derivation from the solar plexus seems *à priori* likely to do good. What is the best form of counter-irritation in these cases? The carbonic acid, which is close at hand, offers itself. Let it be rubbed over the pit of the stomach for a short time by means of a piece of flannel. Dry cupping to this region may be applied with advantage. Subsequently to the counter-irritation, warmth by means of hot water bottles or hot salt bags should be applied.

2. *Keep the Patient in perfect Rest.*—The stomach should be spared fruitless efforts to exhibit nourishment. It is true that in collapse the powers of absorption are not annihilated; but it must be remembered that absorption is, in fact, a mechanical act (dialysis), and that though mere absorption may take place in this way, still vital transmutation is necessary to make the absorbed material of any avail for nutrition; and this assimilating power is wanting. No medicine and no food should be

* Cases have occurred in which symptoms of collapse have happened without previous warning, and have continued with fearful rapidity until death. A few days ago a man was observed to fall down in the street. He was taken to an hospital, and died almost immediately. At the post-mortem examination the appearances peculiar to cholera were noticed in the intestinal canal, and there was no other lesion to account for death.

given by the stomach, but enemata of warm water may be administered with the hope, not of supplying heat, but of diluting the thickened blood. The best way of administering the enema is by using a siphon tube proceeding from a vessel placed at a convenient height, or the douche enema, made for me by Messrs. Francis, Upper-street, Islington. By this means a continuous and equable flow is maintained, and the saltatory jet and the frequent mess of an ordinary enema-syringe are avoided.

It has been proposed to transfuse blood or a fluid analogous to it into the veins, and in some instances this practice has met with at least a temporary success. The warm fluid dilutes the thickened contents of the venous system, promotes a flow in the capillaries, then reaches the minute contracted arteries, and distends them. Motion is renewed, and motion is life. It is at first sight very strange that two such opposite courses as transfusion and venesection should have been of equal benefit. But if we consider that the symptoms are due as well to diminished arterial supply as to retention of products which should be excreted, as well to arterial anæmia as to venous engorgement, we may understand the cause. The veins in the case of bleeding being lightened of their load, the excretory products which had narcotized the system being in part removed, respiration and aëration return, and the column of blood moves. In a patient suffering from syncope, motion of the blood is of the first importance for reanimation. If we tilt the feet, so as to allow the column of blood to fall back upon the heart, the failing circulation is rapidly restored. Again, in cases of threatened death from suffocation, it is only when the current of blood is set in motion that the symptoms of danger pass off. In a case of collapse, in which the arteries are nearly empty and the veins over-full, motion of the blood can only be induced in two ways—either by venesection, which allows an escape from the distended right side of the heart, or by forcing a stream *à tergo* from a vein. Either or both these means may be tentatively employed, but neither should be adopted unless other means are found to fail.

If there be any mode of relaxing the contraction of the arteries, this should be tried. I should think a fair trial should be given to inhalation of chloroform. This procedure has been known to relieve the cramps and to induce at least a temporary reaction. Combined with local warmth to the epigastrium and warm injections of the bowel, it may be yet more successful, and it certainly deserves a fair and careful trial.

—*Medical Times and Gazette.*

Midwifery and Diseases of Women and Children.

CASES OF POSTURAL TREATMENT IN PROLAPSE OF THE FUNIS.

By ROBERT DYCE, M.D., F.R.S. Edin., Professor of Midwifery, University of Aberdeen.

As the two following cases were so eminently successful in this but too fatal complication of labour to the child by this method of treatment, I send them for publication.

