some cause occasionally in the early stages of general anaesthesia. It is commoner in the later stages, and, I believe, constant in the dangerous conditions in which death is impending, unless influenced by drugs—for example, opium—or pathological conditions, such as adhesions, or disease of the nervous system. A dilated pupil should be a signal (not necessarily of danger) to the anaesthetist to make doubly sure of the exact condition of the patient. While respiration and circulation are effectually proceeding, a widely-dilated pupil may soon contract on temporarily discontinuing the anaesthetic. Besides the conditions referred to it may be evidence of excess of anaesthesia, or exhaustion from shock or haemorrhage, and is frequently observed as a precursor of vomiting. A contracted pupil is always reassuring unless produced by drugs, for example, opium or morphine, or by disease. Dilatation of the pupil during chloroform administration may arise from any of the various causes already suggested, but when it is due to too high a percentage of chloroform in the system it is perilous to ignore it.

By implication a dilated pupil is not a *necessary* accompaniment of any part of chloroformization for *operations*, and dilatation generally occurs when the anaesthetic is pushed beyond that, and probably always when death is impending. *Vide* the address, there are many circumstances influencing the phenomena appearing during anaesthetization and operation besides the anaesthetic. The pupil is no reliable guide to the state of anaesthesia although a useful signal of the condition of the patient. Let me quote Snow, Hewitt, and Gill upon the "pupil," and I hope to do them no injustice in observing necessary brevity.

Snow spoke of five degrees[1] of chloroform anaesthesia, the fifth terminating as in death from asphyxia. Snow *specially* draws attention to his use of the word *degrees* rather than *stages*. He says "the pupils are dilated in the fourth degree..........They are occasionally dilated, however, under the slighter effects of chloroform, and even as the patient is recovering from its effects..........In the third degree of narcotism......the pupils are usually, if not always, contracted..........The pupils are acted on by other causes, both internal and external, as well as the chloroform.... Some writers have entered into a good deal of detail about the pupils, but their statements are very conflicting."

Dr. Hewitt[2] describes first, second, and third "degrees or stages" of chloroform anaesthesia, his last being the stage of deep surgical anaesthesia. He says:

The pupils during this part of the administration (second degree) are as a general rule mobile and more or less dilated. They react sluggishly, or possibly not at all, to light.

Of his third degree he says:

All observers agree that it is usually contracted in deep chloroform anaesthesia, but the term "moderately contracted" seems to me to be more appropriate.......Occasionally, as with ether, it remains widely dilated throughout this stage, even though the anaesthetic has not been pushed too far.

Mr. Gill, in the *St. Bartholomew's Hospital Reports*, says:

The normal type (of) the effects of chloroform finds expression in a contracted pupil..........If chloroform be present in an overdose, the pupil is always dilated, the degree of dilatation corresponding to the degree of chloroform narcosis.

I think Mr. Gill says that in 2,000 administrations the pupil was contracted throughout in 96 per cent. He says of one of his exceptional cases:

With the difficult breathing brought about by an encroachment on the upper aperture of the air channels, the complexion became in a slight degree livid, the pupil commenced to dilate, and the pulse got feebler. As soon as oxygen was allowed to enter freely, all the gravity of the case disappeared forthwith.

Of another:

In the case above related "there was no dilatation of the pupil. Why? Because the accumulation of CO_2 was not allowed to reach that degree of CO_2 poisoning associated with a dilated pupil." To obtain clear and distinct ideas of the action of this anaesthetic we must enumerate all the causes operating with it, and isolate as accurately as we can their effects. Only thus shall we avoid the error of ascribing to $CHCl_3$ what is in reality not of $CHCl_3$.

Of a child aged 8 suffering from adenoids he says:

After a little while the pupil became pinpoint, indicating the anaesthetic degree of unconsciousness. Operation performed. Vomited. Pupil remaining pinpoint. But with the advent of returning consciousness the respiration was observed to become shallow, the face pale and covered with perspiration, and the pupil was widely dilated. This condition continued for a few minutes, when, before a second attack of vomiting, the breathing was arrested in full inspiration. When the second act of vomiting ceased, the little patient became conscious very quickly.

