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-

tion, or the existence has been discovered of any suspicious history, I believe that scarcely any operation can be pointed to which is alike so simple, so safe, and so successful.

It is, however, important, even in the most favourable cases, that the operation should be performed with as much care and gentleness and caution as are possible; whereas, I fear, a little haste and roughness occasionally intrude into the management of these patients in hospitals. In all my own cases, I have, in the first place, made a point of using a perforated needle of small size, so that the outflow of fluid is slow, and time is given for the organs within the chest to accommodate themselves gradually to the changes of pressure. The occurrence of pulmonary oedema, as well as of albuminous expectoration, is probably due to the too sudden afflux of blood into vessels which have long been compressed, and whose walls have therefore lost, in part, those physical qualities which render them impervious under ordinary circumstances to the albuminous constituents of the blood. If, however, the lung be allowed to expand slowly, time is allowed for the vascular walls to recover their normal physical relations to the contained fluid. Nothing is gained, but the time of the operator, by rapidly evacuating the fluid through a trocar of considerable size, while it is likely to favour the dangers which have been mentioned, and which I need not repeat.

I have long adopted the plan, which is mentioned in the JOURNAL, of enveloping the chest, after the withdrawal of the fluid, with a broad band of plaster nine or ten inches wide; but I wrap it tightly round the entire chest, and so provide a much more secure and effectual support than is given by applying it to the affected side only. I believe this application to be most useful.

I am in the habit of stopping aspiration immediately on the occurrence of any troublesome cough; and I object to the removal of more than fifty or sixty ounces of fluid at the first operation.

The production of local anaesthesia, as suggested, is too frequently neglected, and a needless amount of pain and terror is thereby inflicted, especially in the case of female patients, who generally dread the operation very greatly; a predisposition to syncope is thus engendered. Most of the cases I have had under my own care have made complete and rapid recoveries. The only fatal case I have had was one in which, after a first puncture, a considerable quantity of pus was evacuated; and, on its rapid reaccumulation, the patient, who was a very nervous person, refused to submit to any further operation except under chloroform, which was accordingly administered, and a drainage-tube passed. She recovered from the operation, and appeared somewhat relieved, but she died rather suddenly a few days afterwards. On *post mortem* examination, the lung on the affected side was found compressed into a thin, hard, flat, tough and dark cake, which was firmly bound down to the upper and back part of the chest.

Another unpromising case was that of a cachectic looking man of middle age, in whom the course of the disease had been exceedingly latent, and who had never suffered, apparently, from any acute illness; on aspiration, we removed about fifty ounces of dark hæmorrhagic fluid, with great relief to the dyspnoea, which was the only symptom of which he complained. He made a fair recovery, and the compressed lung expanded, but we found the physical signs of consolidation (probably tubercular) of the upper lobe continuing after the aspiration. He returned some months after as an out-patient, complaining of cough, emaciation, and dyspnoea; but there was no evidence of any great reaccumulation of fluid; he continued to follow his occupation, and objected to come into the hospital again "until he felt worse". He has not appeared recently, and in all probability he has succumbed to the original pulmonary disease to which the pleuritic effusion must be regarded as secondary.

This case and another, the particulars of which I will immediately mention, lead me to call attention to the impropriety of evacuating by aspiration those pleuritic effusions which arise as secondary to tuberculosis of the lung and pleura. I believe it is unwise to interfere with these secondary effusions, save for the purpose of avoiding impending suffocation, when they are very considerable. The case to which I have just alluded was that of a young man who, twelve months before coming under observation, had had an attack of hæmoptysis, accompanied with other symptoms, which might be regarded as pointing to the existence of phthisis. These symptoms had disappeared, but he had now a considerable accumulation of fluid in the left pleura. He was aspirated, and between sixty and seventy ounces of fluid were withdrawn. The compressed lung re-expanded perfectly, and he made a quick and excellent recovery; but, within a month of his dismissal from the hospital, he reappeared with all the evidences of rapidly progressing phthisis. There can be little doubt that, in this case, the compression of the phthisical lung by the pleuritic effusion had arrested the progress of the tubercular disease, which, when the fluid was withdrawn, developed rapidly. I should always hesitate to recommend thoracen-

tesis for the removal of pleuritic effusion, where there had been any distinct and reliable history of the previous existence of symptoms of phthisis.

