

once or twice a day, a severe irritability comes on in the urethra after passing urine, which gives great annoyance and reflex irritation, with constant desire to micturate. At that time of life you will find a want of control over the semi-involuntary actions of the body; and in this instance, once a forcing and bearing down having been engendered, the patient seems to lose self-control, and, in the tenesmus which accompanies it, confuses in her mind the various symptoms—in fact, she declares herself unable to explain exactly her ailment. You can, however, gain some clue by beginning at the beginning, and making her recall to mind the early symptoms.

I remember one elderly woman who suffered a paroxysm lasting three or four hours every day of symptoms of forcing of urine, uterus, and rectum, who became quite wild during each attack. She could only say it was in the vagina; and that when it once began she was in such general distress that she could not bear herself. Upon going through her history, I found only some urethral irritation, and a little tenderness on pressure. The catheter having been passed, she complained of much pain, and upon inquiry, she said the instrument touched the real seat of the pain complained of at the time of attacks. Upon still further analysis, it seemed clear that the origin of the annoyance was in the urethra. It appeared that when considerable irritation was set up by the passing of urine, a kind of reflex action of the bladder, and then of the pelvic organs, was established, to control which seemed beyond the power of the patient. In this case I advised the use of the tannic-acid bougie and soothing remedies. The passage of the bougie gave great relief, and she was much benefited for a considerable time while under treatment. No doubt irritation occurring in the other pelvic organs will produce this peculiar action; but in two cases I have seen it clearly arose from chronic urethritis. I may, in leaving this subject, remark that, in simple urethritis, such as is seen in the more chronic cases, there is no irritability of the bladder of necessity. You may have it; but in that case it rather arises from more or less of cystitis accompanying it, or from reflex irritation of the bladder.

These symptoms of tenesmus arising from simple causes to a considerable extent resemble those of *polypus of the urethra*, particularly where the growth is situated at the neck of the bladder, in which position it occasionally is carried by the urinary current into the urethra, causing severe annoyance by dysuria, forcing, and generally some appearance of blood. As a rule, it may be said that bleeding in small quantities generally accompanies urethral polypus, not mixed with the urine, but commonly following it. You will see that here, again, the symptoms are very like those of calculus, especially a recent one and of small size. But the passage of a catheter or sound, coupled with digital examination in the vagina, will prove whether or no there be a calculus in the urethra or bladder; because a stone in the female bladder can almost always be detected per vaginam, at least the existence of a solid within it will be shown, while the catheter will tell you if that body be hard, like a calculus, or softer, as an outgrowth. These polypi, however, are so soft that the touch by the catheter cannot detect them. It will be necessary, when these symptoms exist, that we, after a trial of the tannic-acid bougie or other styptics has been made without result, dilate the urethra under chloroform, and examine carefully by the finger, and even with the eye, whether any polypus or outgrowth exist; in which case its removal by a wire snare, or torsion by forceps, or even by the finger-nail if it have a very thin pedicle, should be done at the time.

And this brings me to remark that incipient *epithelioma* of the mucous membrane of the urethra may produce bleeding, pain, and other symptoms of simple urethritis. If it be near the neck of the bladder then we shall have that organ in a state of irritation more or less, with an extrusion of blood following the flow of urine. Now, the diagnosis of epithelioma of urethra from urethritis is difficult, except that bleeding is rare in the latter. In polypus, where we may expect bleeding, the distress is more apt to become paroxysmal; where we have a moderate amount only of distress, with a considerable quantity of bleeding, we may suspect rather an epitheliomatous condition.

However, it is best in such a case to place the patient under chloroform, dilate the urethra, and pass up the finger, when irregularity of the mucous surface can be readily detected. Of course I am here alluding to incipient stages of epithelioma, for in the advanced state it is readily deciphered by vaginal examination of the urethra and bladder. It is by no means difficult immediately to dilate the urethra under chloroform, as has been shown by the removal of large calculi. I may here relate a case which will illustrate the mode of accomplishing it in epithelioma of the neck of the bladder and upper urethra.

