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."

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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 out-patient 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-