The first case was conducted by Dr. Thomas Milne, then a student attending my class in this University, and which most opportunely took place shortly after treating of this method. He reports as follows:—

“ Mrs. W., aged 27, second pregnancy; a healthy, rather stout, but well-made woman. When I first saw her she had been in labour for three hours. The membranes had ruptured some time before. The os uteri was fully dilated. The head presented naturally, and was partially through the brim, and hanging in the vagina, and projecting beyond the external parts, was a loop of the funis; it was pulsating feebly, which ceased during every pain. The pains were regular, and recurring about every ten or twelve minutes. I endeavoured to push up the cord in the interval of the pain, with the patient lying in the usual obstetric position, but failed; when remembering what you recently, in the course of lectures, had been describing of the success attending the ‘postural method’ as proposed by Dr. Thomas, of New York, I at once placed the patient on her elbows and knees, with the head and shoulders lower than the pelvis. I now pressed the head of the child a little up, and then steadily endeavoured to push up the cord; it passed away most readily, but during the next pain it came down as before. I again, in the interval of pain, put it up, but the next pain brought it down. The third time I passed my whole hand into the vagina, and carried the cord beyond the head; when the pain came on, I could only feel it with the tip of my finger, and when the pain left, the funis had slipped beyond my reach, and did not again return. The patient was kept in the same position until the child’s head was fairly in the cavity of the pelvis and nearly touching the perinæum. I then allowed her to take the usual position, and in about two hours the child was born alive, though rather feeble. Both mother and child have since done well.”

The second case of funis presentation was further complicated with placenta prævia.

Mrs. M., during her fifth pregnancy, in the latter months had three several sudden discharges of blood from the vagina—the first time during the night, the second time when dressing in the morning, and the third

time also in the morning the day before her confinement ; on all occasions the discharge stopped suddenly, and proceeded from no accident or cause on her part. The symptoms were suspicious of placental presentation, but as she wanted a good many weeks from her full time by her own calculation, and more especially as the loss of blood had produced no particular constitutional disturbance, I risked the uncertainty by not making any examination. When summoned on the night of her confinement (twelve hours after the last flooding), I was fully prepared for the announcement that the flooding had returned ; but on inquiry I was rejoiced to learn that the waters and not blood had come off—in fact, there was not a stain upon her linen—and that the liquor amnii had been discharged. On now making an examination, I was met by the funis, not merely a loop, but a mass which the hand could scarcely grasp. She had then no pain, but she had had during the evening some weak and distant indications of uterine action. The funis was very tense, and pulsated strongly ; the head could be felt through the os, which was open to the size of half-a-crown, and very dilatable. The external parts were also relaxed. I endeavoured to return the funis while she lay on her left side, but as fast as one portion passed up, another came down. Determining to try the “ postural method,” she was placed on her knees and elbows, the pillows being removed, when, with the slightest possible pressure, the whole mass of funis passed at once into the uterus. I observed also in this, as on the subsequent attempts, that there was no tenseness of the funis, as if the present position had removed some cause of pressure or obstruction, but when in the ordinary obstetric position the cord was tense and resilient when touched. Finding that on withdrawing my hand prolapse immediately took place, I determined to induce pain, hoping that the descent of the head would prevent its return. I gave her at intervals of ten minutes three several doses of a full teaspoonful of Battley’s liquor scalis. Strong pains followed the last dose. The postural treatment was then resumed, and the funis as readily replaced in the interval of pain as before, with the exception of a small knuckle, which seemed adhering near the cervix. The next pain brought it partially down, but on a third attempt, my hand being entirely within the vagina, the funis was passed beyond the head, which was now descending, and retained there with great ease. In another pain it slipped beyond my reach, and gave no further trouble : but still the little knuckle-like portion remained, and which I now discovered to be the placental extremity of the cord and the mass of placenta itself attached closely to the cervix. The patient was now allowed to take the ordinary position, and the child was born in fifteen minutes, strong and healthy.

