sulphuric acid. When uric acid is treated with nitric acid evaporated to dryness over a water bath, and the residue slightly moistened with ammonia, there is formed murexide or diammonic purpurate, which presents a beautiful purple colour. Dr. Garrod has obtained crystals of uric acid from the blood of gouty persons. Of urinary calculi, generally, I shall hereafter have to speak.

Urate of Ammonia (probably a deposit of mixed urates of ammonia, soda, potash, and lime, and now termed "amorphous urates") is the most familiar of all the urinary deposits, occurring variously coloured, and in a finely granular form. It is sometimes found deposited in the structure of the kidney. The precipitate, slightly soluble in cold water, dissolves readily on the application of heat;* acids decompose it, precipitating the uric acid in the form of rhombic tablets.

Urate of Soda is one of the most remarkable among the unorganised substances that exist in a separated condition from the human body, as it forms the constituent of the so-called, or rather mis-called, *chalk-stones* in gout, or spread over the articular surfaces and on the tendons in this disease. Urate of soda from gouty concretions appears under the microscope as long delicate needles, arranged in bundles or stars, or lying separately (Roberts). Urate of soda occurs also, as I have just stated, mixed with the urate of ammonia, as a sediment in the urine. Here it consists usually of minute granules, which are unequally aggregated together in different parts of the field of the microscope. Urate of soda is sometimes *spontaneously* deposited from the urine as hedgehog crystals, especially in children during fugitive febrile attacks.

4. *Salts of Lime*.—These are the *phosphate* and *carbonate* of lime, which occur in the calcareous transformation and so-called ossifications (calcifications) of natural tissues, and in the cretification of the adventitious products of disease (as conversions of fibrinous exudations, tubercles, pus, etc.), and the *oxalate* of lime in the urine in crystals, and as a calculeous concretion of the same.

(a) *Carbonate of Lime* (CaCO_3) occurs almost always in an amorphous state, and usually combined with the phosphate ($\text{Ca}_3(\text{PO}_4)_2$). It is soluble in acids with effervescence. Precipitates of calcic carbonate occur in almost every fluid of the body (Vogel), but rarely as urinary deposits in man (Wedl). It is said to occur occasionally in the urine in globular spheres and cornucopia-like crystals (Bird, Hassall).

(b) *Phosphate of Lime* ($\text{Ca}_3(\text{PO}_4)_2$) occurs in pathological deposits, usually in the amorphous form. It is a frequent ingredient in urinary sediments, where it sometimes, though rarely, assumes a stellar form. Calcic phosphate forms a common ingredient in nearly every animal fluid (viz., blood, urine, gastric juice, saliva, semen, milk, and juices of organs, Frey). It is normally held in solution by an acid, or it forms soluble compounds with certain organic compounds, as gelatin, etc.) Calcic phosphate, when treated with hydrochloric acid, dissolves without bubbles, and is thus distinguished from the carbonate. It is distinguished from the urates by the non-appearance of uric acid (Wedl).

(c) *Oxalate of Lime* ($\text{CaC}_2\text{O}_4 + 3\text{aq}$) is usually found in the urine in the form of obtuse square octohedral crystals, which are sometimes remarkably minute. It is sometimes found in nodular concretions in the pelvis of the kidney between the mamillary processes. The urine of hypochondriacal and other nervous persons especially was thought, by Prout and Golding Bird, to contain oxalates in excess. Oxaluria may result from eating rhubarb too plentifully. Dr. Beale thinks that crystals of oxalate of lime form the nuclei of most urinary calculi. The octohedral crystals are insoluble in water, alcohol, ether, and acetic acid, but soluble in hydrochloric acid. Oxalate of lime, also, occurs in the urine as dumb-bell crystals, which were supposed by Dr. Golding Bird to be formed of oxalurate of lime. They are, however, probably identical in composition with the octohedra. "The precipitation of oxalate of lime as dumb-bells depends upon some physical condition which interferes with the ordinary crystallisation. Very frequently urine depositing dumb-bells contains little masses of viscid mucus; and it seems probable that a certain viscosity of the urine is essential to this globular precipitation" (Roberts). As I have previously remarked, the form of uric acid crystals also, according to Dr. Ord, depends on certain physical conditions of the urine.

