Mayo Robson, and to my own. Thus in 547 operations recorded by Dr. W. J. Mayo malignant disease was found 25 times, a percentage of rather less than five. In this series are ten cases of "exploration" only, the condition found being inoperable. In Kehr's statistics the malignant cases and the acute infective cases are grouped together; of these, there were 129 cases in a total of 1000. In 500 cases operated upon by Mayo Robson there were 49 cases of malignant disease. In my own experience malignant disease has been present in almost exactly 5 per cent. of the cases submitted to operation. And these statistics include not cases of malignant disease of the gall-bladder only but cases of malignant disease in the liver or the head of the pancreas or any part of the bile passages.

Though I cannot agree that Dr. Slade's experience represents, even approximately, the average, I cannot but feel that his investigation is one of great worth and that his support of the view I have long held of the necessity for the more frequent performance of cholecystectomy is most valuable. I think his figures are exceptional but I am sure his conclusions are sound.

I am, Sirs, yours faithfully,

Park-square, Leeds, April 27th, 1905.

B. G. A. MOYNIHAN.

EPIDEMIC CEREBRO-SPINAL MENINGITIS.

To the Editors of THE LANCET.

SIRS,—In view of the epidemics of cerebro-spinal meningitis prevailing at present in New York and elsewhere it may be of interest to report three cases which occurred 12 months ago in my practice in the country near Banbridge, co. Down, and which I regarded at the time as cerebro-spinal meningitis. All three patients belonged to the same family and lived in the same house.

1. The patient was a girl, aged nine years. The mother told me that she had been ill for five days with headache and pains in the back, but she had not been in bed before I saw her. She cried frequently, was dull and listless and slow to answer questions. She had marked photophobia. The temperature was 102° F. and the pulse was 100. Examination of the chest and abdomen revealed nothing abnormal except a slight petechial rash over the skin; the spots were small and distinctly hæmorrhagic and quite unlike any of the ordinary exanthemata. The tongue was slightly coated; the throat was inflamed; there was no diarrhoea. She was placed in bed in a darkened room with ice applied to her head. Four hours later I received a telegram saying that she was dying, and so it proved. She had had a series of very severe convulsions, the temperature ran up to 106·4°, and, in fact, she died half an hour after I arrived. I was painfully surprised at this abrupt termination but I did not suspect then that the case was anything but meningitis of one of the usual types.

2. Nine days later in the same house I saw a boy, aged 14 years, a brother of the first patient. He had been ill for two days and his symptoms were almost identical with those of his sister when I first saw her. Continuous pain, mental torpor, photophobia, and petechial rash were all present. He also had an inflamed throat and the temperature was 102° F. The following day he was worse; there was well-marked retraction of the neck and spine and Kernig's sign was present; later there were spasms of the muscles but no convulsions, also hyperæsthesia over the spine, with increasing pain and difficulty in extending the legs. It seemed a typical case of cerebro-spinal meningitis and to confirm this view I proceeded to perform lumbar puncture to have the cerebro-spinal fluid examined for the diplococcus. In this, however, I unfortunately failed, for on inserting the needle, owing to the violent struggles of the boy and the sympathy of his father, I was compelled to desist, to my lasting regret. This was on the fifth day of his illness; about the seventh the symptoms began gradually to subside and he was convalescent in a fortnight. He was placed on no special treatment beyond rest and isolation in a darkened room, ice-cap, bromides, and antipyrin. The rash gradually disappeared during recovery after the other symptoms subsided.

3. During the convalescence of the last patient his sister, a girl aged 18 years, took ill in exactly the same way. She developed all the symptoms enumerated above, including the petechial rash, but they were less severe and convalescence set in about the tenth day. Both these last two cases were quite well within three weeks and no other similar cases occurred in the house or neighbourhood.

Without having bacteriological proof I would like to know whether the above history and symptoms should justify the diagnosis of cerebro-spinal meningitis in the absence of an epidemic and, if so, in what way might such an occurrence be explained. There was, of course, the epidemic in Dublin five years ago which I was fortunate enough to witness but since then I had neither seen nor heard of any cases in Ireland until these three occurred in such a curious, and to me inexplicable, manner.—I am, Sirs, yours faithfully,

J. HOPE REFORM, M.D. R.U.I.

Crossmyloof, Glasgow, April 20th, 1905.

To the Editors of THE LANCET.