The patient was about fifty years old, complaining of discharge of blood, which she thought came from the womb; she further stated that she lost it in gushes, and after passing water. This is a common description of bleeding from malignant disease of the uterus. The uterus on examination was, however, found healthy. Inquiries were then directed to the bladder, and it was found, after she had taken more particular notice, that the blood arose from the urethra. There was sometimes a considerable quantity, and some of it evidently came from within the sphincter, because she had the power of retaining it. Nothing could be felt by the vagina. She complained of frequent desire to pass urine, and the usual symptoms of irritation about the neck of the bladder. After trying various expedients, the urethra was examined under chloroform. This was done in the following manner, under full anaesthesia:—Weiss's two-valved dilator was first introduced, and then the three-valved one. After this, one of the smallest elastic water-bag dilators of Dr. Barnes's set was employed, and after about five minutes the second size. The urethra did not stretch to the full size of the dilator, but I was able to pass my forefinger with ease into the bladder, and examine its state for about two inches within. This enabled me to detect a superficial epithelioma on the right side, extending within the bladder one inch, and down the urethra half an inch. But little bleeding ensued, and I was at the same time able to apply nitrate of silver to the surface. This case shows that in the female our means of discovering and locally treating diseases of the bladder may probably be much increased.

It has been shown by Mr. Bryant, in the "Medico-Chirurgical Transactions" for 1864, that the urethra can be dilated rapidly, without any fear of incontinence after, even where the urethra has been distended to withdraw a stone two inches in diameter. We need not, therefore, fear that the introduction of the finger would lead to any troublesome result.

Ulceration of a syphilitic kind, running up the urethra into the bladder, causing very great distress, is occasionally seen. A patient was admitted into Guy's Hospital with a calamity of this kind. Urine was constantly running away from extreme irritability of the bladder, with severe scalding pain on passing it. The vagino-urethral septum was eroded from the meatus urinarius to half way to the bladder, and the mucous membrane of the urethra was abraded, readily bleeding on passing the catheter. She was emaciated from constant irritation and want of rest. I first of all applied nitrate of silver to the urethra, which was soon cured of its irritable state. This having been accomplished, one grain of morphia in solution, in the manner to be pointed out hereafter, was thrown into the bladder once a day. This produced speedy relief of the principal symptoms, and she was generally able to hold her urine for two hours; sometimes she could not for longer than an hour.

(To be continued.)

MEMORANDUM

ON

THE NATURE AND THE MODE OF PROPAGATION OF PHTHISIS.

By WILLIAM BUDD, M.D.,

CONSULTING PHYSICIAN TO THE BRISTOL ROYAL INFIRMARY.

"He that would follow philosophy must be a freeman in mind."—PTOLEMY.

[NOTE FROM DR. PAGET.]

To the Editor of THE LANCET.

SIR,—The paper I send enclosed was received by me last December, in a sealed packet, from Dr. William Budd, of Clifton, with a request that I would take charge of it until he should direct me to break the seal. At his desire, I opened the packet a few days ago, and I now send you the contents, requesting the favour of their early publication in THE LANCET. They are an epitome of what Dr. W. Budd has been for some time intending to publish in a more complete form; but his intention has been frustrated, and is still delayed, by the engrossments of professional practice and other circumstances beyond his control.

You will at once perceive the originality of his views, and their very high importance if established. If the evidence now

given of their truth be incomplete, it is at least abundantly sufficient to raise them out of the region of mere hypothesis, and ensure their careful consideration by pathologists.

In a letter to me Dr. W. Budd says he can show strong reasons for believing that his views on tubercle, with certain qualifications, apply to cancer also.

I am, &c.,

Cambridge, Sept. 30th, 1867.

G. E. PAGET.

THE following are the principal conclusions to which I have been led regarding Phthisis or Tubercle:—

1st. That tubercle is a true zymotic disease, of specific nature, in the same sense as typhoid fever, scarlet fever, typhus, syphilis, &c. &c., are.

2nd. That, like these diseases, tubercle never originates spontaneously, but is perpetuated solely by the law of continuous succession.

3rd. That the tuberculous matter itself is (or includes) the specific morbid matter of the disease, and constitutes the material by which phthisis is propagated from one person to another, and disseminated through society.

4th. That the deposits of this matter are, therefore, of the nature of an eruption, and bear the same relation to the disease, phthisis, as the "yellow matter" of typhoid fever, for instance, bears to typhoid fever.

5th. That by the destruction of this matter on its issue from the body, by means of proper chemicals or otherwise—seconded by good sanitary conditions,—there is reason to hope that we may, eventually, and possibly at no very distant time, rid ourselves entirely of this fatal scourge.

The evidence on which these conclusions are founded is drawn from the following principal sources:—

(a) Considerations based on the pathology of phthisis, as showing it to consist in the evolution and multiplication within the organism of a specific morbid matter, with a universal tendency to elimination and casting forth of the same, after the type of zymotic diseases generally.

(b) Actual instances in which there was evidence to show that phthisis was communicated from one person to another.

(c) The geographical distribution of phthisis in past and present times, and, especially, its great fatality now in countries which when first discovered by Europeans were known to be entirely free from it.