5. *Ammonio-magnesian Phosphate* ($\text{MgNH}_4\text{PO}_4 + 6\text{aq}$) occurs in crystals, the fundamental figure of which is the rhombic vertical prism, the common form being like a sarcophagus lid. The crystals are very readily soluble in dilute acetic acid. They are, according to Vogel, "amongst the most common that are met with in the microscopical examination of the human body", which is accounted for by the circumstance, that the phosphate of magnesia exists in almost all the fluids of

the body, and, when it comes into contact with ammonia, which is readily generated in the organism, it unites with it and produces crystals of this, the triple phosphate. The formation of crystals of phosphate of magnesium and ammonium accompanies the process of decomposition in the animal organism, but occurs only in alkaline or neutral sanies (Rindfleisch). This salt, also, forms a crystalline pellicle on the surface of the motions in typhoid fever (Wedl). Besides occurring as a crystalline urinary deposit in alkaline urine, this salt constitutes also one of the well-known urinary calculi. Dr. W. Roberts says, "In the immense majority of cases, the deposition of this salt is only an incident due to the loss of the acid reaction of the urine, and especially of ammoniacal decomposition of the urine."

Cystine, Xanthine, Leucine, and Tyrosine, must be omitted from our consideration from their rarity and the want of time.

6. *Pigmental Formations (pigments).*

(a) The colouring matter of the blood (the hæmatin) furnishes more than one of these, in various forms of crystals, e.g., hæmatoidin, which differs from hæmatin in containing no iron. It is found in old apoplectic cysts, and in otherwise retained extravasated blood. It crystallises in fiery red rhombic prisms, and also in acicular crystals (Robin), which are soluble in chloroform, sulphide of carbon, and absolute ether, but not in absolute alcohol, water, ammonia, solution of soda, or dilute acetic acid.

(b) *Bile-Pigment*.—"Never, or only in very rare cases, is the biliary pigment deposited in a solid form. Virchow detected crystalline deposits of bilifulvin in the epithelium of the gall-bladder" (Rindfleisch).

(c) *The Colouring Matter of the Urine (uroerythrin)*, or urohæmatin. Little is known of this pigment.

(d) *Melanin*.—As a pathological product, melanin, or something nearly allied to it, is frequently met with in great abundance in different organs, tumours, etc. It is a substance remarkable for its insolubility and unchangeableness. It is not soluble in water, alcohol, ether, dilute mineral acids, or concentrated acetic acid. It is dissolved, after some considerable time, in warm potash solutions (Frey).

Such are the principal known substances, which, separated from the organism that generated them, exist within it as unorganised formations. I have spoken of them at present principally in their relations simply as chemical substances; it remains for me to say something more of the conditions under which some of them occur, and their state of aggregation, and the localities in which they are presented.

THE TREATMENT OF STRICTURE OF THE URETHRA.*

By F. N. OTIS, M.D.,

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MR. CHAIRMAN AND GENTLEMEN,—The urethra may be described as an appendage of the urinary bladder and of the vesiculæ seminales; its office, in connection with the vesiculæ seminales, being simply to convey the spermatic fluid, and, in its connection with the urinary bladder, to convey the urine from that reservoir to a proper and convenient distance. It is provided with no especial organic structure, except that which is required for the complete performance of its office as a common carrier. To facilitate the passage of the urine, a propelling power of no insignificant character is furnished by the muscular structures of the bladder, aided by those of the abdominal parietes and the diaphragm. As the urinary fluid is an excretion, irritating in its nature, its immediate and complete discharge from the urethra becomes a necessity; hence this canal is furnished throughout its entire length with a resilient muscular surrounding, which, in its normal condition, effects this purpose with certainty and completeness. In order, therefore, that the fluids which find their exit from the body by the urethra, shall pass through it with the least possible friction, the chief mechanical necessity in its formation is complete freedom from obstruction. Any obstruction, therefore, from any cause—congenital or acquired—must, of necessity, be considered an interference, to a greater or a less degree, with the functional perfection of the genito-urinary apparatus. Constrictions of the urethral canal, from various causes, and at various points, are known to occur, and vary from slight interference with its muscular surroundings to virtual obliteration of the canal.

* "As no other urinary deposit disappears with simple heat, this circumstance offers an easy means of recognition" (Roberts *On Urinary and Renal Diseases*, 2nd edition, p. 68).

* A demonstration given at the Annual Meeting of the British Medical Association in Edinburgh, August 1875.

