anterior or middle lobes outside of the supposed voluntary motor apparatus, and also a lesion in the optic thalamus, in the cerebellum, &c., even where no pressure can be exerted on other parts of the brain by the diseased tissues, can produce paralysis, and not rarely even a complete and persistent one. So it is with convulsions, which we find to be caused just as well by disease of supposed non-motor as by lesions of the supposed exclusively motor parts of the brain.

The second argument I will use is the counterpart of the preceding; it is that *all sorts of morbid alterations, irritative or destructive, occupying the whole or a part of the cortical motor zone or any other part of the encephalic voluntary motor apparatus, may exist without producing either paralysis or convulsions.* In my third lecture on Paralysis, which will soon appear in THE LANCET, I will give a great many facts showing that paralysis may not be produced even in cases of extensive disease of either of the three lobes, or of the ganglions, or of the base of the brain. I will only now, without giving details, state that there are on record cases of destruction or great alteration of the so-called psychomotor centres on one or on both sides without any marked paralysis. Indeed, there are most important cases of considerable alteration of the cortical substance, not only of that of those parts of the anterior and middle lobes considered as containing the motor centres, but also of that of almost all the other parts of the two cerebral hemispheres, without any evident paralysis. Such are cases published by Gama, Lenormand, Billard, Lécorché, Chambert, Abercrombie, Dr. Elam, Brière, R. Bright, Belhomme, Simms, &c. To these cases of disease of almost all the convolutions on both sides of the brain I might add cases in which a considerable pressure was exerted on the surface of a great part of the two cerebral hemispheres, or of local alteration in a great many parts of the convolutions, without any marked paralysis. Such are cases given by the following observers, some of whom believe in localisation: Sir William Gull, Professor P. Broca, Huguenin, Marcé and Luys, Rogerson, Dugast, Gintrac, &c. As regards convulsions, there were none in most of these cases, so that it is clear that a considerable alteration of the pretended psychomotor centres may exist without any marked paralysis or convulsions.

A considerable alteration, and in a great measure a destruction, of tissue, with more or less extensive irritation of surrounding parts, may exist in two lobes, or in almost the whole of one hemisphere, without any marked paralysis or convulsions. Cases showing this are more numerous than anyone who has not specially examined the subject may be ready to admit. I shall soon publish some details of a number of those facts. I will simply now name the following authors who have put them on record. As regards cases of disease of a great part of one hemisphere without paralysis, I will name the following observers, some of whom are very distinguished men:—Dr. H. Day, Abercrombie, Porta, Dr. R. Quain, Thibaut, Pasquet, De Fleury, Dr. J. W. Ogle, Anger, Alègre, W. Roberts, Porral, Alex. Thomson, Pédelaborde, Prof. Richet, Spessa, Prévost and Cotard, Boudet, Lagrange, Mr. Callender, Robert, &c. In most of those cases no convulsions took place. I could lengthen this list considerably, especially if I added to it the names of authors having published cases in which there was but an incomplete paralysis notwithstanding an extensive atrophy of one hemisphere.

I will merely say also, that *disease of the two anterior or the two middle lobes, of the two corpora striata, of the pons Varolii or the medulla oblongata, as also disease of one of the crura cerebri, can exist without marked paralysis or convulsions.* I will only name now some of the authors who have published cases of alteration of the two anterior or the two middle lobes, or the two corpora striata, without marked paralysis. In some only of those cases convulsions have been noticed. I will first give a list of authors of cases of disease of the anterior lobes: Mr. Prescott Hewett, Velpeau, Dr. Banks, Berger, Berghmann, Campana, Gama, Dr. Jamison, Sir G. Burrows, W. Roberts, Dr. A. Leith Adams, Bosc, Cordier, Marcot, Breschet, De Castelnau, J. H. Drawbridge, Trousseau, Avonde, Blaquière, and Baraduc.²

² No doubt in a number of these cases of disease of the two anterior limbs the ascending frontal convolution may have escaped; but there is no doubt also that the disease, in some cases at least, extended to, and even went beyond, the fissure of Rolando, so that a good part of the pretended motor zone was injured or destroyed.

The following is a list of authors who have recorded cases of disease of great parts of the two middle lobes or of both corpora striata, without any marked paralysis, and generally without convulsions: Broussais, Collin, Carron, Siredey, Lallemand, Fabre de Puch, Lesserré, Mr. Jonathan Hutchinson, Troisier, Abercrombie, Durand-Fardel, Andral, Dr. J. H. Bennett, Gintrac, Dr. J. W. Ogle, Aran, Dubruel, Müller, and others.

