state, for reasons which I have pointed out, and which appear to me conclusive. I believe it is admitted, even by the supporters of the old cell-theory, that the fact of conjugation is indisputable; and, as I said before, I am prepared to admit that fusion may not be impossible amongst molecules; but I may perhaps be excused if I remind Dr. Bennett that an admission of the possibility of the truth of a proposition is a very different thing from an admission that a particular instance is the proof of it.

Dr. Bennett refers me to his answer to Dr. Beale in the *British Medical Journal* of Feb. 28th for the reasons why he considers vibriones an instance supporting his theory. I had before and have now again considered the arguments there shown, and have endeavoured to reconcile them with my own observations, but I confess that I have been unable to do so, nor can I see anything to support his views. For Dr. Bennett says in one of his lectures that first of all, in the formation of tissues, we have a fluid state; next a molecular state, which is produced by precipitation from that fluid; after and from these molecules the various tissues.

Now I cannot find from his account that these molecules fulfil these requirements, for in no part is it mentioned whence they arise; and yet in any case and on any theory it is highly important that their origin should be determined. I think, however, everyone will admit that they arise by one of two modes: either, 1st, by a rearrangement of the elements of the decomposing fluid—that is, by spontaneous generation; or, 2nd, by being deposited from the air without. Concerning the former, I do not gather that Dr. Bennett holds any such theory, and consequently need not allude to it further. We will therefore consider them as derived from without—that is, deposited from the air.

It will be apparent that they have their origin in some definite source. What origin? This, it appears to me, is the essential portion of the inquiry; because much of the value of their mode of behaviour hereafter depends upon what they are, notwithstanding their minuteness. How, I ask, are we to arrive at the knowledge of all that they are unless we can trace their derivation?

I think few will disagree with me when I say they must have arisen (that is as far as our most extended theories help us) either, 1st, as a granule from a fluid, as I have pointed out Dr. Bennett requires as the starting point of his theory; or, 2ndly, as a mass separated from some old cell which has broken up; or, 3rdly, by the more formal process of segmentation.

Of the first it is plain we have no proof whatever; and as the second and third do not fulfil the necessities of the molecular theory, I maintain that as far as these vibriones are concerned the molecular theory is *non-proven*. If it is insisted that it is so, then, I reply, only by "spontaneous generation," to which conclusion, as I remarked before, I do not think Dr. Bennett intends to bring it.

Which explanation shall we accept? For myself there appears no difficulty. We shall, if I mistake not, find a solution by watching both the formation of vibriones and comparing it with the mode adopted by vegetable cells in the progress of their development from the single globular cell to that of filament.

This, as I remarked in a former letter, I have already brought forward in the *Microscopical Journal* while on the Diamorphosis of *Lyngbya*. But I may here again repeat an abstract, the more so as further observations upon kindred growths have quite confirmed them.

It is as well that I should state here, that whether we are agreed upon the exact structure of these minute masses of vegetation,—whether we assert they have a cell-wall or not,—there is no doubt but that they increase in size by ordinary growth; neither is there doubt that they have the power of segmentation; and further, that both these processes can in varying degrees co-exist. I mention this because Dr. Bennett makes remarks in the letter referred to which would imply that he was not aware of it, at any rate in its full force; for instance, he says: "If the molecules divide, how is it that filaments form at all?" If both these properties be not granted to vegetable cells, and to these vibriones also, then, of course, I can proceed no further. We disagree completely on the premises, and our arguments would not bear on each other. But if these are granted,—and I suspect they will not be denied,—then the explanation of the appearances seems to be only in accordance with the every-day appearances of the growth of gonidia, of filamentous vegetations, and the laws of filamentous growths. I know nothing in these vibriones at all dissimilar.

The mode of growth in these vegetations is this: A cell continues to segment till the size of the divisions may be reduced

to an exceedingly minute body, cell, molecule, or whatever it pleases others to call it. When so small it is very difficult, even with high powers, to determine whether there be a cell wall or not. I am inclined, however, to believe that there is only an increased density on the external surface of the homogeneous mass of protoplasm. Watch these molecules. Some increase in size, and gradually resume the parent form, a cell with granular contents, nucleus and all—an orthodox cell; others become oval; others elongate to many times their breadth, but sooner or later one or more dissipments appear, and a jointed filament is the result. These segments sooner or later redivide, and thus we have varying appearances, dependent upon the relative frequency of segmentation compared with the power of linear growth. This can be seen by anyone patiently watching these cells, and in many others under conditions imitating natural conditions. Dr. Bennett will, I think, find that while this is the law by which filamentous formation and growth is governed, it explains all his difficulties as to the form and size of the bodies he has depicted in his woodcuts. He must also recollect that filaments have a power of reverting to the single globular cell, or gonidium, at any time during its life, that it exercises this function somewhat capriciously, and thus we have the moniform appearance which is the commencement of the process of separation, which, however, may rest there, or proceed to final separation. The cells thus separated may directly segment by the binary form, or develop into filaments, and so we have growth in all stages and varieties. I believe this is the answer to Dr. Bennett's question, "How he (Dr. Beale) explains the formation of filaments I do not know."