Mr. Gill also remarks:

The measure of anaesthesia—and the term anaesthesia is used to denote the lowest degree of unconsciousness which is needed for all operations, both large and small—is the contracted pupil. And so long as the case remains one of normal anaesthesia, the contracted pupil is the sign which gives confidence to the administrator.

Thus the sign of security in anaesthesia is the contracted pupil. There is inferred from that statement that all the other signs of the constant anaesthetic state are present.

Thus no universal proposition can be made concerning the contracted pupil; and the same applies also to the dilated pupil.

An anaesthetist has two chief aims: (1) The safety and comfort of the patient during and after the operation; (2) the attainment and continuance of surgical anaesthesia, that is, a sufficient depth of anaesthesia to allow of the smooth performance of the operation. He watches the signs of these, and does not identify each of the "degrees" any more than a surgeon operating upon a hernia identifies each of its anatomical coverings.—I am, etc.,

Kensington, W., Feb. 9th. RICKARD W. LLOYD.

[1] Snow, *On Chloroform and Other Anaesthetics*, 1858.
[2] Hewitt, *Anaesthetics and their Administration*, 1901.

THE BILBERRY IN TYPHOID FEVER.

SIR,—I have read with great interest in the BRITISH MEDICAL JOURNAL of February 7th, an article by Dr. Max Bernstein on the bilberry as a remedy in enteric fever and other infectious diseases of the intestines.

I heartily join with Dr. Bernstein in his contention that a highly important principle in the treatment of enteric fever is the neutralization in the intestinal canal of the specific bacilli which develop and multiply there. So long as those bacilli are left unharried, there will be material for fresh infection of the blood, and thereby, as Dr. Bernstein argues, an undue prolongation of the fever.

In the JOURNAL of May 14th, 1898, I published a paper on enteric fever in India, and its treatment on the antiseptic principle. In that paper will be found the following observations, which I wish to quote as bearing directly on the principle which underlies Dr. Bernstein's article:

I at once admit that it is not possible for the antiseptic agent we employ to enter the blood current in sufficient abundance to exert there any influence of importance, on the product of the bacilli, that is, the typhotoxin, which we assume to be the prime cause of the group of symptoms which, taken together, make up our conception of enteric fever. But we must remember that the bacilli which produce the typhotoxin grow and multiply in the intestinal canal. This being admitted, it is reasonable to suggest that while in the intestine the potency of the bacilli can, to a large extent, be neutralized by the free administration of some reliable antiseptic; on doing so we cut off a large dépôt, so to speak, for the production of the specific fever element. Looked at from that standpoint, the production and maintenance of intestinal antisepsis is, I submit, a remedial measure of high importance. It renders harmless, to a great extent, the bacilli in the intestinal canal: in other words, it cuts off "the reserves" which supply to the blood the fever poison.

The publication of Dr. Bernstein's very suggestive article gives me an opportunity for once more pressing on the readers of the JOURNAL the claim for favourable recognition of a method of treating enteric fever which must ever be specially associated with the advocacy of its pioneer, Dr. Burney Yeo.—I am, etc.,

R. H. QUILL, M.D.,
Dover, Feb. 8th. Colonel R.A.M.C.

RESTORATION OF FUNCTION AFTER SUTURE OF NERVES.

SIR,—On reading your leader in the BRITISH MEDICAL JOURNAL of February 7th on the above subject it occurred to me that an explanation of the varying period after suture requisite for the restoration of sensation might be found in the fact that the operation has been performed at varying intervals after the injury. After section of a nerve degeneration takes place in the peripheral end whether the cut ends are immediately sutured or not, but Ballance and Stewart[1] have given proof that after an interval of about three weeks regeneration begins in the peripheral portion of the nerve quite independently of the central, and the two processes of degeneration and regeneration go on together. Complete restoration of structure does not, however, take place unless the ends are sutured. It may be expected, therefore, that after suture the return of sensation will occur the earlier the more advanced the process of regeneration in the peripheral end, that is, within limits, the longer the period that has elapsed after the injury. If, however, the ends of the nerve are sutured immediately after the injury sensation may return almost at once and remain for a short time, owing to the fact that the peripheral portion retains its excitability for a period, but one would expect anaesthesia to follow later.