The third argument is, that *local paralysis or convulsion (face, arm, or leg alone) may appear, whether the lesion is in some part of the voluntary motor apparatus or outside of it.* Indeed, the number of cases in which there has been local paralysis of either the face or the arm from disease of parts outside of that apparatus is greater than the number of cases of these two kinds of local paralysis caused by a lesion of the so-called psycho-motor centres. The same thing, as regards the arm and the leg, is true also for convulsions. I may go farther, and say it is a question whether local or general convulsions in cases of disease of the cerebral convolutions do not always depend, in a great measure at least, on the irritation of the meninges.

The fourth argument is, that *paralysis or convulsions of the arm alone or chiefly, in cases of disease of the convolutions, may appear when the lesion occupies the seat of the pretended psychomotor centre for the leg.* There are many cases showing this.

The fifth and last argument is, that *unilateral paralysis or convulsions may appear on the side of the lesion in the brain.* This I have fully shown as regards paralysis.³ I will merely say now that unilateral convulsions on the side of the lesion are extremely frequent. If, when the lesion is in the left half of the brain, cross convulsions exist in most cases, we find, on the contrary, that the number of cases of *direct* convulsions is much larger than the number of cross convulsions when the lesion is in the right side of the brain. Out of 322 cases of unilateral convulsions from brain disease that I have collected, there are 184 allied with lesions in the right hemisphere, and 138 with lesions in the left. Of the 184 there are 110 cases in which the convulsions occurred in one or in both limbs on the right side, and 84 in which they took place on the left side; so that it is clear that unilateral convulsions appear much more frequently on the side of the lesion than on the opposite side when the right side of the brain is the seat of the lesion. Not so when the left half is diseased; as, out of 138 cases, the left limbs were convulsed only in 31 cases, and the right limbs in 107 cases.

I might bring forward many other decisive arguments against the views that paralysis and convulsions may show where there is disease in the convolutions of the brain or in the radiation from these superficial parts towards the crura cerebri, but I trust that what I have said is quite sufficient. I hope that no surgeon will venture to open the cranium, and make a patient run the risk of a traumatic meningitis or septicæmia, on the mere suspicion that if he is attacked with paralysis or convulsions localised in the arm or the leg, or the face alone, after a blow on the head, it is that some lesion exists along the fissure of Rolando or its neighbourhood. I hope also that the place of application of the trephine, when it is found necessary to make use of that surgical means in a case of fracture of the skull, will be selected according to the classical rules for such an operation in such cases, and not on the more than doubtful significance of symptoms that may depend on disease located not only very far from the place where it may be supposed to exist, but on the other side of the brain than that of the fracture.

Believe me, Sir, yours truly,

C. E. BROWN-SÉQUARD.

Lewes-crescent, Brighton, July 14th, 1877.

THE UNIVERSITY OF LONDON AND MEDICAL WOMEN.

To the Editor of THE LANCET.

SIR,—As the first paragraph of your leader of Saturday last seems to have been written under a grave misapprehension of the intentions of the Senate in regard to the conditions under which they have resolved to admit women to degrees in medicine, I must request your permission to be allowed to give a correct statement of the case.

The Senate, in adopting the report to which you refer,

³ See THE LANCET of January, 1876, and of April and May, 1877.

committed itself absolutely to the principle that the existing regulations in regard to medical degrees shall apply without distinction of sex, in accordance with the power conferred by the Act 39 and 40 Vict., cap. 41. You, on the other hand, impute it to the Senate that it is their intention to modify these regulations in the case of women; such modification having reference in the first place to the admission of certificates of study in foreign medical schools; and, secondly, to the admission of women who are already in practice, or who commenced their medical studies before January, 1875, under less stringent conditions than those who may hereafter enter.

The existing regulations (Calendar, p. 138) provide that every candidate for the M.B. degree shall have studied during four years at one or more of the medical institutions or schools recognised by the University; one year, at least, of the four to be spent in one or more of the recognised institutions or schools in the United Kingdom. This regulation, allowing every male candidate to spend *three* of his *four* years of medical study in a foreign school, has existed from the very commencement of the University. And though no list of recognised foreign schools has ever been published, it has always been understood that certificates would be accepted from Paris, Berlin, or Vienna, the only foreign schools from which, in my twenty-one years' experience, such certificates have ever been presented.

Thinking it not unlikely, however, that female candidates might present certificates from other foreign schools, and not wishing to take upon myself the responsibility of either accepting or rejecting such certificates, I suggested to the committee by which the report was framed the desirability of framing a list of medical schools from which certificates should be received; and this proposition has been adopted by the Senate. Any such list of recognised schools will be equally applicable to all candidates, and will be perfectly in harmony with the existing regulations.

So, again, the admission of the two classes of female candidates already referred to, under such remissions of the regulations as the Senate may deem fit, in regard to "places, order, and modes of study, and exemption from the matriculation examination" (*not*, be it observed, from any of the proper *medical* examinations), is strictly in accordance with the previous practice of the University in regard to male candidates. I send you the regulations which still remain in force (though they have practically died out), in regard to those candidates for the medical degrees of the University who were either (1) in practice prior to 1840, or (2) had commenced their medical studies in or before January, 1839. Each of these classes of candidates was exempted from the matriculation examination, and the conditions in regard to "places, order, and modes of study" were very considerably relaxed.