This plan of treatment was originally proposed by Dr. Gaillard Thomas, of New York, in a paper published in the *New York Medical Journal* for March, 1858, and, although several years have elapsed since that time, the method proposed does not seem either sufficiently known, or, if known, is not appreciated, by the Profession. It seems very clear from the numerous methods proposed to remedy this complication, that no one of them can be depended upon for saving the child, as even in the ablest hands the mortality is fearfully great. Churchill states that practically the mortality is greater than in any other order of labour, more than half of the children in which the funis was prolapsed being lost. Collins lost 73 out of 97 cases, Clark 49 out of 66. I might extend this catalogue, but the fact is so universally admitted, whatever plan is adopted, that it will be unnecessary. Any suggestion, therefore, that holds out a prospect of greater success than has hitherto been attained is deserving of more attention from the Profession. The few published cases scarcely warrant a comparison being made between the result of the postural treatment and the numerous old methods. I feel, however, assured that when it shall come to be more generally known and recognised by the Profession, the rate of mortality will be very materially lessened; besides this, the facility with which it may be accomplished is greatly in its favour, while the entire absence of all danger either to mother or child is a matter of supreme importance. Dr. Thomas's rules are few and simple, and are applicable equally to cases where the membranes are entire as well as where they have been ruptured.

First, if the membranes are entire and the cord detected, he at once places the woman in position, and trusts to this for its return into the uterus, and uses no manual assistance.

Secondly, if the waters have escaped and left the funis below the head, he places the woman in position and pushes it up with the hand, then induces pain either by friction, or better by ergot, and if the presenting part should so occupy the pelvis as to prevent its return by the hand, he uses a gum elastic catheter and tape as a *porte-cordon*; and I would thirdly suggest, from the experience of the two reported cases, that the whole hand should be introduced into the vagina, and if the head interferes, push it up and carry the cord beyond the head, having previously induced pain by ergot.

He believes the cause of the persistence of the accident whatever may have first produced it) to be mainly the slippery nature of the cord, and, secondly, the inclined plane offered by the uterus by which to roll out of its cavity; and his principle of treatment is to invert this plane, thus turning not only this plane, but the lubricity of the cord to our advan-

tage. This he found could be readily accomplished by placing the woman on her knees and elbows with the head down upon the bed, thus inverting the uterine axis.—*Medical Times and Gazette*.

Materia Medica and Chemistry.

EAST INDIAN QUININE

The efforts made by the Indian Government to introduce the Cinchona plant in India and Ceylon are detailed in a voluminous blue book lately presented to Parliament. It contains no less than ninety-four reports and letters respecting the efforts made to extend the cultivation of this valuable plant on the Neilgherry Hills, in Wynaad, Coorg, and Travancore, with a goodly number of reports, showing that the ground has been laid for cinchona cultivation in Sikkim, the Punjaub, Bombay, and Ceylon. There is also a very interesting journal by Mr. Cross, who was commissioned by the Indian Government to make a collection of seeds from the cinchona forests near Popejan, in South America.

It appears that in 1831 the Government of Madras desired the Home Government to have an analysis made of the East Indian bark, and a number of specimens were collected and sent over by Mr. McIvor, the superintendent of the Government plantations. Mr. John Elliott Howard, the analyst, in his report, stated, "I have great pleasure in informing you that the result of my examination of the bark of *C. succirubra*, grown in India, is very satisfactory. I have thus far only operated upon 500 grains, proceeding cautiously, as the quantity of bark sent is small. I find exactly the same constituents as in South American "red bark," and was able to obtain a first and second crystallization of very white sulphate of quinine mixed (as is usual when obtained from red bark) with sulphate of cinchonidine; I have also obtained some cinchonine. This must be considered very satisfactory, and a promising result when the immature age of the bark is considered" (viz., two years' growth). On this favourable report the superintendent was authorized to sell 100,000 plants, which were all speedily applied for by the native planters. A second collection of samples was sent to Mr. Howard for his report which was still more favourable. He wrote:—

"I have since devoted most careful attention to ascertain by experiment the probable market value, especially of the first two samples of bark sent. It will not be necessary for me to detail the various means by

which I succeeded in convincing myself, not only of the existence of the alkaloids, but of their being extant in such a state of purity as is certainly not found in the ordinary samples of red bark imported from South America. The result of my examination tended to show distinctly that cultivation has improved the produce of at least this species of cinchona.

