

3 to scarlet fever, 2 to whooping-cough, 1 to diphtheria, 1 to diarrhoea, and not one to small-pox. Thus the deaths from these principal zymotic diseases, which had been 11 and 13 in the previous two weeks, further rose last week to 19; they were equal to an annual rate of 2·8 per 1000, the rate from the same disease last week being 3·1 in London and 0·8 in Edinburgh. The fatal cases of measles and scarlet fever in Dublin exceeded the numbers in recent weeks, while the deaths from the other zymotic diseases scarcely varied from the numbers in the previous week. Five inquest cases and 4 deaths from violence were registered; and 54, or nearly a third, of the deaths occurred in public institutions. The causes of 24, or more than 13 per cent., of the deaths in the city were not certified.

Correspondence.

"Audi alteram partem."

RAYNAUD'S DISEASE.

To the Editors of THE LANCET.

SIRS,—At the meeting of the Hunterian Society, on the 28th ult., Mr. Hutchinson read a paper on Affections allied to Reynaud's Disease, which it is to be hoped he will publish. The literature of the subject has lately been enriched by the appearance, in one of the last volumes of the New Sydenham Society's issue, of Dr. Barlow's translation of Raynaud's two treatises, with important additions by the translator. In addition to the references therein to numerous cases and reports, allusion may be made to several recent records. Dr. Seymour Taylor has described in detail in THE LANCET¹ a well-marked case of Raynaud's disease in a very stout woman of fifty-six. Dr. Clifford Beale² has noted one case in an intemperate man, and a second in connexion with ague. Other cases have been brought forward by Dr. Ernest Jacob,³ of Leeds, Dr. Suckling,⁴ of Birmingham, and Mr. J. R. Lunn,⁵ besides two very characteristic cases just published by Dr. Affleck.⁶ Dr. Smith Shand, of Aberdeen, has narrated in a contemporary,⁷ under the same designation, a case of pyrexial erythema with nephritis, in which large gangrenous patches appeared on all the limbs, but none on the fingers or feet. It may be questioned whether this interesting case, with that brought before the clinical Society last year by Mr. Bellamy,⁸ have any real connexion with Raynaud's disease; the phenomena seem more suggestive of capillary embolism than of arterial spasm. Cases occupying a doubtful or half-way position are met with, such as those recorded in the last two volumes of the Clinical Society's Transactions by Dr. Coupland and by Mr. Treves.

Mr. Hutchinson's paper, with remarks made by Dr. Barlow upon the same occasion, threw much light upon the group of disorders of the circulation in the fingers and toes. Raynaud's designation, "symmetrical gangrene of the extremities," is objected to, as limiting unduly the scope of the malady, and it is better to call the disease at present by the name of its describer. One of the most important facts (and one on which Dr. S. Taylor also has laid stress) is the originally paroxysmal nature of the symptoms—a "local vascular storm," as it has been termed,—although after repeated attacks the changes may become permanent. The development of scleroderma in some of the permanent cases is very remarkable. Dr. Barlow says that this only occurs in the pale (dead hand) form of the disease. I have, however, in a different affection, watched scleroderma developing on the face as a sequel to long-continued congestion (not pallor).

Raynaud's two stages of "local syncope" or ischæmia, and "local asphyxia" or cyanosis (not to mention a third stage, of erythema), are now said to constitute rather varieties or types of the disease than stages in its course:

some cases have pale, shrunken fingers, others purplish and swollen. The explanations offered by writers for these phenomena are various, and not altogether consistent. Raynaud, in his first paper, attributed the ischæmia to "spasm of the capillary vessels"; in his second paper this is modified, and he speaks of contraction of the smallest arteries and veins as the condition present. In the cyanotic state he considers that the venules are open, the arterioles remaining contracted, and the capillaries fill by venous reflux. Some observers, on the other hand (as Dr. Starr in "Pepper's System" and Dr. Affleck), have conjectured that venous as well as arterial spasm was present in the latter condition, and arterial spasm alone in the former. I understood Mr. Hutchinson to intimate that, granting the ischæmia to be due to arterial spasm, there was as yet no satisfactory explanation of the cyanosis. The anatomical structure of the capillaries, properly so called, surely excludes the idea of spasm in their case. Nor does spasm of the comparatively lax venous channels seem a very probable efficient cause. Now I submit that the natural result of spasm of a "terminal" artery may be inferred from the well-known consequences which follow obstruction of such an artery by other means; for instance, by an embolus. In this case we know that, the *vis a tergo* being withdrawn, the blood pressure falls to zero in the capillaries, and these latter fill by reflux from the veins (as is commonly supposed), being distended with dark blood. If the stasis be complete, blood exudes through the capillary walls, which no longer maintain their nutrition, and the part becoming infiltrated with dark decomposing blood, necrosis or gangrene results. Spasmodic contraction of small terminal arteries (and those of the fingers and toes are for all practical intents "terminal"), uncomplicated by other conditions, must have a similar effect, differing only according to the degree of the obstruction, whether partial or complete. I would therefore submit that the state of local asphyxia or cyanosis, passing on in some cases to gangrene, is adequately accounted for by spasm of the small arteries alone. It is to my mind much more difficult to explain the blanched, shrunken condition of so-called local syncope or ischæmia. We have here, I submit, something more than arterial spasm at work; the capillaries are emptied, and, as they have no strictly contractile tissue, one would look for some cause exerting pressure upon them. Can it be that a gradual chilling of the entire thickness of the member (cf. frost-bite) induces a shrinking of the skin and other tissues, such as would exercise this pressure?