Dr. Storrar (the chairman of Convocation) came in under the first of these categories; Sir Wm. Gull and Sir Wm. Jenner under the second. Several such candidates were admitted to examination for the medical degrees during the earlier years of my registrarship.

It is obvious that women are now in the same position as regards the medical degrees of the University as men were when those degrees were first instituted in 1840.

Exemptions of a kind more or less similar were granted by the Senate when degrees in science were first instituted, and when the law degrees were placed on a new basis.

The determination of the Senate that no modification whatever shall be made in the regulations on account of sex is shown by its non-acceptance of the proposition to which reference is made in the second paragraph of your leader, the large majority of its members agreeing with you in opinion that "no one should become a possessor of a London University degree in medicine unless he or she has received an all-round medical education."

I remain, Sir, your obedient servant,

WILLIAM B. CARPENTER.

University of London, July 16th, 1877.

* * We have few comments to make on Dr. Carpenter's letter. Every statement in the first paragraph of the leader to which he alludes was derived from the report of the committee. Dr. Carpenter's letter shows that important modifications are to be made in favour of those women by whose agitation the dissension between the Senate and Convocation has been brought about. One inaccuracy occurs

in Dr. Carpenter's letter. He quotes Dr. Storrar's name as an instance of those who were exempted from matriculation because they were in practice prior to 1840, and then says that degrees were first instituted in 1840. Dr. Storrar became a graduate of the University in 1839.—ED. L.

VARIOLA AND VARICELLA.

To the Editor of THE LANCET.

SIR,—I feel much satisfaction in thinking that the profession is in any way indebted to me for the publication of Dr. Sharkey's most interesting case. When I read my paper before the Clinical Society, some fears were expressed lest the public mind might be unsettled from its present state of serene composure by the promulgation of such views, and some objection was raised to the expediency of restarting the discussion of a subject which had been finally settled many years ago. In dissenting from this line of argument, I took occasion to remark that nothing can be more important than to take out old beliefs occasionally and see whether they are not decaying under the influence of time. If anything occurs to shake our faith in the value of an old institution, let us boldly challenge its efficacy at once, and let the truth come out, unharmed, as it always must, from the ordeal. When Mr. Hutchinson produced his now classic paper on Vaccino-Syphilis, some timid conservatives were apprehensive of the public faith, but time has shown how much science has gained by this addition to our knowledge. I hardly venture to compare great things with small, but in this discussion on varicella and variola, we have secured, at all events, the possession of this very important piece of evidence from St. Thomas's Hospital. Here there can be no doubt that small-pox so rapidly followed varicella as to indicate clearly the working within the system of the two poisons at the same time. But may it not be possible that a previous attack of a specific disease may be so specially slight as not to interfere with the overwhelming influence of a subsequent stronger dose? or may it not, under exceptional circumstances, even assist the subsequent operation of the infection by leaving behind it some unexhausted germ? All that Hebra asserts is, that chicken-pox will not invariably breed true, but will occasionally revert to small-pox, from which it may originally have been derived; and this peculiarity, he holds, may also depend on the soil which receives the contagious germ. Roseola, or spurious measles, will reproduce itself in its own kind fifty or sixty times, and the sixty-first may, as I have seen, turn out to be a true case of the major disease. May not the same analogy hold good between variola and varicella?—and if so, Dr. Sharkey's paper does not effectually settle the question. Another point of interest in the case is the demonstration of the acknowledged fact that vaccination has no protective power in the absence of a fully-developed areola, and that it requires a clear start from the incubative stage of small-pox in order that this special indication of safety may be produced.

I am, Sir, your obedient servant,

Brook-street, July 16th.

R. FARQUHARSON, M.D.

CLUB REMUNERATION: A SUGGESTION.

To the Editor of THE LANCET.

SIR,—Early in last year (1876) I saw an opportunity of inducing my clubs to raise the annual payment per member from 4s. to 5s.

I was aware that in 1868 a committee of the Birmingham branch of the British Medical Association, under the able lead of Dr. Heslop, had inquired into this question, and had gained the support of the daily press to their own unanimous decision that 5s. should be the minimum sum accepted by the medical officers of clubs. In my own immediate vicinity Drs. E. T. and J. H. Tylecote had distinguished themselves by their advocacy of this and other reforms in sick assurance societies. I wrote, therefore, to a considerable number of my professional brethren in Birmingham and this district asking for a return of any clubs then paying their medical officer 5s. or upwards per head per annum. All replied courteously, and it was noteworthy that each, while regretting