SIRS,—The leading article on Epidemic Cerebro-Spinal Meningitis in THE LANCET of April 15th has drawn the attention of the medical profession in England to this disease; at the same time the English-speaking community on the continent has read of it *ad nauseam* in the columns of the *New York Herald*. It would appear that all which has been discovered in connexion with this disease should be matter of common knowledge, at any rate to our profession. Yet, as your article says, "our knowledge of the conditions leading to the epidemic spread of cerebro-spinal fever is of the scantiest." Some four years ago when in Egypt I read an interesting paper by the late Major Hill-Smith, R.A.M.C., of the Egyptian Army Medical Corps. It was published, I believe, in the annual report of the principal medical officer of the Egyptian army in 1899 or 1900. As I have never seen this paper referred to I presume that it did not attract any attention, yet at a time when once more so much is written of epidemic cerebro-spinal meningitis I may be excused for quoting from memory from a paper which contained an observation of possible importance. When after the battle of Omdurman the Egyptian army took possession of the town an epidemic disease soon arose among the troops quartered in the neighbourhood of the Mosque square. This disease was proved to be epidemic cerebro-spinal meningitis clinically and pathologically and, as far as the conditions obtaining at Omdurman allowed, bacteriologically. The disease first broke out among the companies occupying certain buildings which had been used previously by the Khalifa as stables for the horses of his cavalry and quarters for the men in charge of the horses. The case incidence was much the heaviest in this spot and spread in diminishing intensity from it, indicating, as Major Hill-Smith's map clearly showed, this as the original focus of the disease. Following up this clue it was found on inquiry that a fatal form of horse sickness had been endemic in these buildings and that two years previously it had been so severe and so fatal that the stables had been abandoned and not used until their occupation by the troops of the Egyptian army.

These facts seem to warrant at least the suggestion that the horse sickness and the outbreak of cerebro-spinal meningitis had some close connexion, a suggestion to which subsequent observations of Major Hill-Smith further up the country lent additional support, if I remember his paper correctly, but at this distance of time I cannot be sure of these last observations. With regard to the facts about the epidemic at Omdurman my memory is, I think, accurate, as I spent some six months in Omdurman shortly after reading this paper and the impression of it was increased by the local associations. Where, as the writer of your article admits, our knowledge is so scanty any suggestion based on accurate observation is worth attention and so I venture to recall to your notice the late Major Hill-Smith's paper.

I am, Sirs, yours faithfully,

Florence, Italy, April 19th, 1905.

EDWARD A. GATES.

PEOPLE'S SANATORIUMS AND PRIVATE SANATORIUMS.

To the Editors of THE LANCET.

SIRS,—I have read with interest the first part of your Commissioner's report written from "Nordrach." There cannot be too much enlightenment on the methods and practice of the different sanatoriums, even of one so familiar to most of us interested in the treatment of tuberculosis as Nordrach Colonie.

May I, with your permission, make one or two remarks? I do not hold a brief for Jones's Homes at Sandgate, nor do I approve of its arrangements if they are carried on in the style described by *Truth* from time to time. It seems to me, however, scarcely consistent with fairness to take the private

sanatorium of Dr. Walther at Nordrach Colonie as being the one referred to in the circular as "the open-air cure practised at Nordrach, Leysin, &c.," as evidently (taking into consideration the cost of treatment) the *people's sanatoriums* are the places referred to. One of these existed at Nordrach (village) where the patients (sent by the State insurance societies) occupied the ordinary village houses and when passing through a few years since I saw dozens of these patients walking about through the streets, in and out of the shops, just as they are described as doing at Sandgate. At another village in the Black Forest, Schömburg, where Herr Römpler's and another private sanatorium are situated, there was also a Volk's Sanatorium on the boarding-out system. One or two curiously decorated structures were pointed out to me as galleries or verandahs which looked out on a narrow and shut-in street.

If your Commissioner would describe some of these Volk's Sanatoriums and give details of how the patients spend their day, the quality of their dietary, and the character of the rooms they occupy many of your readers would be extremely interested. My own experience when in Germany was that the often-described sanatoriums of Falkenstein, Hohenhonneff, and Nordrach Colonie are comparatively easy to see but that the less known institutions were not always easy to obtain information about. As an instance, I was unblushingly informed at one sanatorium that another (not 20 miles distant) did not exist. Dr. F. Rufenacht Walters gives a list of people's sanatoriums in his "Sanatoria for Consumptives," but several names might be added now to that in the 1901 edition.

Your Commissioner impresses one as taking rather a different view of the infectivity of phthisis from that expressed in your excellent leading article on Public Health in the Transvaal, where you emphasise the fact that in pulmonary phthisis infection is only possible when certain simple precautions are not observed.

It has been repeatedly pointed out that the presence of sanatoriums in the villages had a beneficial educational effect—e.g., Falkenstein and Göbersdorf. About eight years ago in this city (Belfast) the fear of the infectivity of phthisis was so rampant that great difficulty was experienced in obtaining a new site for the Consumption Hospital. Now this dread has so far subsided that applications are constantly being made for permission to build villa residences inside the hospital grounds.

I am, Sirs, yours faithfully,

F. HOWARD SINCLAIR,

Visiting Physician, Forster Green Hospital, Belfast.

Belfast, April 30th, 1905.

SHOCK AND COLLAPSE.

To the Editors of THE LANCET.