With regard to washing out the pleura after operation for empyema, and the best methods for doing it, and the best solutions to use, the whole subject is elaborately discussed by Dr. Fraentzel of Berlin, in his contribution on Diseases of the Pleura in Ziemssen's *Cyclopadia*, a translation of which will be found in the forthcoming volume of the Anglo-American edition of that work. Dr. Fraentzel describes in considerable detail a method of washing out the pleural cavity by means of two Nélaton's catheters, which he has found eminently successful. The description is too long to reproduce here; but, as the next volume is to appear in March, those who take an interest in this operation will be able to refer to it themselves.

ON DEFECTS IN THE DOMESTIC MEASUREMENT OF DOSES.

By ROBERT FARQUHARSON, M.D.,

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THE scrupulous accuracy with which prescriptions are made up by the highly educated dispensers who are now entrusted with this duty, perhaps tends to make practitioners careless regarding the exact mode of administration of their drugs. For convenience in out-patient departments, the time honoured teaspoon or tablespoon is invariably recommended for the division of doses; and we console ourselves for our want of scientific precision by the belief that these domestic measurements usually correspond to certain definite standards laid down in our text-books. Sometimes, however, we find complaints made that our medicine will not last out the week; and, on inquiry, we find, that the patients have consumed nearly double the proper quantity during that time, although they have conscientiously endeavoured not to exceed the regulated number of tablespoonfuls. This happens so often, and is productive of so much waste and annoyance, that I have lately, with the kind assistance of Mr. Marriott, dispenser to the Western General Dispensary, made a careful inquiry into the capacity of our common domestic measures with the following results.

I will first consider drops which are in occasional use, and which are held in a vague way to be about equal to a minim, although the more educated student of pharmacy would probably tell us that, on an average, they are only equal to half that quantity. Of

Distilled water	...	...	56 drops represent	60 minims
Tinctura opii	...	...	113 "	60 "
Tinctura digitalis	...	...	114 "	60 "
Liquor morphiae hydrochlor.	...	...	100 "	60 "
Oxymel scillae	...	...	80 "	60 "
Syrupus papaveris	...	...	75 "	60 "
Glycerine	...	...	45 "	60 "
Spiritus ætheris nitrosi	...	...	114 "	60 "
Tinctura camphoræ comp.	...	...	112 "	60 "
Acidum sulphuricum dilutum	...	...	55 "	60 "
Oleum ricini	...	...	55 "	60 "
Spiritus terebinthinæ	...	...	110 "	60 "
Oleum anisi	...	...	100 "	60 "

The next point is to inquire in how far the size of the drop is influenced by the shape of the bottle in which the fluid is contained. The experiments just noted were all made by means of an ordinary beaked minim-measure, and I found barely a shade of difference between this and a small phial brought by a dispensary patient. But, on making comparative observations with tinctura opii, I obtained the following variations from my original estimate of 113 drops to 60 minims. Out of the ordinary surgery bottle, I obtained 83 drops to 60 minims, and from a large twelve-ounce bottle in the possession of a patient 93 drops became the equivalent; and, although these differences are considerable enough in themselves, the bottles in ordinary use differ but little from one another, and it will therefore be seen that the quality of the fluid is the main factor in regulating the relative size of the drops.

But drops after all are comparatively seldom used, and the universal engine for the division of doses is the tablespoon, which homely implement is always at hand, and is held to represent a certain fixed quantity. In a series of seven selected at random, and apparently differing very widely in shape and in size, I discovered the following remarkable variations. No. 1, contained six drachms; No. 2, six drachms; No. 3, eight drachms; No. 4, eight drachms; No. 5, eight drachms; No. 6, five and a half drachms; No. 7, five drachms.

It was found that a difference of from one to two drachms is made