in a patient who has acquired considerable tolerance. Often, indeed, a fit or a succession of fits is a premonitory symptom of delirium tremens. And the onset of the latter is frequently assured by the treatment adopted for the former. In this way. A chronic alcoholic, owing to unusual circumstances, finds himself forced to abstain for, say, fifteen or twenty hours. As a result he has a major fit. The medical man summoned in this emergency always, so far as I can gather, emphatically enjoins strict abstinence from alcohol. The fit may or may not recur, but in either event an attack of delirium tremens is quite likely to supervene after an interval of two or three days. I have seen several such occurrences in consultation practice, two of which proved fatal.

In my opinion an attack of alcoholic epilepsy nearly always demands very cautious tapering, and the presence of marked albuminuria decidedly emphasizes the demand. The reason is simple. An alcoholic fit may sometimes occur in alcoholists who have established only low tolerance; in such there will be little risk of subsequent delirium tremens. But marked albuminuria usually points to prolonged, steady drinking and consequent high tolerance, the which greatly increases the risk of delirium.

It is possible in many cases to abort an attack of delirium which is already present by the proper administration of alcohol. But for success two things are essential. The attack must be in a quite early stage, and the administration of alcohol must be prompt and adequate; it has been found necessary to give as much as 16 fluid ounces of whisky or some other spirit within three or four hours. Given under these conditions the alcohol does not intoxicate but acts as a pure hypnotic. Subsequently tapering on the usual lines should be carried out.

It would seem that the significance of acquired tolerance of alcohol has been very largely overlooked; indeed, I have not yet come across in works on alcoholism any reference to the existence of such a condition.—I am, etc.,

Beckenham, Jan. 18th.

FRANCIS HARE.

SIR,—A remark made, with reference to delirium tremens, in the review of Dr. Mercier's *Textbook of Insanity and other Mental Diseases*, which appeared in the *JOURNAL* of December 19th, 1914, seems to have led to some misconception on the part of some of your correspondents. This was probably due in part to the fact that I did not actually quote the statements that I deemed to be "scarcely justified."

As the point at issue may be regarded as of some practical importance, will you allow me now to quote from the book itself? It is there stated (pp. 241, 242) that delirium tremens "has been shown by Dr. Francis Hare to be due to the sudden withdrawal of the whole or a great part of the customary daily quantum of alcohol of those who are accustomed to take regularly a large quantity"; "that an attack of delirium tremens can be cut short with certainty at an early stage by the administration of the customary dose of alcohol"; and "that an attack can infallibly be produced in any one who has been accustomed for some time to take large doses of alcohol by cutting off or materially diminishing the customary quantity." (The italics are mine.)

These statements seem to me to be too sweeping. (1) There are many cases in which attacks take place when there has been no cessation or diminution of excessive drinking; (2) there are many cases in which the attack cannot be cut short in any stage by the mere administration of alcohol in any quantity; (3) although there may be, and probably are, many cases in which an attack does supervene on cutting off or materially diminishing the customary supply of alcohol of a person who is drinking excessively and taking no other food (or practically in such cases the amount of other nourishment taken is negligible), yet there certainly are many cases of threatened delirium tremens in which unremitting attention to nursing and nourishment (sometimes with the aid of forced feeding), and appropriate medicinal treatment, will work even a rapid cure in conjunction with complete stoppage of alcohol.

I am well aware that the doctrines of the certain danger of stopping the alcohol of excessive drinkers, and of the successful cutting short of delirium tremens by giving alcohol, are very old, and indeed were generally taught, almost dogmatically, up to less than half a century

ago; but I cannot doubt that the unqualified statements to which I have called attention will be considered by many experienced practitioners to need revision in the next edition of Dr. Mercier's invaluable book.—I am, etc.,

January 21st.

THE REVIEWER.

P.S.—Having seen, by your courtesy, an advance proof of Dr. Hare's interesting letter of January 18th, I beg permission to note (1) that every *sober* regular drinker of alcohol in any form must needs acquire a marked degree of tolerance, though he does not suffer from total abstinence; (2) that no mention is made by Dr. Hare of the important matter of nourishment in the treatment of delirium tremens.

COLOUR VISION THEORIES.

SIR,—Mr. Devereux Marshall, in last week's letter, says that he wished in his paper to point out that Dr. Edridge-Green's "deductions as well as his theory have been accepted by a very large body of scientific men." On reading his paper again I fail to find such evidence in support of the Edridge-Green theory, the bulk of the paper being in support of his tests for colour blindness, which I think no intelligent and unbiassed person disputes. At the International Congress in 1913 the favourable reception of Dr. Edridge-Green's paper was limited to his tests, but, as Mr. Devereux Marshall must remember, there were several who demurred to his theory.

