

was undoubtedly based on his observations of cases of angina and aneurysm, and I suggest that this train of thought led him astray.

The "tenderness in the scalp related with visceral stimuli in the whole vagus field" is a phenomenon with which I have only a textbook acquaintance. My lack of any clinical experience of it leads me to believe that it is a rare phenomenon, and therefore not one on which an important physiological argument can lightly be based. I tried in my paper to argue from phenomena such as are met with in the routine clinical work of any general surgeon.

In reference to the deep tenderness over an area of pleuritic inflammation you find a difficulty in the fact that with the outpouring of fluid this tenderness disappears, though it may be only up to the level of that fluid. I am bound to accept the pleural cavity as germane to the argument, since it is developed from the peritoneal cavity, but the fact that you mention seems to me not a difficulty but a confirmation of my thesis. As the fluid collects it separates the visceral from the parietal pleura, and so stops the friction of the parietal pleura, which is the source, by pleuro-cutaneous radiation, of the deep tenderness.

A theory such as Mackenzie's theory of referred pain when once it has become orthodox dies hard, and it is right that we should be very critical of new and possibly subversive doctrines. But if the unity of the gastro-intestinal tract be granted I feel confident that an unbiased study of the clinical and operative phenomena available will lead to the general conviction that we have been trusting to a theory of reflexes that does darken counsel, and that the true mechanism of abdominal pain is simpler than Mackenzie believed.—I am, etc.,

Manchester, June 8th.

JOHN MORLEY.

PATHOGENESIS OF ACUTE PRIMARY GLAUCOMA.

SIR,—There are one or two points in Mr. Duke-Elder's letter (June 9th, p. 1000) which call for reply. He has instanced the wide differences of measurement, but those methods used for measuring capillary pressure which give high readings are open to the criticism that they stop the flow, produce banking up, and so measure the pressure in the arteries—kinetic energy of flow being converted into static energy. The least equivocal methods give the low readings—for example, that of finding the countervailing pressure which stops bleeding from a small cut in the finger, such a cut undoubtedly severing arterioles as well as capillaries. Also the method of introducing a quartz micro-pipette into a branch of a capillary network, and finding the pressure which just prevents entry of blood corpuscles into the pipette. Both these methods have been applied to human skin. Landis introduced such a pipette into what he calls arterial capillaries found in the mesentery of the frog. These are straight vessels, usually unbranched, and like arterioles. He has measured the systolic pressure by forcing a dye into the vessel. Now the pipette both occupies room in the vessel and obstructs flow, and banking up is produced by injection. The systolic pressure so measured is much too high, but it comes into his calculation of the average normal lateral pressure of the capillaries. He also introduced the pipette into a branch of a net of what he calls venous capillaries. This would give a much more accurate reading, but not differentiating one set of readings from the other he gives an average of them all, and this must therefore be too high. His method of measuring the passage in or out of fluid from a capillary is no less open to criticism, as shown in the papers by J. McQueen and myself, which are appearing in the next number of the *British Journal of Experimental Pathology*.

One important factor generally left out of reckoning is the counterbalancing pressure of the tissues. The capillary pressure can rise to considerable heights under certain conditions, such as obstruction of the veins, but as the tissues are confined by membranes the tissue pressure rises concomitantly. In the eye the pressure of the humours counterbalances that of the minute vessels, and a very small excess suffices to maintain velocity of flow. If the aqueous

fluid be let out and the abdomen be compressed the iris swells up, and the minute blood vessels, no longer supported by the counterbalancing pressure, may burst. In the kidney the fluid in the capsules counterbalances the pressure in the glomerular capillaries.

The method which I use for measuring capillary pressure is the same as that used in measuring blood pressure in the arm by means of the pneumatic cuff and manometer, the only difference being that the indices of capillary pressure are observed under the microscope. There is abundant proof that this method is fairly accurate, and I apply it to higher animals, mice and bats, no less than to frogs and toads. The capillary pressure in the minute capillaries of the fat in the mesentery of a mouse is lower than that in the big capillaries of a frog.—I am, etc.,

London, N.W.3, June 18th.

LEONARD HILL.

SIR.—Mr. W. S. Duke-Elder, in the opening paragraph of his letter (June 9th, p. 1000), states:

"Dr. Ramsay suggested that the evidence was in favour of the formation of this fluid by dialysis in the same manner as the other tissue fluids, and to this Dr. McQueen objects on two grounds: first, that there is not a sufficient pressure in the capillaries of the eye to allow such dialysation, and secondly, that the aqueous humour is formed in quantity too great to be accounted for in this way."