“ I must remark that the commercial value of specimens of bark intended for the manufacturers of sulphate of quinine can never be ascertained by the mere knowledge of the percentage of alkaloid soluble in ether, since it is necessary that this should be shown to exist in such a state as to crystallize with acids into the required compounds.

“ In this case of No. 1, the bark from the thickest part of the lower branches of a *C. succirubra*, two years and five months old, this examination was most satisfactory, confirming that which I stated in my first report as to the facility with which the alkaloids were obtained in a state of purity, although the amount of red colouring matter in the bark is very great. The amount of purified alkaloids I estimated at 6 per cent., consisting of quinine 3.14 cinchonidine 2.03, cinchonine 0.80. This large product of alkaloids might probably be still further increased by surrounding the stem with moss, in the manner which Mr. McIvor has so happily suggested, since Dr. De Vry found 8.409 per cent. of alkaloids, in a stem which had been so treated. It seems to me, from this trial, that the East Indian bark, the produce of *C. succirubra*, will rival in price the Bolivian *Culisia*, which is by no means the case with the bark of the branches of *C. succirubra*, as grown in South America. It is important to remark, that the very high price of between 8s. and 9s. which has quite recently been paid for red bark in this market, applies only to those pieces of bark from the trunk of the tree which possesses, from their age, a peculiar bright red appearance. I have forwarded a small vial with commercial sulphate of quinine obtained from this No. 1, as also sulphate of cinchonidine separate from the above. I have only to remark further on this bark, that its appearance bespeaks its good quality, and that there can be no doubt the season chosen (24th of February) is most favourable to its being well secured.”

Mr. McIvor, the superintendent of the plantations, appears to have tried the plan of mossing the bark of the plant in order to increase the deposit of the quinine therein, and wished to secure it to himself by patent, but the Government were of opinion that as it was invented in the course of his official duty, it would be a bad precedent to adopt. The experiments made by Mr. Clements to Markham proved, however, that the plan was extremely beneficial; he states that a tree two and a

half years old yielded alkaloids of 2.43 per cent., but 5.20 when mossed for a year. These results, he states, gives us the certainty that the correct method of treating the cinchona trees is to cover the stems with moss, to remove the bark periodically, to renew the bark by mossing the stem, and to allow the tree to continue growing until it attains its utmost size. Dr. De Vry told Mr. Markham that with muriatic acid and caustic soda he treated the green bark and produced a fine powder consisting of all the febrifuge alkaloids of the bark, which will practically be as efficacious as the expensive sulphate of quinine.

From a return included in the report, it appears that the number of plants on the Neilgherry Hills, which at the beginning of 1863 were a little over 100,000, in May of the present year exceeded 1,100,000. In the other districts mentioned in the list the same activity is manifested. From Ceylon Mr. Markham reports that the coffee growers have taken to the cultivation of the cinchona in a hearty manner as many as fifty planters having applied for plants, of which 180,000 have been distributed, the manager of a large estate belonging to Rothschild being the foremost amongst them.

It also appears that Government have ordered new roads to be made especially for the use of the districts in India where this plant is being cultivated, and there can be little doubt that the supply will be greatly increased, as the cultivation of the plant is rapidly extending.—*Chemist and Druggist.*

Miscellaneous.

PRODUCTION OF THE SEXES AT WILL.

The following is a very brief extract, condensed from the *American Journal of Science and Arts*, for July, 1864, and January, 1865, of an important memoir of M. Thuny, of Geneva, and of an account of some experiments of MM. Coste and Gerbe, on the Law of the Sexes. The original memoir of M. Thuny was published in the *Bibliothèque Universelle*, in 1863.

This investigator was first led to his conclusions by the following well known facts:—1st. The fundamental or morphological identity of the sexes. From this he concludes that the difference of the sexes is due to slight differences in the process of the development of the ovum in its earliest stages. 2nd. That in plants (those which are unisexual), the character of the sex may be controlled by the management of external agents. 3rd. That, according to Huber, the ova of the Bees, if fecundated early, produce workers (females), whilst if fecundation be

retarded until the twenty-second day, all the eggs deposited produce males.