The occurrence of peripheral neuritis in Raynaud's disease is now well known, having been demonstrated by Dr. Wiglesworth and by Dr. Affleck in this country, besides prior observers abroad. But recent investigation, as shown by Dr. Barlow and by Mr. Hutchinson, makes it probable that the neuritis is a result rather than a cause, occurring in certain cases when the disease has become permanent and severe, and being sometimes followed by scleroderma. This view would relieve the pathology of the disease of one possible theory, and one which is rendered improbable by the paroxysmal nature of the symptoms. The analogy between Raynaud's disease and paroxysmal hæmoglobinuria and other vaso-motor spasms is very interesting, and may be said now to rest on a firm basis of clinical records.

I am, Sirs, yours truly,
Finsbury-circus, E.C., Dec. 11th, 1888. R. HINGSTON FOX.

EXAMINATION FOR THE FELLOWSHIP OF THE ROYAL COLLEGE OF SURGEONS.

To the Editors of THE LANCET.

"Who shall examine the examiners?"—*Punch*.

SIRS,—One of your Birmingham correspondents was kind enough to allude to some remarks of mine at the medical students' dinner, but he did not convey a quite sufficiently extended nor an altogether accurate idea of my complaint. Of late it has become part of my duty to look into the matter of the College examinations and their results, and I must say that my impression of them is most unfavourable. In the last paper for the pass examination of the Fellowship there are four questions, all of which had to be answered, and of these the first and third are questions so simple that they ought only to have been recorded as questions for the Membership. The second question asks the candidate to state the life-history of the hydatid echinococcus

¹ Vol. i. 1887, p. 208.

² Brit. Med. Journ., vol. i. 1887, p. 730.

³ *Ibid.*, p. 780. ⁴ THE LANCET, vol. ii. 1887, p. 964.

⁵ Trans. Clin. Soc., vol. xx., p. 259.

⁶ Brit. Med. Journ., vol. ii. 1888, p. 1269.

⁷ *Ibid.*, vol. i., p. 343.

⁸ Trans. Clin. Soc., vol. xx., p. 195. See THE LANCET, vol. i. 1888, p. 730.

hominis, giving all the stages of its development. This is a question which, in the light of recent researches, throwing doubt altogether upon the text-book descriptions, I say, cannot at present be accurately answered. The question further goes on to ask how the candidate would diagnose this disease when present in the abdomen, and to describe the methods of surgical treatment which he would employ for it in this situation. No clue is given as to whether this is meant to include the separate diagnosis and treatment of hydatid disease of liver, kidney, spleen, broad ligament, Fallopian tube, &c., and the peritoneum, or whether it means the peritoneum only. If the latter is meant, then I say no diagnosis is at all possible. I have seen and operated upon as many cases of this kind, I am perfectly certain, as any living surgeon, and the only position within the abdomen in which I have ever seen a diagnosis even guessed at by practitioners of all shades of opinion, rank, and experience, from the President of the Royal College of Physicians downwards, has been the liver. The text-book statement that hydatid disease of the peritoneum may be diagnosed by hydatid *fremitus* is one which is totally irreconcilable with my own experience, and my views upon this question have received a remarkable confirmation by the fact that one of the members of the Court of Examiners responsible for it was called into consultation to a case of hydatid disease of the abdomen, and signally failed to make any diagnosis whatever. The fact is recorded in a recent number of your own journal, and I now ask this particular examiner specifically how he would have answered his own question. Again, if this question is to mean that surgical treatment is to be described for hydatid disease of all the organs in which it might occur within the abdomen, the candidate would have needed the whole time allowed for the whole four questions in which to give descriptions of eight or ten totally different operations. On the other hand, if it meant only hydatid disease of the peritoneum, then I confess that, with an exceptional experience, the question would have floored me, and therefore I am reasonably complaining of the gross injustice of it to less experienced candidates. The fourth question contains the extraordinary clause which asks the candidate to indicate the cause of the great mortality formerly prevailing after fracture of the base of the skull. I have discussed this point with twelve hospital surgeons of large experience, with one exception all Fellows of the College of Surgeons by examination. With one accord they deny that there is any diminution within their experience of the mortality of this injury. They say that it is as fatal as it has ever been, and no amount of research which we have been able to give to the subject has unearthed any statistical proof to the contrary. We have an uneasy feeling that this question is an antiseptic riddle, and we have discovered a passage in Erichsen (9th edition, vol. i., page 737), which seems to be the only solution to the conundrum. There it is inferred that the employment of antiseptic measures (syringing out the external ear with a solution of carbolic acid, and plugging it with iodoform) will contribute to the recovery of a fracture of the base of the skull, which is compound when the membrane is torn. But no precautions are recommended for the Eustachian tube. Some curious facts indicate that this extraordinary question was put for the purpose of getting this still more extraordinary reply. If it be so, then I say that, if a single candidate has failed by a single mark on this account, the examiners need to be hauled over the coals; for it is simply intolerable that, while we in the provinces are teaching our students to laugh at all such crotchety nonsense as this, the examiners in London should be enabled to pluck our students for want of belief. The alternative is that, if this question was not put for the purpose of getting this crotchety answer, it is altogether meaningless.