With regard to the manner in which the nervous impulse is transmitted between the two ends of the sutured nerve before organic union has occurred, I venture to suggest an alternative explanation to that advanced by Sir William Banks. It might be accounted for in a similar way to that of "secondary

[1] BRITISH MEDICAL JOURNAL, September 27th, 1902.

contraction" in muscle. Although the fibres in the peripheral end are not structurally perfect, one may assume that they possess a certain amount of conducting power, and if an impulse be started at the skin it will travel along the regenerated portion, accompanied, as is known, by an electrical change. The electrical change, on reaching the region of the suture, will stimulate the hyper-excitable (because recently trimmed) central end placed in apposition. whence an impulse will be transmitted to the spinal cord. The electrical change is very minute, much less than in muscle, but it seems more reasonable to explain the transmission of the nervous impulse in this way than to assume a process analogous to wireless telegraphy. It would thus be a matter simply of electrical conduction.

I do not know if Ballance and Stewart believe that motor as well as sensory fibres are regenerated peripherally, but Fleming[2] states that the evidence is all against the regeneration of the motor fibres in this way. If the latter is correct it is easy to understand why motor power is restored much later than sensation.—I am, etc.,

Hull, Feb. 9th. E. E. LASLETT.

THE COLNEY HATCH FIRE.

SIR,—Every one in any way responsible for asylum management must feel that everything possible should be done to prevent a recurrence of such a catastrophe.

When I was in the United States two years ago inquiring into the insane and epileptic hospitals there I found that in the wooden buildings they had had frequent fires; these usually broke out in the laundries and inquiries carefully carried out showed that in many cases the cause was the dust that had accumulated on the hot pipes becoming ignited. Looking at the view in the *Graphic* and remembering the high wind prevailing at the time this may possibly have been the cause at Colney Hatch.

Wooden buildings have now been prohibited in the United States and thin embossed steel is largely used for ceilings and dados in the hospitals for the insane and epileptic, it certainly gives a more finished appearance and does not chip off like plaster.—I am, etc.,

Didsbury, Feb. 4th. JNO. MILSON RHODES, M.D.

AN IMPROVED HYPOBROMITE PROCESS FOR THE ESTIMATION OF UREA.

SIR,—As Dr. Pechell left for South Africa on January 30th perhaps you will allow me, shortly, to reply to the letters of Dr. Gilchrist and Mr. Longworth.

1. I acknowledge our ignorance that Dr. Pechell had made a rediscovery. Nevertheless, a considerable amount of time was spent by him at the Library of the College of Surgeons, after we had exhausted the half-dozen books in the Laboratory that were likely to help him, and subsequent to the appearance of Dr. Gilchrist's letter I spent an afternoon at the library of the Medical and Chirurgical Society investigating the point. As a result of these and other inquiries I can unhesitatingly say that the fact contained in Dr. Pechell's paper is not a matter of common knowledge amongst medical writers. The best reference that I have found is in a paper by Noël Paton in the *Practitioner* for 1886.

2. All who have had much to do with research are aware how difficult it is to be certain that one has not overlooked a paper by one's own countrymen, to say nothing of foreign literature. Dr. Gilchrist is exceptionally placed with regard to the work of French authors, but I made inquiries of Dr. Pechell with regard to his knowledge of French and German before he commenced to search for literature. Probably his failure to find the papers of Méhu and others is partly due to the fact that he is a comparative novice in research.

3. To Dr. Gilchrist's complaint that English investigators are unacquainted with French scientific work, I would only say that if he were resident in England he would soon find that there is another and a very different side of the question. —I am, etc.,

W. S. LAZARUS-BARLOW.

The Clinical Laboratory, Westminster Hospital, Feb. 7th.

*** This correspondence is closed.

THE MAGNESIUM WIRE TEST FOR LEAD.

