CORRESPONDENCE.

MEDICAL EDUCATION.

SIR,—It often happens, even among professional friends, that we find the parents of a boy who has finished his school life in doubt as to the best course to follow in their choice of the plan and place for their son to learn all that is necessary for his quatification for the medical profession. When the parents live in London they generally have personal feelings and interests in the selection of the hospital, and the only question of importance is whether their son should prepare for the London M.B., the Conjoint Board, or the L.S.A.

We find parents generally looking forward to their son being independent of their help, financially, when the four or five years are over; and they calculate the expenses they will have to meet during that time and at the end of it, in fees for tuition, and the examinations which are to be passed at certain intervals, leading up to legal qualification and registration.

The most important question, however, they do not generally consider, and that is the boy's future life when they have done what they have thought the best they could for him. They may be led to go to rather more expense in sending him in for university training, with the idea that the qualification he may obtain will give him better chances of success in practice, and a higher value for his work. It is well for parents to be careful how they are misled by this idea. In many cases it would really be wiser to spend as little as possible on obtaining a legal qualification, and keep some reserve to give the young man something to start with, than to leave him with nothing but a university degree. The question of most importance is the great difference between the fitness of a young man to start on his own account, when he has finished his five years after university training, as compared with the more practical work he has to go through for the non-university diplomas. Of course if a boy shows great talent and has taken scholarships at school and some financial help thus comes to the parents, they do not hesitate to let him go in for a university career; but when a boy has only average ability and the parents have other sons to provide for, and are unable to do more than start the medical one in life with a qualification, it is a different matter. It is one of the principles of university training that the practical use of what is being learnt is of very secondary importance, and that the chief object in view is to train the mind, leaving the practical knowledge to be obtained elsewhere than at the university.

If a boy wants to be qualified for practice and in a position to earn his own living as soon as he has finished his medical course, and has obtained his registrable qualification, his education ought from the very first to have been directed to practical purpose. The whole question of raising or lowering the standard of education for any profession turns upon financial conditions, and where we see clearly that the ordinary practice of medicine is not one likely to attract young men with capital, it is well to realize that practitioners, required as they are by the great mass of the population, will not be found if the costs of education are so high that the investment of capital in education will not find attractions.

It may be admirable to take a high sentimental view of what a boy's life is to be, but human nature will not give into this kind of sentiment, and no practical good will come from any indulgence in it. If the public can be taught to appreciate well educated men and recognize their value, the article will be supplied, but if they regard doctors as necessary evils and treat them as in China, there is little hope of raising the standard.—I am, etc.,

London, July 5th. ROBERT LEE.

THE PROTOZOAN THEORY OF VACCINIA.

SIR,—With your permission, and in the interest of science, I would point out that in his commentary on the protozoan theory of vaccinia, the contributor of the article on Small-pox and Vaccinia in the BRITISH MEDICAL JOURNAL of July 5th, pp. 56, 57, has failed to give adequate consideration to the experimental evidence in favour of that theory.

Dismissing as beside the question the various admonitions which he considers it right to bestow upon the protozoan advocates, and to the expression of which he devotes so much of his space, let us look for a moment at the state of the facts.

With glycerinated lymph derived from the Halle Vaccine Laboratory v. Wasielewsky in 1899 started a series of inoculations from rabbit-cornea to rabbit-cornea. The act of inoculation consists in making a very slight oblique puncture so as to raise the superficial epithelium, with a needle charged with material from the previous case. The lesion is so small as to be hardly visible to the closest inspection, and the vaccine process runs its course in less than a week without the supervention of vascular or leucocytic processes. About the third day the vaccinal changes are at their height, and consist in a slight thickening of the epithelium so as to form an elevation about 3 mm. in diameter above the normal curvature. This is sometimes surrounded by smaller elevations of the same kind. The projections consist of swollen and vacuolated epithelial cells, containing the peculiar bodies with which the name of Guarnieri has been associated. These bodies have been shown by Hückel to be more numerous as the process goes on; he failed to find them earlier than the third hour *post inoc.* Several hundreds were counted by him in a series of sections made from a cornea fixed twenty-seven hours *post inoc.*, and the number goes on increasing until the height of the process is reached about the fourth day, after which they become gradually less visible owing to the supervention of necrotic changes in the containing cells. The bodies are highly refractive, spherical or irregular in shape, vary in size from the smallest visible granule to a mass 7μ or 8μ in diameter, and lie close to the nucleus, the membrane of which they cause by their pressure to bulge inwards. These bodies are found in the interior of seemingly healthy cells, and have nothing to do with the ballooning and reticular degeneration of Unna and others. E. Pfeiffer inoculated 96 rabbit and guinea pig corneas, Hückel 76, Gorini used 130 corneas for testing the purity of glycerinated calf lymph, v. Wasielewsky vaccinated no fewer than 203 rabbits in this situation. In all the specific bodies were found. They were also produced on the (relatively few) occasions when it was feasible to inoculate the cornea with small-pox material. From the work of Monti, E. Pfeiffer, Salmon, Hückel, Gorini, and others it appears that these bodies are not to be found in the cells of the normal cornea, and that no other known form of irritant —neither cantharides, croton oil, silver nitrate, glycerine, osmic acid, Indian ink, cocci from vaccine lymph, bacteria from other sources, myxomycetal spores, skin scrapings from measles, scarlatina, chicken-pox, foot and mouth disease, nor material from non-specific pustules on the teats of cows—evoked in corneal epithelium the appearance of the bodies, which are therefore justly considered to be "specific." Active vaccine or variola lymph alone is capable, so far as we know, of producing them; and the fact that they are not the result of some soluble toxin acting on the cells is proved by the circumstance that, as shown by Guarnieri, E. Pfeiffer, and Gorini, lymph that has been deprived of its activity by filtration loses its power of producing the bodies, whilst Bossalino and Gorini have shown that lymph that has either spontaneously lost its virulence, or has been deprived of it by heating to 60° for an hour is incapable of producing the bodies.