I am, Sirs, yours, &c.,

Birmingham, Dec. 17th, 1888.

LAWSON TAIT.

"ARE WE DEGENERATING PHYSICALLY?"

To the Editors of THE LANCET.

SIRS,—In a leading article of Dec. 1st, you seem to attach considerable importance to the opinions of many members of the profession on the above subject, elicited by an energetic "northern lay contemporary." The subject is no doubt of vast importance, and therefore all the more is it necessary to receive with much caution and scepticism

the opinions of medical men, no matter of what eminence, unless they are based on patient and accurate observations, and not merely on personal impression formed more or less haphazardly. Before receiving their opinions, I would like to know what methods they adopted in order to honestly come at the facts. I have before me the carefully drawn up report of the Anthropometric Committee of the British Association. The author, Charles Roberts, F.R.C.S., has devoted long and careful consideration to the subject, and his statistics have been methodically and laboriously worked out, and are therefore worthy of the highest regard. From this report I see that the physique of the inhabitants of these islands varies very considerably according to social standing and environments, the professional classes being the highest in the scale, and the artisans in the towns being the lowest, the intermediate position being occupied by the country labourers, farmers, &c. I also find that living in towns exercises a deleterious effect, especially upon the poorer populations in the crowded districts. In the case of London the ill effects have naturally been more widespread, and the physical standard of the home counties has sensibly depreciated, owing to the constant drain of all the best manhood to supply the ever-increasing demand for stout artisans and labourers for the heavy trades in the metropolis. Granted, then, that town life does exercise a prejudicial effect upon the physique, how are we warranted in indulging in optimistic views of the future of the physique of the inhabitants of these islands, when we see year by year the exodus from the country and the crowding into large towns in a continuous ever-increasing stream? Civilisation has a general tendency to improve the physique, and the question should not be, "Are we degenerating?" but "Are we improving?" The physique of the town populations probably is better than it was thirty years ago owing to improved sanitary surroundings, and still more to the immigration from the country; but I ask, are we to be content and indulge in optimistic felicitations while the social changes going on around us are gradually attracting the country populations into the vortex of the big towns, and as surely reducing the physical type to the dead level of the artisan classes?

I am, Sirs, your obedient servant,

Sheffield, Dec. 10th, 1888.

C. N. GWYNNE, B.A., M.D.

"THE ALLEGED INCREASE OF CANCER."

To the Editors of THE LANCET.

SIRS,—In the annotation in your issue of Dec. 15th upon the above subject you remark that improved diagnosis of malignant disease and greater accuracy in making returns do not suffice to explain the rise in the death-rate from cancer, but that more detailed statistical returns, especially as to the organs primarily affected, the ages and sexes of the subjects, and the districts in which the various forms of cancer most prevail, are required. Herewith I enclose a copy of a paper read by me at the Royal Medical and Chirurgical Society on April 29th, 1884, by which you will see I drew attention to this subject and produced detailed accounts. I compared the number of deaths which occurred in England and Wales in the years 1850, 1861, 1872, and 1881, by which I showed that mortality had increased from 4966 in 1850 to 13,542 in 1881, and that the death-rate in the 1,000,000 had increased from 320 in the former year to 520 in the year 1881. I further contrasted the increase in the mortality from cancer in each division and county in England and Wales, and then analysed the death-rate of each county separately, and finally drew attention to certain factors existing in the different counties to which might be attributed to a greater or less extent the increase observed. I analysed the deaths at different ages and of different sexes. As you remark, a careful study of this is very suggestive. In one of the concluding passages I said: "I do not wish to be an alarmist, but because it is incontestably proved that cancer is becoming more and more common, is it a reason that we should shirk the subject? On the contrary, I contend it is all the more reason why we should face the fact boldly, and endeavour if possible to discover the cause of the increase, and, having discovered the cause, to apply ourselves to find a remedy."

I am, Sirs, your obedient servant,

FREDK. BOWREMAN JESSETT.

Upper Wimpole-street, W., Dec. 17th, 1888.