Mr. Duke-Elder's paraphrase of my letter is quite an erroneous one. I did not confuse filtration with dialysation.

By filtration is meant the passage of fluid through a membrane as a result of a difference of hydrostatic pressure on the two sides. (Waymouth Reid in Schäfer's *Textbook of Physiology*, vol. i, p. 280.) Dialysation or dialysis does not imply a difference of hydrostatic pressure on two sides of a membrane. Krogh (1922) is also misquoted.

Landis, in 1927 paper (*American Journal of Physiology*, vol. lxxxii, p. 217 et seq.), which is the important paper dealing with passage of tissue fluids, never found how much the pressure in the "arterial capillaries" was above the tissue pressure because he never measured the tissue pressure. The osmotic pressure of the tissue fluids in the frog would not be overcome by 145 mm. of H₂O as stated by Mr. Duke-Elder. As regards the supposed pressure gradient of 22 to 28 mm. of Hg in the capillaries of the eyeball, I did not argue from analogy solely, as anyone can see who reads more of the paragraph than Mr. Duke-Elder quotes.—I am, etc.,

Halesowen, June 16th.

JAMES M. MCQUEEN.

THE FUTURE OF OBSTETRICS.

SIR,—In India little girls of 6 years old are married to men of 40 years of age, and many little children join the cruel and awful ranks of widowhood before they have menstruated. This is done, and approved of, in the sacred name of religion. In England men and women are allowed to qualify and try to perform the most difficult operations in obstetrics in the most unfavourable surroundings, after having completed at most two months of practical midwifery and after having delivered perhaps less than ten cases. People can get used to and approve anything.

The maternal death rate in Siam is ten times as great as that of England (at least 40 per 1,000). The point I want to make is this: although every law of hygiene is broken, although clothes and rags and filthy nails are scraped around the vagina and sometimes even into the uterus, yet 950 out of 1,000 women survive. The converse is equally true. Because only 4 women out of every 1,000 die in England it does not mean that the other 996 had a safe confinement. I am firmly convinced that no woman who has to submit to any form of interference has as safe a confinement as possible. Every vaginal examination is potentially dangerous, and they are absolutely unnecessary in over 90 per cent. of all cases. It is precisely those cases of labour which are normal, or vary but little from the normal, which require so many months of the student's time to master, and which are so liable to lead him into excessive interference in his practice.

The whole crux of the matter is that the student is practically ignorant so far as midwifery is concerned when he

qualifies. It is high time that every unit of the medical profession faced the facts and insisted on a longer and more practical course in obstetrics. It will then be realized that until about three times as many beds are available for the instruction of medical students the desired improvement is impossible. The necessary hospitals might be built by private companies, for I am sure they could be run at a profit, or they might be obtained by the conversion of existing infirmaries. The important point is that the medical profession should be unanimous in demanding that they should be obtained and that the ridiculously inadequate training in obstetrics should be recognized and altered.

Professor McIlroy in your issue of March 17th (p. 467) writes that "British midwifery is the admiration of the world." I am sure she has adequate evidence for that statement. Whatever it is the world admires it certainly is not the institutions, for we have none that can compare with the admirable clinics of such places as Leipzig, Berlin, or Vienna, or even, as far as equipment goes, with the Government Hospital, Madras. I do not think that either we ourselves or the world can admire our results or the amount of research work carried out in Great Britain on obstetric subjects. Let us rather determine that British obstetrics shall again be the admiration of the world, as it once was.

We spend enormous sums of money on cancer research which may or may not be the means of saving lives. Shall we refuse to spend money in preventing the wastage of the mothers of England just because we know the causes of their deaths and could prevent them? The plain unvarnished truth is that there is not the money, there are not the institutions, and there are therefore not the facilities for adequate instruction or effective research.

It is because Dr. Cressy, in his courteous reply (March 10th, p. 412) to my letter published in your issue of February 18th (p. 284), failed to answer my two questions that I have ventured to refer once again to what, I am sure, is the root of our obstetric evils.—I am, etc.,

G. W. THEOBALD,

Professor of Obstetrics and Gynaecology,
Bangkok.

Siam, May 22nd.