(d) Its much greater prevalence in low levels and among crowded communities, and its entire absence, unless by casual importation, at very high levels,—conditions which are well known to rule, in the same directions, the spread of zymotic diseases generally, and especially of that group in which, as in phthisis, the morbid matter is cast off in a liquid form.

(e) Its very high rate of prevalence in convents, harems, barracks, penitentiaries, &c.—that is to say, under the very social conditions which are known most to favour the propagation of diseases of the zymotic group.

Among the data relating to geographical distribution the following striking facts may be here mentioned:—

1st. When the South Sea Islands were first discovered phthisis did not exist there. Since the aborigines have come into intimate contact with Europeans the disease has not only made its appearance among them, but has become so widespread as to threaten their extermination.

The contrast between original entire immunity and present extreme fatality is very striking, and can only be rationally explained by the importation of a new and specific morbid germ.

Try every other supposition, and the facts are inexplicable; make this one supposition, and they are at once explained.

2nd. The late Dr. Rush, of Philadelphia, who made very accurate inquiries to determine the point, satisfied himself that when America was first discovered, phthisis was unknown among the native American Indians. Now it is very fatal to them.

The very significant contrast here exhibited between the past and present history of these two races, in respect of phthisis, is exhibited at once, and at the present time, among the negro race in Africa, in different parts of the area of that great continent.

It is well known that negroes are peculiarly liable to phthisis.

Now, everywhere along the African seaboard where the blacks have come into constant and intimate relations with the whites, phthisis causes a large mortality among them. In

the interior, where intercourse with the whites has been limited to casual contact with a few great travellers or other adventurous visitors, there is reason to believe that phthisis does not exist. Dr. Livingstone and other African travellers have given me the most positive assurances on this point.

The idea that phthisis is a self-propagated zymotic disease, and that all the leading phenomena of its distribution may be explained by supposing that it is disseminated through society by specific germs contained in the tuberculous matter cast off by persons already suffering from the disease, first came into my mind, unbidden, so to speak, while I was walking on the Observatory hill at Clifton, in the second week of August, 1856. The close analogy in many quite fundamental points between this disease and typhoid fever had often impressed itself on me with very great force while I was engaged in the study of the latter, and in the preparation of the papers which I have published on it. I now saw with a clearness which had never occurred to me before, that, with the exception of the qualifications necessary for their application to a chronic disease—for the most part of slow evolution and indefinite duration,—the leading conclusions to which I had been led respecting the propagation of the fever, might be applied with the same strictness to phthisis also.

This idea had no sooner taken possession of my mind than considerations of great force and in overwhelming number crowded upon me in illustration of it.

In the course of the same evening I drew up some notes on the subject, and before the end of the month my views upon it had taken, in outline, the exact shape which they now have. The long interval which has occurred between the summer of 1856 and the present date has been occupied in collecting data bearing on the various questions raised by this new theory—in accumulating evidence of various kinds, and in examining and carefully weighing difficulties. During the whole of this long time the subject has scarcely ever been absent from my mind. The result has been only to confirm me more and more in the truth of my first conclusions. I earnestly hope that they will not be lightly rejected. At any rate, I can say that they have not been brought forward in haste or without due deliberation. I have, in fact, considerably exceeded the ten years, which, with a fine sense of what is due to such an enterprise, the Roman poet prescribed as the time to be given to every composition intended by the writer to endure.

Many causes have helped to prevent me from giving my views on this subject sooner to the world. Chief among them I may name want of time to put them into that scientific form, and clear logical order, under which alone an innovation so daring has any chance of being entertained, much more of being accepted, by the profession. This task, however, I hope to complete in the course of a few months. Meanwhile I have thought it well to place this memorandum, by way of record, in the hands of a friend, to be made public at any moment should occasion seem to require it.

Manor House, Clifton, Dec. 1st, 1866.

THE NATURAL HISTORY & TREATMENT OF LATERAL CURVATURE OF THE SPINE.

By RICHARD BARWELL, F.R.C.S.,

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No. I.

THE plethora of medical literature is such that it is the duty of every man to refrain from the printing-press unless that which he intends to publish is something more than a mere paraphrase of older work—unless he have something to teach that is worth the time of busy men to learn. In writing these few papers upon lateral distortion of the spine, I am aware of attacking a subject upon which a great amount of ink has been expended, whose literature is, nevertheless, in an unsatisfactory state—firstly, because the pathological views of authors are so varied and manifold; secondly, because not one of the theories, nor all of them put together, account for the occurrence of rotation in lateral deviation of the vertebræ, for the predilection of this condition to girls and young women, nor for its prevalence on the right side. Among these innu-