

by Dr. Williams, is an obvious example of the latter, a recognised though rare condition, and has nothing whatever to do with heredity. We see cases of congenital syphilis more often, but no one argues for a hereditary predisposition to syphilis.

While Dr. Williams's figures and generalisations as to the family relationship of consumptives may be accepted without question, they do not seem to support his conclusions (1) that both male and female phthisics transmit a tendency to the disease, and (2) that the male transmits a greater power of resistance than the female. Indeed, the transmission of a resistance against the disease, together with a tendency to it, seems somewhat of a paradox. The broad generalisation of the greater as well as the earlier influence of maternal tuberculosis has been reached by many authorities, but they have by no means drawn the same conclusions as Dr. Williams. For instance, Dörner, in an investigation on behalf of the Baden Ministry of the Interior (quoted in Köhler's "Results of Tuberculosis Research," 1911, p. 43), remarking on the same fact, concludes that it points to infective and not hereditary influence. He further finds that of children dying from tuberculosis in the first and second years of life 33·5 per cent. had a tuberculous mother, 14·7 per cent. a tuberculous father. There is no reason whatever in falling back upon vague hereditary transmissions of tendencies or specific weaknesses for an explanation of such facts.

Latham quotes Bang of Copenhagen as demonstrating that if the calves of tuberculous cows were removed at birth from their mothers it was possible to form a herd absolutely free from tuberculosis. Similarly, orphans through tuberculosis, when removed before too cumulative infection has occurred, prove as healthy as other children. We shall, I maintain, be absolutely at fault while we attribute to hereditary influences results which are clearly explicable by the cumulative effect of a tuberculous environment. The moral of which is that if we want to stamp out consumption we must begin at the beginning and remove children from infectious surroundings to some such institutions as orphanages.

I am, Sir, yours faithfully,
Norwich, March 31st, 1913. ARTHUR H. PRIESTLEY.

THE POSITION OF THE DISPENSER UNDER THE NATIONAL INSURANCE ACT.

To the Editor of THE LANCET.

SIR,—The great transfer of dispensing to the chemist can only result in the throwing out of employment many persons who have spent their whole time as dispensers to doctors and institutions, many of whom have spent time in study, fees at colleges of pharmacy, and have secured the dispenser's diploma of the Apothecaries' Society. The most serious reflex has not yet been felt, their appointments tending to wane rather than to terminate abruptly. They represent a class of person who are in every way better fitted to do the dispensing for the insured persons rather than the assistant and junior of the trade who have done little dispensing in the past.

It is quite deplorable that the chemist is not requiring the institution or doctors dispenser to dispense under the Act. Analysis of advertisements (in a trade journal) made by myself shows more than three times as many situations open this year than last in the chemist trade. Only 4 per cent. of the advertisements referred to dispensers and 1 per cent. for the holders of the Apothecaries' Hall diploma. My position after nine years' service being seriously affected has actuated me to do all possible to seek a remedy with the help of my colleagues. Special efforts are being put forward to plead our case in the right direction, and much valuable help would be rendered by a general testimony of the medical profession. Postcards expressing sympathy in the movement from members of the medical profession would prove an asset and would be gratefully received. The names would only be used generally in the very legitimate effort to get the right to be employed to do work which has been taken from us so ruthlessly by the trade which does not wish to employ us for economic reasons since 32 per cent. of the situations are for juniors and 27 per cent. for unqualified assistants of

the trade. "The qualified chemists wanted are chiefly for supervision purposes."

Allow me to thank the profession in anticipation of their favours,
I am, Sir, yours faithfully,

GEORGE REED,
Dispenser.

The Provident Dispensary, 53, Surrey-street, Croydon,
March 31st, 1913.

WHEN TO OPERATE IN APPENDICITIS.

To the Editor of THE LANCET.

SIR,—Sir George Beatson has very kindly sent me a copy of his paper on Appendicitis, so that I might obtain the information I desired in respect to the results of his 300 cases. On examining these figures I find 126 were quiescent cases on admission, and therefore do not count in the consideration of the mortality occurring in acute cases. Of the remaining 174, 10 had general peritonitis, of which 7 died; 164 were acute, of which 69 only had abscesses, and 4 died. The total mortality in acute cases, therefore, works out at 6·2 per cent., a low figure, it is true, but not so low a one as many surgeons who operate early can show, and not so low as obtained recently at St. Mary's Hospital.