For these reasons M. Thuny concludes that *the sex is determined previous to fecundation, or rather by the maturity of the ovum at the moment of fecundation.*

If no fecundation takes place, the development is arrested at a certain stage, and the ovum perishes; but if fecundation occurs, there is a new accession to life's force, which suffices to carry it through all stages of embryonic and extra-uterine life.

Now, according to M. Thuny, during the earlier stages of the ante-fecundation history of the ovum, the sex is female; but, if the development continues without fecundation, it becomes male. By impregnation the sex is fixed for ever.

In uniparous mammalia the ovum leaves the ovary at the beginning of each rutting period in a very immature condition, and passes slowly through the fallopian tubes, the uterus, and finally, if unfecundated, is discharged.

Now during the whole of this slow passage, the ovum is maturing. If, therefore, fecundation takes place early in the *period of heat*, the sex of the embryo will be female. If later it will be male. * * * Now, if M. Thuny is right, *fecundation at the commencement of the menstrual period will produce females, and later, will produce males.* He does not indicate the exact turning point.

Anxious to subject his theory to the list of disinterested experiments, M. Thuny gave minute directions to M. Cornaz, an intelligent Swiss stock-raiser, and son of the former President of the Swiss Agricultural Society. These directions were followed in twenty-nine cases, and *in every case, without exception, the desired sex was produced.* First, in order to propagate the breed of a very fine Durham bull, M. Cornaz wished to get heifers; he made twenty-two experiments, and got heifers every time. He then wished to get a few bulls of half breed to sell his neighbors; he made seven experiments, and got bulls every time.

The great importance of the theory, if true, both in a scientific and a practical point of view—both to the physiologist and the farmer, cannot be doubted. But the history of the theory can only be accomplished by intelligent and very careful observation. The physical signs of the generative period differ in different species, and in different individuals of the same species, particularly in domestic animals. It is always well marked in wild animals, but in domestic animals it is often obscure. Close and patient observation will, however, overcome all these difficulties.—*Nashville Journal of Medicine and Surgery.* *

Canada Medical Journal.

MONTREAL, DECEMBER, 1866.

The 29th section of the Medical Act for Great Britain provides the following: "If any registered Medical Practitioner shall be convicted in England or Ireland of any felony or misdemeanor, or in Scotland of any crime or offence, or shall after due inquiry be judged by the General Council to have been guilty of infamous conduct in any professional respect, the General Council may, if they see fit, direct the registrar to erase the name of such medical practitioner from the register." Acting on this power, the General Council of Medical Education and Registration of the United Kingdom has recently published a list of delinquents with the causes which gave rise to the punishment inflicted on these persons. Among the number we notice the name of an individual who we believe some short time since appeared in our city, but who found Montreal too limited a sphere for his operations, and hence his stay with us cold-blooded Canadians was but short. The sentence as published against this man reads: "Samuel LeMert, for infamous conduct in a professional respect." It is to be regretted that in Canada we have not the power to apply a wholesome check on quackery. The sooner our colleges acquire from the Legislature similar powers the better, as it is notorious that several (to the credit of our graduates they are but few) individuals in this country are living on the credulity of their fellow men. We notice by the last periodicals received from Great Britain that a trial was pending on the suit of a Dr. Hunter against the publisher of the *Pall Mall Gazette* for libel. Dr. Hunter may be known to several of our readers; he is described as a Canadian M.D. This is not the case; we believed he received many years ago a Governor General's license to practice in this Province; but had a salutary law, as the one above quoted, been in force, the same punishment might have been meted out, and his name struck off the roll of properly qualified practitioners, "for infamous conduct in a professional respect." The sooner the profession is roused to action in this matter the better, roused

to a sense of its own dignity, and fully alive to the moral degradation of permitting a continuance of these practices without one single attempt on its part to redress the wrong. Had we possessed that power and exercised it in respect to the individual above alluded to, we would have had at least the satisfaction of reading in the evidence of the plaintiff in the case, that he had received a Governor's license to practice Physic, Surgery and Midwifery in the Province of Canada; but that he had been struck off the roll of regularly registered practitioners for infamous conduct in a professional respect.