SIR,—In reference to the training of midwives and the education of medical students in midwifery I wish to suggest that the bigger midwifery schools should confine themselves to the training of medical students and graduates, and that the smaller midwifery training schools should receive the pupil midwives. This would leave a much wider scope for the better education of medical students and doctors. I do not suppose there is one of us who, when called out as a young graduate by a midwife for an emergency, did not regret at some time or other having had so little experience in his training school, both in ante-natal work and in deliveries. An abnormal case can be made normal during the ante-natal period: a normal case can be made abnormal during labour. The adoption of my suggestion would mean that the larger schools, finding themselves short of nursing staff, would have to employ a greater number of staff nurses. A staff nurse's post is equivalent to a post-certificate course to her. Thus, in effect, the larger schools would train medical students for their qualifying examination, and would provide doctors with post-graduate experience, and staff nurses with post-certificate experience. The smaller schools would train pupil midwives for their certificate examination.—I am, etc.,

Middlesbrough, June 11th.

G. H. GIFFEN DUNDAS.

PUERPERAL SEPSIS.

SIR,—It is the universal experience that many a Sairey Gamp, with no knowledge of nail-brush or lysol, working under the least favourable surroundings, has yet been able to show a spotless record. I myself can think of more than one such whose cases "never go wrong." We know, too, that it is common for difficult cases, attended by trauma, ending even in manual removal of the placenta, conducted in the most unsuitable surroundings, still to be followed by an uneventful convalescence.

On the other hand Mr. Burt-White, in his most interesting and, to my mind, important paper (*British*

Medical Journal, June 9th, p. 974), describes normal cases, under skilled modern supervision, that have had morbid puerperia, whilst Dr. James Young (p. 967) is so impressed by the danger of grouping parturient women within four walls that he urges that no maternity home should be without its isolation block.

These facts taken together seem to show, as Dr. Young himself suggests, that her own home is the safest place in which a woman may be confined, unless, perhaps, some exceptional difficulty is anticipated. If fever should occur the isolation difficulty does not arise, while if surgical intervention should become necessary she can be moved to a hospital or nursing home as easily as from a maternity home.

In a speech the other day the Minister of Health proposed, in addition to an increased number of maternity homes, an "improved" training of midwives. If by this he meant that he hoped that in future all pupil midwives would be taught to make only rectal (instead of vaginal) examinations in normal cases, I think few will disagree. But if it is proposed to add to their intellectual burden, then the results will certainly be disappointing in more ways than one.

Another not unimportant point arising out of Mr. Burt-White's paper is relative to the position of the practising midwife. At present there is a tendency at least to hint that a morbid puerperium is the result of some failure in her aseptic technique: in some instances she is made to feel that she is sitting on the edge of a precipice over which the first "temperature" may push her. It is to be hoped that one result of Mr. Burt-White's observations will be general recognition that however skilful, however careful, and however conscientious a midwife may be, untoward complications may appear, just as they do in surgical and medical practice, and that, when they do, she needs sympathy and help rather than blame. Moreover, if an investigation is conducted in the spirit of helpfulness, she will be much more ready to assist than if she feels that any evidence she may give is likely to be used to her own disadvantage.—I am, etc.,

Sydenham, S.E., June 13th.

W. M. PENNY.

THE TREATMENT OF MALIGNANT DISEASE BY COLLOIDAL LEAD.

SIR,—I cannot help thinking that Dr. Wyard has been ill advised to invite me to demonstrate further and completely the futility of his work and report, which reflects not only on himself, but inevitably also on the institution with which he is connected, for he states that the so-called investigation was a concerted effort in the Cancer Hospital. However, as he is unwilling to accept my "*ex cathedra* statement," which it was to his advantage to do, I shall take the trouble, which I would rather have been spared, of exposing the full extent of his errors in respect of the method of preparation of his so-called "colloidal lead," of which he says "I . . . affirm that they are the same as those used in Liverpool." From Dr. Wyard's account of his various methods the following points emerge.

1. Dr. Wyard's preparation was made by sparking . . . in a medium of distilled water. *Afterwards he added the gelatin and electrolytes.* We spark in a medium containing the gelatin and electrolytes. So far as one can say, without wasting the time necessary to investigate something which would be valueless to our work, a heavy, direct current such as that which appears to have been used would produce in distilled water lead hydroxide and large particles of metallic lead; there would certainly be very little colloidal suspension of lead. It is of primary importance to spark in the medium containing gelatin. In this respect, therefore, the method employed at the London Cancer Hospital is diametrically opposed to our own.

2. Dr. Wyard used isoelectric gelatin in some of his preparations. This is acid, and would, therefore, lead to the formation of lead ion (Pb ++), the very thing the method is designed to avoid.

3. The preparation used by Dr. Wyard at the London Cancer Hospital does not appear to have been centrifuged; consequently it would contain large particles of lead—particles large enough to block capillaries—especially when made as described.

4. There is no mention by Dr. Wyard of any examination to discover whether lead ion were present in the material used. Undoubtedly the preparation would contain large quantities