Congenital narrowings of the urethral orifice are, perhaps, as common as congenital phimosis; but this point is so much in the line of inflammatory action, from infantile balanitis and irritations from other causes, that it is quite impossible to draw the line, in any case, between the congenital and the acquired condition. Other than at this point, there is, I believe, no record of congenital urethral stricture. Strictures, at all other points, are recognised as of inflammatory origin, from traumatism, from masturbation, or from gonorrhoea. Any inflammation, set up by any cause, which dips below the mucous membrane lining the urethra, occasions, of necessity, an aggregation of plastic connective-tissue material, which, becoming organised in the submucous structure, is at once established as a point of obstruction in the normal urethral canal, and, when surrounding it (either by interference with the muscular resiliency of its walls, or by contraction of its lumen), this is elevated into the importance of a true urethral stricture.

Now, assuming that complete freedom from obstruction in the urethral canal is necessary to completeness of function, it will be readily seen that the slightest narrowings at once assume a pathological importance. It will also be seen that, in order to appreciate the extent of any contraction in a given case, some method must be adopted by which the exact normal calibre of individual urethrae may be ascertained. It is easy to see that no *average standard* for the human male urethra can be arrived at, which will be of service in determining this point. Authorities are explicit in stating that, when No. 21 of the French scale (viz., 12 of the English) can be passed through a given urethra without hindrance, no stricture can be said to exist; but the simple mechanical nature of the proposition necessitates the rejection of any such arbitrary method of determining the normal urethral calibre. If the virile organ differs in size in different individuals (which no one here present is likely to deny), is it unreasonable to believe that *urethrae* also differ? This once admitted, makes an average standard impossible. We must then have some means of *actually measuring* the normal dimensions of every individual urethra, in which stricture is present, if we would hope to ascertain the locality and value of the contractions.

For this purpose, I have devised an instrument—the *urethrometer*. This consists of a small straight cannula (No. 8 F), terminating in a set of fine steel springs hinged upon the cannula, and also upon the distal extremity of the instrument, where they unite. At this point, a fine rod, running through the cannula, is inserted. This rod (which is worked by a screw at the handle of the instrument) when retracted, expands the springs (six in number) into a bulbous shape, from ten or twelve millimeters in circumference when closed, and capable of expansion up to forty-five millimeters.*

A thin India-rubber stall (Fig. c), drawn over the closed springs, protects the urethra from injury, and prevents the access of urethral secretions to the interior of the instrument. When introduced into the urethra, and expanded up to a point which is recognised by the patient as filling it completely (and yet is easily moved back and forth), the index at the handle will then show the normal circumference of the urethra under examination. In withdrawing the instrument, contractions at any point may also be exactly measured, and any want of correspondence between the calibre of the canal and the external orifice be readily appreciated. Accepting what has been stated in regard to the urethral function, it must also be accepted that the slightest abnormal contraction of the urethra at any point must result in an increased amount of friction at such point. Friction means irritation; chronic irritation of the mucous membrane increases the normal secretions. The cause being continued, this naturally eventuates in purulence; hence, a chronic purulent discharge—in common parlance, *gleet*. This enables me to make the broad statement, that *gleet is the signal which nature hangs out to call attention to the fact, that the urethra in*

Fig. 1.
Urethrometer.

which gleet occurs is always strictured at some point. This troublesome complaint, which it has from time immemorial been the habit of surgeons to treat by local injections and internal remedies, has then a mechanical origin; and I need little apology for saying that it can never be more than palliated by any variety of treatment that does not look squarely to the removal of that cause. Stricture of the urethra has been looked upon as chiefly important in its relations to micturition, and when this act has been performed without difficulty to the patient, no stricture has been inferred. This difficulty, however, rarely occurs until a stricture has encroached largely upon the urethral calibre. When this difficulty of micturition has occurred, and stricture from this fact has been recognised, it has been deemed sufficient to restore the canal at the strictured point to a calibre sufficient for easy micturition; then the patient has been pronounced *cured* without at all taking into consideration the normal urethral calibre. Strictures have been dilated, divulsed, divided, up to a purely imaginative standard, with no consideration of the normal urethral structure. As a consequence, only by the merest chance, if at all, has the contracted urethra been restored by treatment to its normal dimensions. Stricture-tissue is simple cicatricial tissue, a tissue well known for its resilient properties. Is it, then, to be wondered at that authorities should agree that stricture is an incurable disease? or that the patient afflicted with stricture after having been treated by *partial* distention, divulsion, or division, should be required to keep up the dilating process by the use of the sound or bougie for the rest of his natural life? If a resilient band should be placed around the penis, constricting the urethra more or less, would it be considered a proper and efficient mode of getting rid of it to stretch this band a little from time to time? On the contrary, a *complete* division of it would be insisted on, if permanent relief were contemplated. Why not, then, apply this common sense plan to a resilient band constricting the *internal* aspect of the urethral canal alone?