In reading these figures one is struck by the rarity of cases of general peritonitis, 10 out of 174, a proportion which does not correspond with the frequent incidence in London, 19 out of 136 (St. Mary's) (15 cases in my last 57). Again, a mortality of 70 per cent. is exceptionally high. I think we should all feel very happy if our experience showed that 95 cases out of 174 would quiet down without suppuration. Certainly in my experience the proportion of suppuration in acute cases (admitted to hospital) is very much higher. Nor does Sir George Beatson mention the occurrence of multiple abscesses. He has no trouble with leaking or ruptured abscesses, and one is therefore compelled to infer that the resistant power of the Scotch peritoneum is much greater than that of the English.

Sir George Beatson acknowledges the benefit of early operation, but decides that it is not practicable. With this opinion I entirely disagree. At the present time, with competent modern surgeons in every district, telephones to summon them, and motors to bring them to a case, no patient need be left for more than 24 hours without being operated upon, except under very unusual conditions.

I am, Sir, yours faithfully,
W. H. CLAYTON-GREENE.

Devonshire-place, W., March 30th, 1913.

JOSEPH CLOVER.

To the Editor of THE LANCET.

SIR,—In your leading article on Joseph Clover in the last issue of THE LANCET you ask if he and Lister were at the first operation under an anæsthetic in Europe. I happen to know that Lister was, and in all probability Clover was also. He was in residence at the hospital in 1846, and so would naturally see anything remarkable that was going on. It may be of interest to recall that he was what is now termed the R.M.O., in those days the "resident apothecary," and my father was one of the last to be "apprenticed" to him—i.e., what now corresponds to taking out a complete or perpetual hospital attendance ticket. This was in 1847. I quite agree with your appreciation of Clover's genius. As a young man I knew him well, and almost the last anæsthetic administered by him was to a member of my family. A quiet, kindly man with a genius for useful mechanical invention and a power of thought that was remarkable. Some years after his death a late colleague of mine thought he would try and improve on Clover's small ether inhaler. He was able to study all Clover's models, and being an excellent mechanic himself, came to the conclusion that he could not improve on the original pattern. I only know one real improvement, that of the wide-bore through-tube of my friend Sir Frederic Hewitt. What an ideal anæsthetist Clover was—calm, cool and encouraging to the patient and a real help to the surgeon. In his time there were few who followed the profession of chloroformist, and the giving of anæsthetics was often a rather haphazard performance by untrained practitioners. As an expert it was no wonder

that Clover had the large practice which he deservedly enjoyed for a good many years.

I am, Sir, yours faithfully,
London, W., March 28th, 1913. F. WILLIAM COCK.

THE GENITAL FUNCTIONS OF THE DUCTLESS GLANDS IN THE FEMALE.

To the Editor of THE LANCET.

SIR,—Owing to an unfortunate concatenation of circumstances my first Arris and Gale lecture, which appeared in your issue of March 22nd, was published without being properly revised. For one thing, the lecture was published a week sooner than I expected, and as a result the corrected proof can only have reached you while THE LANCET was passing through the press—probably earlier than usual owing to the advent of Easter.

Apart from verbal and other small corrections, I regret to say that there are errors in some of the figures in Table I. These, of course, make the averages at the foot of Table I. (which should really be Table II.) inaccurate, although they do not affect the general results in any way. Further, it is obvious that the section dealing with the "Clinical Observations of the Effects Produced on the Thyroid by Ovarian Insufficiency in Women" should follow that on "Effects of Oöphorectomy on the Thyroid." As I believe no one can be too careful in the matter of accuracy either of style or scientific detail, I have felt it necessary to call attention myself to what has happened rather than leave it to someone else to do so. If anyone should be sufficiently interested in the subject and desire to have the correct figures I shall be glad to furnish him with one of the revised reprints which I hope will be available shortly.

I am, Sir, yours faithfully,
Liverpool, March 22nd, 1913. W. BLAIR BELL.

A REVISED NOMENCLATURE IN PHTHISIS.

To the Editor of THE LANCET.

SIR,—Amongst the remarks you make about my article on sanatorium benefit in East Essex you say "the term 'early case' has gradually become almost synonymous with a 'favourable case,' and this is to be regretted since an 'early case' may be an acute case and quite unsuited for treatment in a sanatorium, a hospital being the more suitable institution." With this everyone must agree. Do you not consider that the time has come when the whole nomenclature in phthisis pulmonalis ought to be put on a definite footing, and that a fixed classification ought to be used throughout the country?