SIRS,—I quite agree with Mr. P. Lockhart Mummery that arguments should be based on experimental evidence, but these arguments must satisfy the ordinary laws of reasoning. I based my arguments on the elementary fact which has been overlooked by Mr. Mummery—namely, that fluid transmits pressure equally in all directions. Surely this requires no further experimental proof. Mr. Mummery apparently thinks that increased atmospheric pressure tends to drive the blood from the peripheral to the deeper vessels but that this does not occur in health because the vaso-motor centre compensates for it by altering the calibre of the vessels. (Has Mr. Mummery any experimental proof of these views?) Are we to understand that the vaso-motor centre can keep open vessels which are subjected to an increased pressure of, say, two atmospheres? If so, by what mechanism does it do so? I pointed out in my former letter that increased atmospheric pressure could not affect the distribution of blood in the vessels because it was transmitted to all parts of the circulation equally. If it cannot alter the distribution, how can it alter the blood pressure? If the absolute pressure of the blood be x and the atmospheric pressure be y , the blood pressure as determined by instruments is $x-y$. If we increase y we increase x by the same amount according to the ordinary laws of hydrostatics and the blood pressure remains $x-y$. As to Crile's experiments I have not been able to refer to his book and must therefore rely on the short abstract given in the second lecture on p. 782. These experiments consisted in placing dogs in a condition of shock in tubes and then raising the air pressure in the tubes. It is not clear from the description whether the animal was completely inserted in the tube or whether it was only partially so inserted, but from the

account given I concluded that part of the animal was outside the tube, because of the fact that the blood pressure was apparently recorded and it is not easy to see how this could be done if the whole animal was in the tube. I am confirmed in this view by the statement that, arguing from these experiments, "Crile attempted to treat human beings in the same way." A description follows of the pneumatic suit in which the head and thorax are left outside. These experiments, then, refer to *localised* pressure only and I do not see how they support the hypothesis that *general* increase of atmospheric pressure will cause a rise of blood pressure.

Mr. Mummery says that in health increased abdominal pressure does not produce any appreciable alteration in the blood pressure because the vaso-motor centre is able to compensate for it. No doubt this is true within limits but is there no increase of blood pressure during forcible abdominal compression such as occurs in straining or during a vigorous muscular effort? It is obvious that when the abdominal vessels are dilated and the blood pressure is low the same amount of pressure will produce a larger effect than under normal conditions.

I am, Sirs, yours faithfully,

CHARLES POWELL WHITE.

St. Thomas's Hospital, S.E., May 2nd, 1905.

"THE KING'S CORONER."

To the Editors of THE LANCET.

SIRS,—Your reviewer of the above book says:—

(1) The mere citation of the names of cases might well have been supplemented by more or less copious extracts from them upon any points likely to occur in practice. There would seem to us to be room, therefore, for a carefully compiled work of this class by a sound lawyer, with (2) perhaps chapters not only on such subjects as the law of evidence but a few practical hints on the actual eliciting of it from witnesses and upon such cross-examination as the coroner is justified in administering in order to sift their stories where he has no legal assistance from interested parties.

Concerning (1), I may say that I have been engaged for a considerable time upon the work suggested, so that "Mr. Henslowe Wellington will amplify his work in this direction" as proposed by your reviewer. Concerning (2), "the law of evidence" and "the actual eliciting of it from witnesses and such cross-examination as the coroner is justified in administering" can only be acquired by attending the Central Criminal and other High Courts and carefully studying the judge, counsel, and witnesses, as is the daily routine of,

Yours faithfully,

R. HENSLOWE WELLINGTON,

Barrister-at-Law and Deputy Coroner.

Temple, April 29th, 1905.

** We understand Mr. Wellington to mean that he had intended to do what our reviewer recommended him to do—an example of what the American psychologist called "the simultaneous consultation of higher intelligences."—ED. L.

ASSOCIATION OF MEDICAL DIPLOMATES OF SCOTLAND.

To the Editors of THE LANCET.

SIRS,—It is the intention of the above association to present a petition to the Royal College of Surgeons of Edinburgh at the forthcoming fourth centenary praying that the title of "Licentiate" may be altered to "Member." Let me urge upon those holding diplomas of the Scottish corporations to join the association forthwith in order that we may voice the wishes of all the Licentiates. It is very important that this object should be pushed forward as rapidly as possible. Our honorary secretary, Dr. David Walsh, 18A, Hanover-street, W., will be glad to receive the names of those interested in this urgent and far-reaching movement and wishing to become members.

I am, Sirs, yours faithfully,

CLAUDE ST. AUBYN-FARRER, President.

London, W., May 1st, 1905.

THE PETITION OF THE LICENTIATES OF THE SOCIETY OF APOTHECARIES TO THE GENERAL MEDICAL COUNCIL.

To the Editors of THE LANCET.

SIRS,—Will you allow me to state, in reply to numerous inquiries, that the number of practitioners *really* affected by the non-descriptive title of L.S.A. appears to be greater than is generally supposed? An examination of the Medical