

SIR,—There will probably always exist, and always have to be combated, an unfortunate prejudice against the secretory theory (as opposed to theories of filtration, dialysis, etc.). This prejudice arises on quite extraneous—I mean unscientific—grounds. It is the idea that to deny that the behaviour of the living cell can be deduced from the boy's first book (or any existing book) of physics is equivalent to investing the cell with occult powers.

It cannot be too strongly asserted (though it is nauseating to have to assert it at all) that the secretory theory would not be a scientific theory if it sought to confer upon the cell any measure whatever of supernatural activity. It is only in the minds of those who have fainted upon the scientific way that the secretory theory has any connexion with "vital force," "biotic energy," "neovitalism," or any such refuge of the destitute.

To conclude on the evidence available that rather crude hypotheses like those of filtration, dialysis, etc., may (in view of the great complexity and delicacy of living matter) prove to be reductions to a simplicity which is absurd is perfectly scientific, and, whether the conclusion itself is right or wrong, it voices no greater heresy than does the suggestion that a man may act differently from a corpse.

The secretory theory merely emphasizes the fact that matter which has attained that peculiar degree of complexity which constitutes aliveness is apt to behave otherwise than matter which has not this complexity. No metaphysical theory whatever is involved or implied, and not the slightest departure is made from that working hypothesis of absolute materialism (or absolute objectivity) which is the inalienable basis of all scientific knowledge.—I am, etc.,

Liverpool, May 29th.

BERNARD CHAVASSE.

#### THE ORIGIN OF ISCHAEMIC CONTRACTURE.

*The Case of Tyndall v. Alcock.*

SIR,—Mr. Edward Thompson (June 2nd, p. 961) should not censure Mr. Roth (May 26th, p. 921) for his comments on Professor Hey Groves's article on this case (May 12th, p. 807). Mr. Roth at least suggests a valuable point which will help others to avoid the advent of this nightmare—ischaemic contracture—which, as those engaged in orthopaedic practice know so well, is far less rare than Mr. Thompson's fortunate experience would suggest. Mr. Thompson's note is merely a genuine expression of the sympathy which, of course, all of us feel for Dr. Alcock, but it adds nothing whatever to guide us in preventing the occurrence of this grave complication.

Mr. Roth did not stress the point which I am sure he had very much in mind—namely, that "full flexion," as advocated in the books, is a factor equal in importance to tight bandaging in the production of ischaemic contracture. If the forearm in relation to the upper arm is "5 minutes past" (to use a clock nomenclature) the evil is likely to occur. On the other hand, putting the elbow up at "7 or 10 minutes past"—that is, 40 to 60 degrees—will be safe.

Supporting Mr. Roth's point, I would mention that one of the worst cases I have seen followed bandaging of the upper forearm, for a graze, by a village nurse. Of two cases that have happened to myself, one followed "full flexion" ("5 minutes past") and the other on tight bandaging of the forearm after plating a forearm fracture (without flexion), although, fortunately, being alive to the possibility, I got in on the problem before more than a temporary damage had occurred to the flexor muscles of the index finger.

Professor Hey Groves himself, in his article, makes no very helpful deductions to guide us in the anticipation and prevention of the condition. I agree with him that the displacement of bone fragments has no importance whatever in the production of ischaemic contracture. That it is a dangerous thing to flex a swollen elbow-joint I have already admitted; but he puts forward the old fallacy that the existence of the radial pulse is the index of safety. This is a snare and a delusion, and, if I may be allowed to say so, very bad teaching. I have had the condition occur under my most intense observation while

believing that all was right as long as the radial pulse could be felt.

The threatening symptoms are pain, pain, pain! associated with lividity and swelling, and disinclination for active movements of the hand and fingers. Relieve these symptoms immediately, and if this is done within the first twelve or twenty-four hours all will be well. Have no regard for the fracture; play for safety where the circulation is concerned. The fracture and the function of the elbow can always be relieved subsequently, but, in spite of Professor Hey Groves's statement of Sir Robert Jones's opinion on the improvement that is possible in the Volkmann complication, I maintain that present-day methods in most cases can do nothing more for ischaemic contracture than improve the cosmetic appearance of the hand and forearm.

Several other points in Professor Hey Groves's article are open to discussion, and I should like to take them up with him; but please save me from going "exactly counter, as so often happens in our profession," which, as Mr. Thompson maintains, Mr. Roth did "without reason." Mr. Roth had, indeed, every reason to communicate the results of his considerable experience for the benefit of all those who have had to deal with these elbow fractures, and his remarks are not lightly to be turned down by Mr. Thompson, while the subject is so fresh in our minds, as being beside the point.—I am, etc.,

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Guy's Hospital, and the Royal National  
Orthopaedic Hospital.

London, June 4th.

SIR,—With regard to the origin of ischaemic paralysis, surely Mr. Roth will admit that a vascular lesion unassociated with fracture may produce this condition.

A few months ago I was called to a young girl, whose arm was lying on a pillow, extended at the elbow. On examination I noticed that the limb was cold, and found that she had no radial pulse. The supracondylar fracture was easily reduced, and the arm treated in the fully flexed position. A perfect anatomical and functional result followed, but the radial pulse had not reappeared eight weeks later. I take it that the radial pulse may disappear at once by pressure of fracture ends or during the next three or four days by thrombosis. It would be interesting to know if any orthopaedic surgeons have statistics of the absence or disappearance of the radial pulse in their wide experience of fractures about the elbow.—I am, etc.,

Liverpool, June 4th.

R. KENNON, F.R.C.S.

#### TETRA-ETHYL LEAD IN MOTOR SPIRIT.

SIR,—In the report of the second public meeting of the Committee of Inquiry on Tetra-Ethyl Lead in Motor Spirit which appeared in the *British Medical Journal* of May 19th (p. 871) there are several misquotations of the evidence I submitted. Thus reference is repeatedly made to two men (mechanics) who, it is erroneously stated, were medically examined by me. These men were examined, independently, by their own private doctors, and also, independently, by an expert clinician engaged in lead examinations of cancer patients undergoing lead chemotherapy. I was not consulted in these examinations, but I thought it proper to record, without comment, in my memorandum of evidence a statement of these independent clinical findings, for which I have no responsibility. The only submission I make in this connexion is that, in the circumstances of an additional exposure to lead, it is justifiable in the interests of the men themselves that they be kept under medical observation—the stricter the better. (I assume that these mechanics are habitually exposed to a small lead occupational risk.)

I regret that my memorandum of evidence, of which, as specially requested by the secretary to the committee, I had furnished a sufficient number of copies for circulation to the press, was not so distributed; had it been circulated these misquotations could not have occurred.

Numerous misquotations of the evidence have appeared in the lay press, and the *Times*, in its issue dated May 16th, not only makes the above misquotation, but also