SIR,—I have read with much interest the article on the magnesium wire deposition test for lead in the BRITISH MEDICAL JOURNAL of January 31st, p. 242, by Dr. Jacob and Mr. Trotman. As one who was responsible for much of the chemical work in the communication referred to in Dr. Abram's paper[1] quoted by Dr. Jacob, perhaps you will be good enough to allow me a little space to discuss this.

The process put forward by us in 1896[2] was a clinical method whereby the presence of lead in small quantities could be ascertained with accuracy and dispatch, and was in no way meant to be anything beyond this. We were perfectly familiar with the process of electrolytic deposition of lead by which an accurate determination of the quantity of the metal in a given volume of liquid could be estimated, in fact this is referred to in both our above-mentioned papers.

In our hands the simple method we put forward has always worked satisfactorily. Using pure magnesium foil there is no precipitate of lead iodide in a control experiment with distilled water, or normal urine treated as we suggest; upon the addition to either of even so small a quantity of lead as one in fifty thousand, however, a canary-yellow precipitate of lead iodide is produced.

There is no doubt that in urine not containing lead the magnesium becomes stained a yellowish-brown colour, but this is very different from the characteristic colour of lead iodide.

The ordinary magnesium ribbon being frequently contaminated we used for our experiments a pure foil which we obtained from Messrs. Johnson, Matthey and Co., and this has proved most satisfactory.

With regard to the conclusion of Dr. Jacob that "a test having a limit of 1 in 50,000 will fail," we did not think it necessary to mention to any one familiar with analytical work that a very weak solution of lead in an organic fluid might be concentrated by evaporation before applying our test. We are aware that the lead is excreted in an organic combination, and it was a knowledge of this fact which led us to employ an electrolytic method for the detection of the metal.

Since reading Dr. Jacob's paper I have repeated the experiment with normal urine treated as we describe, and in no case did I obtain any yellow colour indicative of lead iodide.

In conclusion, may I say that I consider the process put forward by Dr. Abram and myself in 1896 is still the most satisfactory one for the clinical detection of lead in pathological organic fluids.—I am, etc.,

PROSPER H. MARSDEN, F.C.S.,
Lecturer in Pharmacy and Demonstrator in Materia Medica.
University College, Liverpool, Feb. 3rd.

ROYAL NAVY AND ARMY MEDICAL SERVICES.

THE WAR COMMISSION.

AT the meeting of the War Commission last week evidence was given by Sir Frederick Treves, who expressed his views as to the over-organization of the Army Medical Service at the date of the war, the need of decentralization, and of giving to the individual officer more freedom of action and emancipation from merely clerical work. He gave evidence as to the inadequacy or unsuitability of much of the stores, instruments, and ambulance at the date of the war, as to the training of army medical officers, and as to military hospitals generally. He also gave explanations as to the recent action of the Medical Advisory Board at the War Office and the steps which have been taken since the war, or are now in contemplation, to reform the defects of the Army Medical Service.

THE SOLDIER'S SHIRT.

ARMY ORDER 28 authorizes the issue of an extra shirt to the infantry in June next. Hitherto the men have had only two shirts; it is satisfactory to find that an end is to be put to this uncleanly arrangement. Another move in the same direction was made some little time ago, when orders were given that the sheets of the beds should be changed every fortnight instead of once a month.

MALE NURSES IN THE ARMY.

A COMPREHENSIVE scheme of reorganization aiming at a differentiation of the duties of non-commissioned officers and men of the Royal Army Medical Corps has been under consideration at the War Office.

It has been decided to recommend that the non-commis-

[2] Allbutt's *System of Medicine*, vol. vi, p. 627.

[1] Three Cases of Lead Poisoning, with a note on a Simple Method for the Detection of Lead in Organic Fluids, by John Hill Abram, M.D. London, M.R.C.P.—The *Lancet*, January 16th, 1897.

[2] The Detection of Lead in Organic Fluids, by John Hill Abram, M.D. London, M.R.C.P., and Prosper H. Marsden, F.C.S.—British Association, Liverpool, 1896.