To divide the stricture *completely*, and restore the urethra completely to its normal dimensions, would alone seem to promise permanent results. I have been treating urethral stricture on this plan for the last three and a half years, and the result has proved most conclusively, that the radical cure of stricture is possible; and, more than that, that it is easy and certain in the great majority of cases. I have, found, moreover, that the operation necessary for this end is simple, and unattended even with the danger and suffering which accompany the ordinary treatment by forcible dilatation or divulsion. My plan of treatment has been by dilating the stricture or strictures, up to the normal dimensions of the canal, as previously determined by the use of the urethrometer, and then dividing the stricture tissue thoroughly and completely; keeping up the normal calibre by gentle introduction of suitable sounds, until healing of the wound has taken place. The result of this treatment has been (in every case where the stricture has been thoroughly divided), complete and permanent recovery; complete *disappearance—absorption*—of stricture-tissue, and this proven by the results of operations on a very large number of cases. Time does not permit me, on this interesting occasion, to go much more fully into the details of this variety of the treatment of urethral stricture, but I will lay before you the results of operations by this method in 100 cases, comprising 203 operations on 258 strictures, which were reported in full to the New York State Medical Society in February of this year.

Time after Operation.	No. of Cases.	No. of Strictures.
3 years ..	1	4
2½ ,, ..	1	7
1½ year ..	2	8
13 months ..	3	14
1 year ..	4	7
10 months ..	1	2
9 months ..	1	1
8 months ..	1	1
7 months ..	2	10
6 months ..	7	21
5 months ..	1	7
4 months ..	1	3
3 months ..	4	15
2½ months ..	1	10
2 months ..	4	11
1 month ..	1	1
3 weeks ..	1	5
2 weeks ..	1	1
Total ..	37	128

In thirty-one cases, none of the strictures had recontracted. In six cases, most of them had been absorbed, while some remained.

RESULTS.

Cures. Re-examined. No recontraction ..	31 cases.
Cure. Patient perfectly well when last heard from.	
No re-examination ..	52 ,,

* I desire at this point to acknowledge my obligation to Messrs. Meyer and Meltzer of London, the surgical cutlers, who have made important improvements in this as well as in the various other instruments which, during my few weeks' sojourn in this country, they have manufactured for me. I am able to commend these instruments to you not only for their use in the diagnosis and treatment of urethral stricture, but as specimens of mechanical skill and finish which challenge the gratitude of the patient as well as the surgeon.

Perfect relief for a length of time. Return of symptoms. Re-examination. Stricture found to have recontracted	4 cases.
Perfect relief for a length of time. Return of symptoms. No re-examination	5 "
Relief of most symptoms. Some remaining. Patient still under treatment	4 "
Partial relief	3 "
Result not known	1 case.

Total . . . 100 cases.

Out of the 258 strictures, 52 were in the first quarter inch of the urethra; 63 in the following inch, viz., from $\frac{1}{4}$ to $1\frac{1}{4}$; 48 from $1\frac{1}{4}$ to $2\frac{1}{4}$; 48 from $2\frac{1}{4}$ to $3\frac{1}{4}$; 19 from $3\frac{1}{4}$ to $4\frac{1}{4}$; 14 from $4\frac{1}{4}$ to $5\frac{1}{4}$; 8 from $5\frac{1}{4}$ to $6\frac{1}{4}$; 6 from $6\frac{1}{4}$ to $7\frac{1}{4}$.

Authorities allege that the great majority of urethral strictures are found in the vicinity of the bulbo-membranous junction, and cite various possible causes for their frequency in this locality.

By the above statement it will be seen that they occur, as would naturally be expected, in greatest frequency where the inflammation begins the earliest, and rages the hottest, and gradually diminish in frequency in the deeper portions of the canal.

Normal Calibre of the Urethra.