May I point out to Dr. Edridge-Green that, in order to support his views on the colour vision of primitive man, which I admit are very captivating, it is necessary for him to show that at one time red and violet were the only colours recognized—that is, that then yellow and green were not seen as distinct hues. It is well known that yellow and green ornaments and pigments were used by the ancient Egyptians, and that yellow was certainly recognized in Homeric times; yet he baldly asserts that "all the evidence points to Homer's vision being dichromatic, and that he saw red and violet with a wide neutral region." This, in the face of the leading article on January 9th, in which express reference is made to the Homeric word *ξανθός*, or yellow. No doubt has ever been raised as to the meaning of this word, which is frequently used to describe the golden hair of Achilles and of Ulysses. In one single passage Homer uses the word with a slightly different meaning, *ξανθὰς ἵππους*, or bay mares, but it can never mean either red or violet. Dr. Edridge-Green entirely neglects this word, *ξανθός*, and urges, in support of his views, the two Homeric words, *λειδής* and *πορφύρεος*, which he asserts mean violet. I believe no Greek scholar, including Liddell and Scott, would admit that either of these words was used to define any definite colour in Homeric times. Further, *ζώνη* in the *Odyssey* means a snowdrop; it only meant a violet in post-Homeric times.

Dr. Edridge-Green expresses surprise at my statements about contrast colours. It is necessary to distinguish between Simultaneous and Successive contrast colours. When one part of the retina is stimulated and attention is directed to a different part of the retina, Simultaneous contrast effects may be induced (as in the instance with green paper on which is a white or grey patch, the whole being covered with tissue paper). These Simultaneous contrast colours are most effectively excited, as Dr. Edridge-Green rightly says, by pale, not saturated, colours.

Successive contrast colours are, however, most distinctly seen when the inducing colour is a saturated one. These are the phenomena that occur when, whether by moving the eye or by moving the coloured object, the same part of the retina is subjected to now one and now to another kind of stimulation, as, for instance, in the old advertisements of Pears' soap.

Dr. Edridge-Green is an interesting psychological study. Here is a man who has a sincere and honest faith in his theory. He tells us that he has searched in vain for facts that are inconsistent with it, yet when they are presented to him, or when the arguments that he adduces in support of his views are shown to be faulty, he seems incapable of appreciating the fact. This is surely the kindest way to regard his attitude. He asserts that red and violet were the only colours known to Homer. When it is pointed out that in Homer, in the *Zendavesta*, and in the *Norse Edda*

there is no word to express blue, but that Homer frequently uses a word that expresses yellow, he pays no attention to this word for yellow, but gives two Homeric words that never meant blue or violet in Homeric times. Again, in my previous letter I showed that he failed to distinguish between mixing colour sensations and mixing coloured pigments; and now I find that he shows a similar want of discrimination between Simultaneous contrast and Successive contrast effects. Further, his meaning is exceedingly difficult to follow: I would instance his explanation of Benham's top that I quoted in full on January 16th; what impression does he intend to convey to his readers by the expression "red green"?

At the same time his practical tests, and his classification of cases of colour blindness deserve all praise, and his theoretic ideas on the extremely difficult subject of colour vision may possibly be of value from a scientific point of view, although he is unable to express them precisely in words; it may be that they are incapable of such expression.—I am, etc.,

Newcastle-upon-Tyne, Jan. 23rd.

A. S. PERCIVAL.

THE "ELDERTON" CONTROL METHOD OF TESTING THE VALUE OF NEW TUBERCULIN.

SIR,—Dr. W. H. Wynn, who has become a convinced advocate of the method of large doses of tuberculin which I made the burden of my Parkes Weber Prize Essay in 1909, criticizes with just severity Dr. Batty Shaw's desultory observations and conclusions upon the value of tuberculin treatment.

Dr. Batty Shaw's work is self-condemned when he presumes to place reliance solely upon the most variable, most capricious, and most elusive phenomena—râles of various sorts—as the essential criteria for estimating improvement. He calmly ignores the recognized and trustworthy evidence furnished by such objective phenomena as the quality and quantity of sputum, the condition of the blood, the state of nutrition, controlled by analysis of the urine, the state of the temperature and pulse, and the general condition of the patient. Both he and Dr. Rigg attempt by this fantastic and untrustworthy scheme to prove, what is admitted by myself and all those who have used large doses, that these small doses have no effect at all in modifying either for better or for worse the tuberculous process. Nearly a quarter of a century ago these small doses were tried and proved to be useless. All statistics furnished by Dr. Lawrason Brown and others tend to show that these small doses are valueless.

It surprises me to find that Dr. Rigg¹ considers a rise of temperature to 93° F. is evidence of a reaction to a dose of 0.000001 c.cm. I have seen the temperature rise to 101° and even 102° after an *injectio vacua*, whether as a result of expectancy or due to some temporary disturbance of thermotaxis, is a matter of conjecture. A rise of temperature *per se* is not a reaction, even though many, who use small doses of tuberculin, will have it so.