In the opening address of Mr. Coleridge, the learned counsel for the plaintiff, he is reported to have said that his client had "published a book which was advertised, and perhaps not in the best taste, nor could it be considered strange that it should excite comment. It was the mode of advertisement adopted in America." To this we would merely remark that it is the mode adopted by quacks and charlatans, not only in America but in all other parts of the world; but that it is not adopted by men who respect themselves, and whose desire is not to make a livelihood by acting on the credulous fears of the multitude. We should suppose that our Canadian Universities have the power without seeking legislative enactment of striking off their roll such of their sons who prove recreant to all that is commendable and honourable in their walk through life; but with regard to the two licensing bodies the College of Physicians and Surgeons of Lower Canada and the General Council of Medical Education and Registration of Upper Canada, the case is different. To enable them to carry out this most necessary check against wrong doing, they must acquire the power from the Legislature; and the sooner it is done, the sooner will the profession in Canada secure that respect from the community and from outsiders which is so desirable. These suggestions we offer with the sincere hope that they may be carried out by those two bodies, and that by joint action the desired amendment to the acts of incorporation giving them summary powers may be secured from the Legislature at its next meeting.

A MEDICAL CONGRESS.

It is proposed to hold an International Medical Congress at Paris during the Exhibition, to begin on the 16th of August, and to last two weeks. It is to be composed of two classes of members; national foundation members, or French physicians, who are to pay a subscription of twenty francs, and adherent members, or foreign physicians, who are not required to make any pecuniary contribution. M. Bouillaud is the president of the

committee. It may interest the profession to know that the following subjects have been proposed for discussion :—

1. Pathological anatomy and physiology of tubercle; tuberculization in different countries, and its influence on general mortality.
2. The general accidents which occasion death after surgical operations.
3. Is it possible to propose to the different governments any efficacious measures to restrain the propagation of venereal diseases?
4. The influence of the alimentation used in different countries upon the production of certain diseases.
5. The influence of climates, races, and different conditions of life upon menstruation in different countries.
6. The acclimation of European races in hot countries.
7. The entozoa and entophytes which may be developed in man.—*Transcript.*

MEDICAL NEWS.

The use of Sir William Burnett's disinfecting fluid has been ordered to be discontinued on her Majesty's ships, from the fact that several fatal cases of poisoning have occurred by its having been accidentally swallowed by seamen.

ARMY MEDICAL DEPARTMENT.—It is a noticeable fact in connection with the constant complaint of army assistant-surgeons respecting slowness of promotion, that less than three months since the seniors of these officers obtained their steps under twelve years' service, whereas now there are forty assistant-surgeons on the list who have completed twelve years' service. It would appear, from this fact, that the chance of promotion to a full surgery (the great step) is daily becoming less and less. The evident reluctance of old surgeons-major of thirty years' service and upwards to take their half-pay retirement, is no doubt one of the causes of the plethora with regard to assistant-surgeons' promotion, these officers (surgeons-major) looking forward to their promotion to deputy-inspector-general's rank. However, as this list contains only twenty-seven officers, most of the expectants have but a weak chance of having their hopes gratified.

Mr. Edmund Sharpe has presented to the British Museum a statue of the son of Rameses the Second, about four feet high. He bears a standard on each side; it is of most beautiful workmanship, on hard polished breccia. It is placed near the head of Memnon, in the Egyptian Gallery. It is in a very good state of preservation, and is a beautiful specimen of Egyptian art. It is curious as a lithological specimen, the breccia being formed of the consolidated sand of the desert, including jasper, chert, and other siliceous pebbles.