22 Mm. circumference	1	36 Mm. circumference	1
28 " "	3	37 " "	2
29 " "	1	38 " "	6
30 " "	28	40 " "	1
31 " "	25	Not noted	4
32 " "	19		
33 " "	3		
34 " "	26		
			100

In ninety-nine carefully measured cases, the average normal calibre was 31.84 (deducting the case of child of ten years, 22 millimetres), nearly 32 of the French scale.

I have noticed a constant relation between the circumference of the flaccid penis and the size of the urethra. For instance, when the circumference of the penis was 3 inches, the calibre of the canal was found to be 30 m. of the French scale; when it was $3\frac{1}{4}$ ins., it would be 32 m.; $3\frac{1}{2}$ ins., 34 m.; $3\frac{3}{4}$ ins., 36 m.; 4 ins., 38 m.; $4\frac{1}{4}$ to $4\frac{1}{2}$ ins., 40 m. (Fig. 2.) No exceptions have been found to this in the careful measurement with the urethrometer of over 500 cases.

The instruments used in my operations upon stricture have been the following. The bulb-pointed bistoury or meatotome, when the strictures have been within the first half inch from the orifice. Beyond this, I have used my *dilating urethrotome* (Fig. 4), either the straight or the curved. This is an instrument (constructed on the principle of the ordinary parallel rule) which, when closed, measures 18 (French); the bars are separated by means of a screw apparatus at the handle, the amount of separation being registered on a dial at the handle of the instrument. A knife, two millimeters in breadth, runs in a groove down the upper bar of the instrument, guarded after the manner of the urethrotome of M. Maisonneuve. The size of the urethra having been ascertained by means of the urethrometer (or by assuming it from the proportions of the flaccid penis), the urethrotome is introduced beyond the stricture; the screw at the handle is turned, dilating the instrument up to, and a millimeter or two beyond the normal calibre, in order to make the strictures completely salient; then the blade is drawn through them, cutting through the entire mass of cicatricial tissue, severing the strictures completely. The diagnosis of stricture is made in every case by the *bulbous sound* (Fig. 3); and freedom from stricture after operation is demonstrated by the free passage of a bulbous sound of a size corresponding with the previously ascertained calibre of the urethra under operation. For a fuller account of this method of procedure and its results, I would refer you to a paper read by me before the New York State Medical Society in February 1875, and published by Putnam's Sons of New York. The point of most importance to establish, in the view I have presented to you of urethral stricture, is the calibre of normal urethra. This is alleged to be so much beyond that heretofore accepted, that the most ample proofs will naturally be required in support of the correctness of this position.

Material for the practical demonstration of the normal urethral calibre, which I had hoped to be able to afford you to-day, has not been available. In advance, then, of the tests which you may hereafter apply to urethral proportions (either by the use of the urethrometer or by assumption from the circumference of the flaccid penis), I beg leave to report the recent application of the foregoing views to cases in some of the leading hospitals of London. In the course of a clinical lecture, which (through the invitation of Mr. Berkeley Hill) I delivered at the University College Hospital on July 6th, 1875, I demonstrated the pre-

sence of several strictures in the urethra of a patient, whose flaccid penis measured $3\frac{3}{4}$ inches in circumference. After complete divisions of the strictures with my dilating urethrotome, I passed without force No. 32 (French) through the entire urethra and well into the bladder.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 2. Urethral Scale, 1 to 40 m.—Fig. 3. Bulbous Sound.—Fig. 4. Dilating Urethrotome.

Through the courtesy of Mr. Alfred Cooper, Mr. Walter Coulson, and Mr. W. F. Tecvan, I operated repeatedly in the London Lock Hospital and in St. Peter's; and, in each case operated on, the proportionate correspondence of the urethral calibre with the circumference of the flaccid penis was proven. One case was met, in Mr. Coulson's service at the Lock Hospital, when the circumference of the flaccid penis was $4\frac{1}{4}$ inches. After division of an anterior contraction, I passed a solid steel sound, No. 40 F., through the entire urethra without the least force. This instrument was withdrawn by Mr. Coulson, who then expressed his conviction that the normal portions of this urethra had not been overdistended by the introduction of the sound, No. 40. A sound of the circumference of 40 (French) is just six lines, or half an inch, in diameter.