I wish to warn the medical profession against sharing Dr. Batty Shaw's obvious error of imagining that the casual administration of a preparation called tuberculin, no matter what the doses may be, or how they are given, or for what period, is a satisfactory way of estimating the value of tuberculin treatment. I trust medical men will rather put their faith in the solid, careful, and unbiassed work of those who have long and carefully studied the various methods of using tuberculin. Nobody has the right either to applaud or to condemn any method of treating pulmonary tuberculosis unless he has watched the effects of treatment for at least five years. As yet Dr. Batty Shaw is not even an apprentice in the use of tuberculin. I would strongly recommend any medical man who has been captivated by Dr. Batty Shaw's way of estimating by means of râles the effects of useless doses of tuberculin in a small group of cases, not even controlled by after-examinations, to visit our tuberculin dispensary at 1, Manor Street, Chelsea, where he will learn by clear and direct evidence, firstly, that these small doses have no beneficial effect, and secondly, that in the majority of cases large doses bring about improvements which manifest themselves in diminution of sputum till it ceases, disappearance of tubercle bacilli from the sputum, increased body weight,

marked increase of energy, disappearance of cough, marked improvement of the temperature and pulse, improvement of the physical signs, and often apparent restoration to health.

Dr. Wynn has had abundant opportunities of observing the effects of tuberculin in large doses for a number of years, because it is now four years since he visited my tuberculin dispensary. He is one of the few medical men who wished to see things for himself, and his letter proves conclusively that he is an advocate of large doses, and agrees with me that small doses are useless. No sound authority upon tuberculin treatment would expect Dr. Batty Shaw's method to have any but a purely negative result. Dr. Wynn is a far better guide on the question of tuberculin treatment than Dr. Batty Shaw, because he has not been afraid to use doses large enough to have an effect.—I am, etc.,

W. CAMAC WILKINSON, M.D., F.R.C.P.,

January 25th.

Director, Tuberculin Dispensary.

RECOGNITION OF BELGIAN PRACTITIONERS IN THE UNITED KINGDOM.

SIR,—In pursuance of the Order in Council dated January 7th, 1915, by which the Medical Act, 1886, was made applicable to the Kingdom of Belgium, the Executive Committee of the General Medical Council has passed the following resolution:

That so long as the Order in Council of January 7th, 1915, remains in force, any person who produces evidence satisfactory to the Registrar that he has obtained the degree of Doctor of Medicine in any of the Belgian Universities of Brussels, Ghent, Liège, or Louvain, and is legally authorized to practise medicine, surgery, and midwifery in Belgium, and who satisfies the Registrar as to the other particulars specified in Section 12 of Part II of the Medical Act, 1886, shall be entitled to be registered in the *Foreign List of the Medical Register*.

It is necessary, according to the provisions of the Medical Act, 1886, that any person applying to be registered in respect of the degree of M.D. of any of the above-mentioned universities should produce to the Registrar documents or certificates as follows:

1. Evidence that he possesses the diploma of M.D.
2. Satisfactory evidence of identity.
3. Satisfactory evidence of good character.
4. Satisfactory evidence—
 - (a) That the applicant is not a British subject; or
 - (b) That, being a British subject—
 - (i) The said diploma was granted to the holder at a time when he was not domiciled in the United Kingdom, or in the course of a period of not less than five years, during the whole of which he resided out of the United Kingdom; or
 - (ii) He was practising medicine or surgery or a branch of medicine or surgery in the United Kingdom on January 7th, 1915, and that he had continuously practised the same either in the United Kingdom or elsewhere for a period of not less than ten years immediately preceding January 7th, 1915.

Application should be made to the Registrar for a form for the use of Belgian practitioners seeking registration. Belgian practitioners who fulfil the prescribed conditions may thereupon, without further examination, be inscribed in the *British Register* and acquire all the rights and privileges of duly qualified British practitioners.

The documents, the prescribed fee (Five pounds), and the cost (One shilling) of the Government stamp, should be transmitted to the address set forth hereunder:

The Registrar of the General Medical Council,
299, Oxford Street, London, W.

—I am, etc.,

A. J. COCKINGTON,

Offices of the General Medical Council,
London, W., Jan. 27th.

Acting Registrar.

BELGIAN STUDENTS AT CAMBRIDGE.

Textbooks of Anatomy Wanted.

SIR,—We are gradually building up a little Belgian University here for students who, for one reason or another, cannot go to the front, and for professors who are past the age of serving. Altogether we have between one hundred and two hundred students, and some fifteen to twenty professors.

¹ BRITISH MEDICAL JOURNAL, January 16th, p. 140.