From the fifteenth and also from the twenty-fifth generation on the rabbit cornea, v. Wasielewsky, with every precaution against contamination with ordinary vaccinia, inoculated calves, and obtained typical vesiculation, and the production of active lymph. His experiments in vaccinating children from the rabbit cornea are of such importance that I will give them almost in his own words:

> The corneal epithelium of 3 rabbits that had been inoculated forty-eight hours previously with material from the forty-sixth generation was scraped off, rubbed up with glycerine water, and sealed in capillary tubes. A fragment of what remained in the mortar was used to inoculate a normal rabbit cornea, which, twenty-four hours afterwards, presented the characteristic bodies. Three hours after removal from the rabbits the material was brought down to the vaccination station, and used by Geheimrath Risel for vaccinating 7 children, six insertions being made in each case. This was on July 26th, 1899. On August 2nd, when the children presented themselves, the following was ascertained to be the result:
>
> On 1 child there was no vesicle.
> On 2 children there was 1 vesicle.
> On 1 child there were 4 vesicles.
> On 2 children there were 3 vesicles.
> On 1 child there were 6 vesicles.
>
> Thus out of 42 insertions 18 were successful, or a percentage of 42.8.

A previous experiment with lymph of the forty-third generation on the rabbit cornea had given an insertional success of 33 per cent. Will any one maintain that in the forty-seventh successive cornea lesion there still remains any trace of the original material used in starting the series? I think not. The only admissible explanation of the facts is that the vaccine producer, whatever it is, propagates itself in the inoculated cornea. Bacteria are absent. Some may say that they may be present though not demonstrable either microscopically or culturally. To this position I would reply in the words of the schoolmen: *De non apparentibus et de non existentibus eadem est ratio.* The organism of vaccinia cannot be of infinitesimal minuteness, for it is stopped to a large extent by a three-fold filter paper, and altogether by a Pasteur or Berkefeld candle. The corneal cells are so easily isolated and examined that an organism of such dimensions could not escape detection. The presence of the bodies is the only abnormality constantly visible in the specifically affected cells, and all the experimental evidence at present available is in favour of the view that variola and vaccinia are the result of an intracellular parasitism, the outward and visible expression of which is the appearance of the bodies in question. It is with the explanation of the true nature of these bodies, whether they are the actual parasites or protoplasmic changes induced by the parasites, that we shall have to concern ourselves if we wish to solve the problem as to the etiology of these diseases.—I am, etc.,

E. J. MCWEENEY, M.A., M.D., D.P.H.,
Bacteriologist to the Local Government Board for Ireland.

Dublin, July 8th.

GENU VALGUM.

SIR,—Mr. Morton's paper in the BRITISH MEDICAL JOURNAL of June 21st recalls to my memory a contribution of my own on the subject of genu valgum, made long before skiagraphy was, I think, even known, or at least used in the elucidation of surgical diseases.

In 1892, at the British Medical Association meeting at Nottingham, I read a paper, entitled, "Overgrowth of the Inner Tuberosity of the Tibia as a Cause of Genu Valgum, Independent of Elongated Inner Condyle of the Femur," which excited considerable interest, and was fully discussed by Messrs. Walsham, Noble Smith, Morton, and Braidwood, in which I demonstrated most conclusively, aided by a plaster cast of both legs of one of my patients, that the tibial enlargement was in a very considerable number of cases the sole cause of the deformity. After remarking that many surgical authorities made no allusion to this variety of genu valgum, I quoted Walsham, Holmes, Barwell, and Owen as having noticed the same, but not attaching great importance to it. Dr. Macewen called my attention to the following observation in his work on *Osteotomy:*

> There is another element found in about one-third of the whole number of cases consisting of an increase of osseous matter on the inner side of the tibial diaphysis at its proximal extremity, which causes the head to sit askew on the shaft. This tibial deformity when present is so to a small extent though in a few it forms a prominent feature.