During a recent personal discussion of this point with Sir Henry Thompson (whose able works on genito-urinary diseases are accepted as of the highest authority in America as well as in Europe), he remarked that "he had long seen the practical necessity of a higher estimate of the normal urethral calibre than that generally assumed". I have the greater pleasure in repeating this statement, as I had, before this, accepted this distinguished surgeon's position in this matter, as in harmony with the continental schools (in fixing a normal urethral standard), and hence in direct antagonism with my own.

Yesterday, I received a letter from Mr. Berkeley Hill of London, also a well known authority in such matters, from which I shall take the liberty of quoting some farther facts pertinent to this important matter. He writes as follows: "I did Syme's perineal section in a case of traumatic stricture which you saw lying in my wards, and who had been several weeks in Guy's and St. Bartholomew's Hospitals, and under private treatment, before he came to me, without any instrument having reached his bladder. I measured the circumference of his flaccid penis, and found it $3\frac{3}{4}$ inches; I turned to my audience, and said, 'Now, according to Dr. Otis's observation, this urethra should easily admit 32 F.' I took up 32 F. sound (at the size, a general murmur ran round). I placed it in the meatus, and it slid down to

the bulb, quite by its own weight. Then it was stopped, of course, by the stricture. I then proceeded to divide the stricture upon a Syme's grooved and shouldered staff in the perineum; the thick part of the staff was No. 26. I held it up to the audience, along with the usual one, No. 16, to show the difference. After division, I took 32 F. sound again, and slipped it readily into the bladder." He further writes, "I divided a contracted meatus this afternoon in a private patient (his own doctor being in attendance); after which No. 39 F. passed readily down into the bladder, as I had announced that it would when I measured the canal with the urethrometer. I then made the practitioner pass 39 himself, in order that he might be sure that there was no hocus-pocus in the matter."

To this I will simply add, that *complete* restoration of strictured urethrae to their *normal* calibre, can alone give promise of permanent cure of stricture; and that, if this be thoroughly effected, the result will be as certain and complete as in all other surgical procedures, when the *mechanical* requirements for restoration of impaired structure and function have been thoroughly and judiciously complied with.

SPONTANEOUS GENERATION AND THE "SEARCHING BEAM."

By LIONEL S. BEALE, M.B., F.R.S.,
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II.

IN his paper read before the Royal Society, of which an abstract appears in the BRITISH MEDICAL JOURNAL for January 29th, Dr. Tyndall expresses the wish "to free his mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of 'spontaneous generation'". With this view, he proceeds to discover, by the aid of a luminous beam and his unaided eye, "ultra-microscopic particles" (!), amongst which, as he tells us, he "shows" that "the matter potential, as regards the development of bacterial life, is to be sought". But, extraordinary as it may seem, he does not establish the fact that all particles *within the microscopic limit* are to be demonstrated by this luminous beam. Dr. Tyndall discovers much beyond the microscopic limit, but fails to discern some things within that range which have been demonstrated by many observers. In fact, observer after observer has actually seen, by microscopic examination, bodies in fluids under circumstances which, Dr. Tyndall affirms, preclude their presence. By the "searching ray" alone, Dr. Tyndall proved to his own satisfaction that the fluid in certain flasks was "sterile", but he does not say that his conclusion was confirmed by microscopic examination.* It is possible that he may not mean by "sterile" the complete absence of bacteria; but, as he talks about searching for the trace of "a bacterium", one is led to suppose that he has already reached a point in minute investigation to which no other mortal can ever hope to attain. But he does not stop at the trace of a bacterium. He demonstrates *ultra-microscopic particles*, and defines the exact situation where matter potential to develop bacterial "life" is to be sought. In another memoir, perhaps, the form and constitution of the "matter" will be minutely described, and its potentiality exactly defined. By some searching beam, the bacterial life will be shown in process of development from the matter potential.

It does not appear that Dr. Tyndall has performed any experiments with the view of ascertaining whether, by the luminous ray, he can detect a moderate number, say a few hundreds or thousands, of minute bacteria within the microscopic limit suspended in fluid; nor does he tell us whether the beam had displayed bacteria that could not be found by the microscope; nor whether the searching beam distinguishes organic matter from inorganic matter, and dead from living bacteria. By the microscope, some information in respect of particles within the microscopic limit may be gained upon these and some other points; but the beam is for particles of another order, and "potential"—whether in all cases to develop life is not decided. In fact, it seems just possible that the ultra-microscopic particles in question may have no more to do with the development of bacteria than their gigantic representatives which make up the greater part of the organic and inorganic particles of the dust of our air.