I may also ask—If a jointed filament is to be produced from a round cell or mass of vegetative protoplasm, what plan is simpler and more in accordance with the known laws of vegetable cell-formation? As far as my own observations have extended, I have never in these phases of cell-growth seen anything like fusion, either to produce a globular or filamentous form. But, even had I done so, I see no *proof* of the molecular theory in it, because it has not been shown that it has just been precipitated from a fluid, as is required by the theory; and if these small bodies are the result of segmentation, then its coalescence would be rather a conjugation—at least a very different condition from coalescence of granular matter recently precipitated from a fluid in the midst of living tissues.

Hence I maintain that these vibriones are no proof whatever of Dr. Bennett's molecular theory. If he will insist that they are, then I again urge that it can only be by the help of another theory—namely, spontaneous generation. Will Dr. Bennett ask its assistance?

Dr. Bennett has also read my remarks on uterine fibres rather hastily. When I pointed out its action, not only during the whole of pregnancy, but also in the unimpregnated state, he objected that the latter was a new fact in physiology; upon which I assured him it was not so to those who had opportunities to view its action, and referred him to a published opinion. His rejoinder is, "It is new—in the normal state." As I made no qualification on this point, his answer admits the truth of my remarks. However, I will again join issue and say, "Even in the normal state." Perhaps the labour-like pains of dysmenorrhœa might suffice as an instance.

J. BRAXTON HICKS, M.D. Lond., F.R.S.

Wellington-street, London-bridge, April, 1863.

THE EXAMINERS OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND.

To the Editor of THE LANCET.

SIR,—I have read with much interest Mr. Heath's letter concerning the examiners of the Royal College of Surgeons of England, and the numerous replies it has called forth; and as an old Edinburgh student, and one who has undergone the examinations here and in London also, I have thought that a Scotch opinion of the London examiners, at this their time of trial, would not be out of place.

The general opinion here is, that the examiners in London are—and I can assert it without fear of contradiction—sharp, practical men, quite at home with their subjects, and simply desirous of testing the student's knowledge of those practical points which he must, in some way or other, acquire before ever he can practise surgery with success. Of course they possess the natural quickness of speech and demeanour common to their countrymen, which, though it may tell against us Scots, who think faster than we speak, can have little effect upon the English candidate, who is quite accustomed to it.

I do not intend to deny that we in the North are ignorant of the name of Mr. Caesar Hawkins and his alleged peculiarities; and in all the cases of rejection (with which I am acquainted) of Edinburgh students, the failures have been attributed to the decision of this gentleman; but in all these cases I have also found that there have been fierce (very fierce in some cases) disputes about mere words between examiner and candidate, and we all know that some examiners cannot bear in the slightest possible degree to be corrected. What one of my Edinburgh readers does not know of a certain famous professor who, as he pleases, will make the wretched candidate confess to some favourite doctrine in pathology or medicine? I think my English friends have no reason to find fault with the examiners at the College of Surgeons of England, as more severe are not very difficult to be had. I see no reason to alter the opinion expressed by the late Albert Smith, "that the inquisitors are altogether a gentlemanly set of men, who are willing to help a student out of a scrape rather than 'catch question' him into one."

I have thus, at the request of a number of students and practitioners, given you our opinion of these gentlemen; and we take this opportunity of bearing testimony to the kind and gentlemanly treatment we received at the hands of the examiners of the Royal College of Surgeons of England.

I am, Sir, your obedient servant,
Edinburgh, May, 1863. M.D. Edin.

I enclose my card.

To the Editor of THE LANCET.

SIR,—The correspondence now going on with reference to the overbearing conduct of the examiners of the College of Surgeons of England induces me to call your attention, and that of your readers, to the "bumptiousness" of the Council itself, as exhibited in my case.

In possession of three medical diplomas, I found it desirable to obtain a surgical licence, and my papers being some lost (possibly) and in a far distant city, I begged the College to accept my diplomas as vouchers for my having gone through the required curriculum. On speaking to the President, in company with the Secretary, the latter gentleman (answering, I presume, for the Council) replied that the certificates must be produced. I offered to procure duplicates of the certificates of the various professors for all the necessary lectures, and pointed out that the obtaining of either of my medical degrees necessitated the requisite amount of hospital attendance. But the Secretary stated that candidates for medical diplomas had only to produce certificates of clinical medical, and not of clinical surgical, attendance. I then reminded him that all hospital certificates ran to this effect—"has attended the clinical medical and clinical surgical practice," &c., and that the one is never mentioned without the other. To this the reply was, that this fact was well known to the College, but that they chose to take technical advantage of the fact that medical corporations did not require all the clinical surgery mentioned in the certificates presented.