Now that the State has undertaken the crusade against consumption all the statistics will be of enormous value, and unless these are comparable in every respect this will be of little real value. If a committee was formed of such men, say, as Sir R. Douglas Powell, Sir T. Clifford Allbutt, Sir William Osler, and Dr. Arthur Newsholme, of the Local Government Board, a scheme acceptable to the whole profession could be formulated. The nomenclature could be adopted in the medical schools, &c., and would prove, I think, of the greatest use to those interested in sanatorium benefit. I am, Sir, yours faithfully,

J. D. MACFIE,
Tuberculosis Officer to the Essex County Council.
Chelmsford, March 31st, 1913.

AUTO-VACCINATION IN TUBERCULOSIS BY THE X RAYS.

To the Editor of THE LANCET.

SIR,—While agreeing that for a man to have a grievance is abominable, it is with no view to self-advertisement that I desire to draw your attention to the plagiarisms of the original article on the above subject contributed by me to THE LANCET in January, 1907, garbled versions of which appear from time to time as abstracts from foreign journals in the medical press of this country, where the credit for this particular research is ascribed to foreign authors, who do not hesitate to use expressions that I have found it necessary to

coin, without so much as citing the source of their information.¹

I trust, therefore, that you will pardon my writing at length on this matter. Conspicuous amongst these is a Swiss practitioner in the *Correspondenz-Blatt* of Basel, who contributes an original article to that journal "On the Conservative Treatment of Tuberculous Glands."² In that article he uses the expression "auto-vaccination," which has been interpolated by the translator into "some kind of auto-inoculation."

My object in using that expression was to connote a physiological as distinguished from a pathological process, such as auto-inoculation so often is, and consequently fraught with danger. While the author cites one British author (Gulland) in the usual manner, he curiously cites Dr. Peter Paterson (whom I cited in my article) without giving the clue to Paterson's article by any footnote, spelling his name in the German or Norwegian fashion (Patersen) he omits reference to my article, the one with which he seems to have been mainly concerned.

It is not for me to exaggerate the importance of my research, which has not yet been repeated in this country, but I can assert from my experience that the operation for the removal of tonsils alone would thereby be reduced 80 per cent., with the greatest advantage to the sufferers, from the point of view of restoring immunity, when applied before the suppurative stage has set in. One dare not publish them on account of the *odium theologium* that unfortunately exists. But a careful study of the results that have been obtained in Berlin, Paris, and Basel, to mention a few, goes strongly to support my contention. Quite apart from the mere results, these researches throw light upon the lymphoid and lymphatic mechanisms of the body in their bearing upon pathological processes of an undreamt of kind, such as will before long establish "the logan stone" of our science upon surer foundations.

I am, Sir, yours faithfully,
Harley-street, W., March 31st, 1913. H. D. McCULLOCH.

A QUESTION IN LUNACY CERTIFICATION.

To the Editor of THE LANCET.

SIR,—Dr. Robert Jones, in remarking on the slackness of justices in the matter of lunacy certification in your last issue, might very well extend his valuable advice to the medical profession in general. It is astounding how ignorant are medical men on the mere bare facts of the Lunacy Acts. It is excusable, too, since it is only in private and public mental hospitals where the practice of the laws of lunacy can be thoroughly studied. And the pitfalls are many, even to those who are in the actual practice of lunacy, so it is not surprising to learn of the errors of the unprofessional and unpaid layman, the justice. In my opinion every medical man in general practice should hold a certificate in psychology or give proof of a sound training in this branch of his profession. The provision in the Lunacy Act, 1891, on the matter of the text of this letter, was undoubtedly a convenience to meet such cases where other justices than the judicial authority had signed the reception order. And the wording of the Lunacy Act, 1891, Section 24 (3), is simple enough, for it says:—

A reception order made after the passing of this Act shall not be invalid on the ground only that the J.P. who signed the order shall appear to have not been duly appointed under Section 10 of the principal Act, if the order is within 14 days after its date approved and signed by a judicial authority.

But surely it is the duty of the certifying physician to see that this is in order, for is there not a circular letter by the Commissioners in Lunacy, embodied in Archbold's "Lunacy," pointing out that all persons concerned with lunatics should make themselves acquainted fully and textually with the provisions of the Lunacy Acts? And as medical men are paid for signing lunacy certificates, and justices receive no remuneration, excepting the stipendiary, should not the former know their business, whatever may be the failings of the latter?

I am, Sir, yours faithfully,
JOHN FREDK. BRISCOE,
Westbrooke House, Alton, March 31st, 1913.

¹ Brit. Med. Jour., Feb. 10th, 1912; Ibid., March 22nd, 1913; Ibid., March 29th, 1913.

² xlii. Jahrg. 1912, No. 20, Basel, 20 Juli, pp. 729-741.