This paper was published in detail in the *Provincial Medical Journal* for January, 1893, abbreviated in the BRITISH MEDICAL JOURNAL for February 18th, 1893, and again referred to in an abstract of paper read at the meeting of the British Medical Association at Bristol, 1894, when I showed two patients, respectively 6 and 3 years after operation, with perfectly satisfactory limbs, in the opinion of the members present. I refer your readers who are interested in the subject to the *Provincial Medical Journal* of the date mentioned, but if inaccessible to any one wishing for a copy I shall be happy to send one on application.

Although the value of skiagraphy in dealing with obscure and deep-seated injuries and deformities cannot be overestimated—and I willingly admit the greater accuracy and elegance of Mr. Morton's skiagraphs compared with my crude photographic productions—yet it is important to notice that the same results have been obtained by myself and others by clinical observation and careful measurements long antecedent to the application of skiagraphy. More than twenty years since, after operating by Ogston's method on a case of genu valgum, I found the deformity was only imperfectly corrected, and on further examination I discovered the enlargement of the inner tuberosity of the tibia, and carefully removed a considerable wedge of bone with the best results. In my paper I compared the methods of wedgetaking and simple sections, and in my judgement nothing but removal of a wedge from the head of the tibia can produce good results; and it is a serious mistake in these cases, as in tarsectomy, to take away too small a wedge. Free removal of bone can do no harm. I regret that my retirement in 1896 from the post of Surgeon to the Hospital for Children and Women in this city has prevented my carrying out my observations on this most interesting subject, with all the advantages offered by the advance of skiagraphy.—I am, etc.,

JOHN EWENS,
Consulting Surgeon to the Bristol Royal Hospital for Children and Women.

Bristol, June 28th.

SIR LAUDER BRUNTON'S MODEL SANATORIUM.

SIR,—Sir Lauder Brunton apparently retains the courage of what I venture to regard as his erroneous opinions, still unshaken by the apt criticisms of "Observer" and "Vindex." He now challenges correspondents to show him any other existing sanatorium that is better than Wehrawald. I have no hesitation in saying that I believe I can show him at least three such, if not more, already in existence in this country.

Since Sir Lauder has no hesitation in urging by name the claims to excellence of a foreign institution to which fees are paid, I presume there can be no objection to mentioning by name three similar institutions which are honestly endeavouring to supply the requirements of our consumptive fellow-countrymen in this country. I believe Nordrach-on-Dee, the Mundesley Sanatorium, and the London Open-Air Sanatorium to be superior in all essential qualities for the open-air treatment of phthisis to those offered by Wehrawald. Wehrawald would doubtless have greater claims as a hotel than any of the three sanatoria I have mentioned, but in its appointments as a sanatorium I submit it has distinctly less claims than any one of them. As a matter of fact, I could mention more than three sanatoria in this country which are superior to Wehrawald, but the three cited will serve as a sufficient response to Sir Lauder's challenge.

The defects of the "ideal" Wehrawald have been freely pointed out by "Observer" and "Vindex," but there are one or two points in Sir Lauder's letter to which I should like especially to refer. It is obvious from his description that in Wehrawald the first great essential of a sanatorium—abundant and pure air—has been sacrificed to the "palatial hotel" side of the institution. Thus the blocking of the ends of the corridor by rooms and the interference with ventilation so entailed is a violation of the principles recognized in the best sanatoria in existence in England.

Further, the dining room of this ideal sanatorium appears to me to be open to the gravest objection. On one side of it, says Sir Lauder, "are large windows looking into the forest, and the opposite wall is almost completely covered with mirrors, so that wherever a patient sits he almost seems to himself to be dining in the open air." In this matter, I feel inclined to exclaim with Hamlet "I know not seems." Surely it is not the atmosphere a patient seems to be in, but that which he is in, which is of vital importance, and he certainly cannot be said to be in the open air when he is seated in a room of which, so far as one can gather from Sir Lauder's description, only one side has windows, the air through which is drawn in to supply some 50 to 100 patients, as best it may be, by an electric fan on the roof. This room might pass as a dining room in a stuffy hotel, but it is entirely unsuited for the requirements—mirrors notwithstanding—of a dining room in a modern open-air sanatorium. Moreover, a patient does not merely dine in this room, he takes breakfast and luncheon there; in fact, he probably spends nearly three hours there daily. In the dining rooms of two out of the three sanatoria I have mentioned, and in many other sanatoria, large windows are placed on three sides of the room and during a greater portion of the year are kept wide open, the patients being warmly clad and taught to be indifferent to draughts. Under these conditions, the patients not merely seem to be, but are, to their undoubted advantage, practically sitting in the open air.