What may be the object sought by the Lincoln's-inn gentry in refusing gentlemen similarly circumstanced to myself—for I presume mine is not a solitary case—it is hard to tell; the result is, of course, that the Colleges of Edinburgh or of Dublin pocket the fees and issue the desired licence, and one in all respects equal to that of the London body. I think, Sir, that it would be well that the professional public should, by expression of their opinion, form a tribunal to which even the mighty College of Surgeons of London must bow, and I submit the case for this purpose only.

May I also point out, through the medium of THE LANCET, a fact with which I doubt whether very many candidates for surgical licences are acquainted—namely, that the Royal College at Edinburgh issue a licence to practise midwifery to every successful candidate for their diploma. Surely this is a step in the right direction, and one which it would be advisable for the others to follow, instead of demanding extra fees for the certificate of a branch of the profession which every surgeon is called upon to practise. In addition, I can truly say that a more courteous Court of Examiners I had never the pleasure of meeting, and I have had to appear before six.

I am, Sir, yours obediently,
May, 1863. W. T.

To the Editor of THE LANCET.

SIR,—In reference to the correspondence brought forward in your journal of Saturday last regarding the examinations at the College of Surgeons, and the conduct of the examiners, espe-

cially that of Mr. Hawkins, I have to inform you that I recently passed the first professional examination, and was examined by Mr. Hawkins, to whose gentlemanly and courteous behaviour I have much pleasure in bearing the highest testimony. I must candidly admit that the examination is very severe, but I am sure that no student who is properly prepared need fear any questions put to him. I therefore beg to corroborate the statement of your correspondents, "J. T. J." and "A Third-year's Student;" and am glad to say that I am acquainted with many who bear out its correctness.

I am, Sir, yours obediently,
May, 1863. A GLASGOW STUDENT.

To the Editor of THE LANCET.

SIR,—I was very much surprised at the appearance of Mr. Heath's note in your impression of the 9th. When I passed the College of Surgeons in 1856, I could not help remarking the gentlemanly and kind treatment paid to me on that occasion by the whole Court of Examiners. Mr. Caesar Hawkins, in particular, was most courteous on the evening I had the honour of having the membership conferred on me.

I quite agree with your correspondent, "A Third-year's Student," that if a student is well prepared he need not fear any of the examiners.

The injustice done to Mr. Hawkins, and the importance of a thorough ventilation of the subject, will, I hope, be an apology for troubling you.

I am, Sir, yours &c.,
J. C. B. SMALLMAN, M.D., M.R.C.S. Eng., &c.
Willingham, Gainsborough, May, 1863.

To the Editor of THE LANCET.

SIR,—As I had the pleasure of being examined in surgery at the College by Mr. Caesar Hawkins in January, 1861, will you allow me to state that no examination could have been conducted in a more gentlemanly or considerate manner. I am satisfied that many who were at that time my fellow-students formed the same opinion of Mr. Caesar Hawkins's unimpeachable character as an examiner.

I am, Sir, yours respectfully,
May, 1863. BARTHOLOMÆUS.

DR. MURCHISON'S WORK ON FEVER.

To the Editor of THE LANCET.

SIR,—My attention has been directed to a misprint in my work on Fever. At page 471, where the characters of the eruptions of typhus and enteric fever are contrasted in a tabular form, the distinguishing characters under the head of No. 8 have been accidentally printed in the wrong columns. It is the eruption of typhus which appears on the fourth or fifth day of the fever, and that of enteric fever which rarely appears before the seventh day. Although these facts are clearly indicated at pages 117, 131, 457, and 470 of the work, a casual reader might carry away an erroneous impression from merely referring to the table in question. As the point is of some importance, I beg those of your readers who possess the book to make the necessary correction. A note pointing out the error will be inserted in all copies of the book sold after this date.

I am, Sir, your obedient servant,
Wimpole-street, May, 1863. CHARLES MURCHISON, M.D.

ASSISTANT-PHYSICIANS AT KING'S COLLEGE HOSPITAL.

To the Editor of THE LANCET.

SIR,—It is a curious fulfilment of your prognostications respecting the appointments at King's College, that the Council should have appointed as assistant-physician for diseases of women and children a gentleman who is neither a physician nor a graduate in medicine; for Mr. E. Day is not a M.B. Lond., notwithstanding the formal announcements to that effect which have appeared in the public journals.—Yours obediently,

May, 1863. A LOOKER-ON.

INFORMATION SOUGHT BY THE FRENCH AUTHORITIES.—M. Jaccoud, professor at the Paris Faculty, has been entrusted with a very interesting medical mission. He is now, by order of the French Government, at Vienna, where he is examining the medical schools and all the institutions connected with the teaching of medicine. He is to visit the whole of Germany for the same purpose, and report to the medical authorities at home.