Dr. Tyndall remarks, at the commencement of his paper, that his

* Towards the end of his paper, however, Dr. Tyndall does say, with respect to one infusion of turnip, that "no amount of searching with the microscope could (did it) reveal in it . . . the trace of a *living* bacterium". He does not tell us what magnifying power he searched for the trace of a "living bacterium", or what the "trace" is like, or in what particulars the "trace" of a *living* differs from the trace of a *dead* bacterium.

"method" has "not been much turned to account"; but it seems not to have occurred to him that it might have been desirable to compare the results of his new method with those of the method resorted to by *every previous observer*, and still relied upon. Why does he not have the fluids in his one hundred and thirty-nine flasks—his cloud of sterile witnesses—or some of them, carefully examined by some one familiar with the use of the microscope in such investigations, and the results of the examination published? Others would then, perhaps, put their trust in the searching beam. It is quite certain—however Dr. Tyndall may mistrust the microscope—that the careful microscopic observations of previous observers alone render the production of such a paper as his a possibility. All the searching beams in the world could not imprint the image of a bacterium upon the human retina, or render it evident to the mind of man. What the searching beam can do has yet to be proved; and the very long list of different organic substances used by Dr. Tyndall will merely astonish the uninformed mind. Dr. Tyndall knows, as well as any one, that experiments may be varied and multiplied without their real value being increased in the least degree. Twenty flasks might have carried conviction as well as five hundred. If in not one of twenty, or even of ten, could a bacterium or allied organism be detected by proper microscopic examination, many of us would feel convinced that Dr. Tyndall had obtained results differing from those of other experimenters, whatever the explanation might be; but, as it is, we are left in doubt. His results *may* really confirm conclusions already arrived at; and he may have again to exclaim, amid the confusion and uncertainty besetting the doctrine of spontaneous generation, "to err is human".

The Fellows of the Royal Society and many others may be disposed to agree with M. Pasteur in the opinion that spontaneous generation is "a chimera"; but very few will admit that *pronouncing* supersedes the necessity, or in any way assists the process of *proving*; and it is evident that Dr. Tyndall thinks a large "cloud of witnesses" necessary to convince. The "undoubting conviction that, this being so, it is possible to remove parasitic diseases from the earth" is then expressed; and Dr. Tyndall adds, this question is, therefore, of the last importance "to the medical profession, and, through them, to humanity at large". But he adds: "the state of medical opinion regarding it is not satisfactory". It is to be hoped it may be long indeed before medical opinion may become what Dr. Tyndall considers "satisfactory". The conclusion about the removal of parasitic disease from the earth is inconsequent. In the first place, what is wanted is not that "spontaneous generation" should be *pronounced against*, but that the truth should be ascertained; or, failing that, that the probabilities for and against should be accurately stated and balanced. But, secondly, if spontaneous generation were a fact, the extirpation of parasitic diseases would be just as conceivable as in the opposite case. Thirdly, whether spontaneous generation be a fact or not, if the active agents in propagating certain contagious diseases be bacteria, it is quite certain that these diseases will never be extirpated, for the extirpation of bacteria is not conceivable. Lastly, the doctrine that contagium consists of bacteria is opposed to broad and well known facts, as I will endeavour to show in another short paper, if space can be found.

ON THORACENTESIS.

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I SHOULD be glad to supplement the interesting and instructive article on this subject in the JOURNAL of February 12th, by some reflections which my own recent experience of this operation has suggested. It has so happened that, during the past year, a considerable number of cases of pleuritic effusion have come under my care in King's College Hospital, many of them having been transferred thither from my outpatient practice at the Brompton Hospital. I have had as many as three patients in the hospital at the same time, in whom the operation of thoracentesis had been performed. Any one who saw these patients operated upon would find it difficult to realise the serious dangers which, we are now told by some authorities, attend this operation.

That serious dangers do exist in certain cases, is amply set forth in the able *résumé* contained in the JOURNAL of the conditions which affect the heart and lungs both before, during, and after aspiration. But it must be borne in mind that in many cases in which the operation is undertaken, it is adopted as a last resource, and in order to rescue the patient from some of those very dangers which may even prove fatal during or immediately after the operation; whereas, in recent cases, where no unfavourable symptoms have been observed before the opera-