

The question of giving a small dose of rum to men who are exposed to extreme wet and cold in the trenches is not to be answered by the accuracy of rifle shooting tests under entirely different conditions. If a man is shivering with cold his shooting will be so bad that any stimulant which causes skin warmth will improve his shooting, in spite of all charts and curves that are drawn up to the contrary under different conditions.

One of the great dangers of an extreme chill is to stop skin action, and to throw such a burden upon the other great excretory organs that serious organic mischief results. A dose of rum (in hot milk by preference) tends to set up skin action, and therefore under suitable conditions may rightly be recommended by the army medical authorities.—I am, etc.,

The Temple, London, Feb. 22nd.

JOSIAH OLDFIELD.

SIR,—There is joy in heaven over Sir Victor Horsley. In your issue of January 30th he made a number of surprising assertions without giving any evidence in support of them, and one of them was, on the face of it, grossly inaccurate. I, having been acquainted with Sir Victor Horsley's methods before—Shakespearian scholars will recognize the phrase—asked him for evidence of the others. He calls this request vulgar abuse, and excuses himself from giving the evidence on the ground that two years ago I made a slip in some figures. It seems a queer reason for refusing to give evidence of his own assertions. It is true that I did make a slip—I never had a head for figures. I did make a slip, but as soon as my mistake was pointed out I acknowledged it and withdrew it. When Sir Victor Horsley drew attention to my mistake, I did not call his doing so vulgar abuse. He now calls this mistake a lie, and I suppose he does not call this vulgar abuse. But Sir Victor Horsley's judgement on what is and what is not vulgar abuse is not to be trusted.

Sir Victor Horsley says there is no obligation on him to notice my request for evidence of his assertions. Then why does he notice it by calling it vulgar abuse? I suppose we have most of us made up our minds about the value of his unsupported assertions, and, for my part, I pay regard, not to what he may say, but to what he can prove; and his inability to prove his assertions on this occasion does not induce me to modify my attitude.

The only "aspersions" I made upon his article were that it was grossly inaccurate and verbally violent. He does not dispute these aspersions, so we must take it that he admits them to be just.

On the other hand, I was careful to give him credit where credit was due, and to welcome the change, from his treatment of the prison doctors to his solicitude for the good name and reputation of his profession; and this is why I say there is joy in heaven over him, for are we not told that there is joy in heaven over one sinner that repenteth? I venture to surmise that the joy is none the less that Sir Victor Horsley is not there to share in it. He would be storming at the archangels and teaching them their business, as he has stormed at the Lord Chief Justice, the Home Secretary, the prison doctors, and the heads of the army, and has taught them all their business. Alas! that he should have fallen on such evil times, when wisdom cries in the streets, or at any rate, bellows in the journals, and no man regards it!—I am, etc.,

Parkstone, Dorset, Feb. 20th.

CHAS. A. MERCIER.

COLOUR-VISION THEORIES.

SIR,—I have to thank Sir James Porter very heartily for his kind remarks.

In reply to Dr. Percival, I must confess that it is very difficult to make points clear when the facts are quite different from the statements usually accepted. There are certain men who can do this: Sir Lauder Brunton, who first communicated my theory to the Royal Society, is one, and I feel sure he could make it intelligible to nearly every reader of the BRITISH MEDICAL JOURNAL.

When I referred to the mixture of red and green making yellow, I alluded to the sensations produced by pure spectral colours. Perhaps it would be as well to compare theory and actual fact and show how minutely they correspond.

Theory.—The sense of light was developed first, and then the sense of colour, colours of the largest and smallest wave-lengths being differentiated first—that is, the red and the violet. This red and violet, as evolution proceeded, gradually encroached on the white region in the centre of the spectrum until they met and a new colour—green—was differentiated. As evolution proceeded a new colour—yellow—replaced the red-green of the previous trichromatic stage.

Fact.—There are innumerable varieties of dichromatic vision, ranging from those who can only differentiate the two ends of the spectrum to those who have a very small neutral division and are much less colour-blind. We also have a number of persons, amongst whom are some of the most eminent scientific men and best observers in this country, who declare that there are only three colours in the spectrum—red, green, and violet—and that the yellow and orange regions would be better described as red-green and the blue region as green-violet. I interpret the term red green in the same way as the normal use the term yellow-green—that is, that the yellow region appears to have a red and a green element in it. This is borne out on actual experiment. If a red light be shown first and then a yellow one, the yellow is designated green. If a green light be shown first and then a yellow, this is designated red. In the same way, a yellow light between two red lights is called green, if between two green lights red. It will be noticed that this is similar to the normal with a yellow-green light. If seen by the normal between two red lights it is called green, if between two green, yellow. This class is deficient in hue perception as compared with the normal, and in conditions of difficulty becomes dichromatic and makes dichromatic mistakes—that is, confuses red and green.

The various colours of the Benham top are caused by the factors I have given, and the mixture of the various sensations according to the position of the lines, the illumination and the rate of rotation.—I am, etc.,

London, Feb. 19th.

F. W. EDRIDGE-GREEN.

SIR,—In his interesting letter in your issue of February 20th, Mr. Percival writes: "According to the trichromatic theory, the primary colours are red, green, and violet, and white light can be produced by the mixture of the three primary colour sensations," etc.

But is this the ordinary trichromatic theory of vision? I once made an earnest and perhaps unsuccessful attempt to understand the theory as expounded by v. Helmholtz and v. Kries, and it seemed to amount to the following: If it be admitted that the effect of any colour stimulus upon the retino-cerebral mechanism can be reproduced by a mixture of not more than three chromatic stimuli, then it is reasonable to suppose that, on the sensory side, colour vision depends upon the coexistence of three independent "elements" or unit excitatory processes. The point is that the theory is an attempt to express physiological processes in terms of experimental facts, that is, the equivalents of stimuli, and has nothing to do with the psychological analysis of sensations.

Further, the theory is a generalization of the experimental facts. We cannot reproduce all the spectral colours from a mixture of red, green, and violet lights, since mixtures of green and violet appear less saturated than spectral green-blue, and to maintain the trichromatic equivalence of stimuli we have to proceed thus: Homogeneous green-blue mixed in certain proportions with red matches a certain mixture of green and violet, or, x parts of green-blue mixed with y parts of red match a parts of green mixed with b parts of violet. Hence we can write: x parts of green-blue match a parts of green plus b parts of violet minus y parts of red, which gives us the unmixable colour in terms of the three chosen colours in a formal way, but not in one capable of objective interpretation. As enunciated by v. Helmholtz, in the general form that colour vision is a function of three independent excitatory processes, the theory is difficult and perhaps impossible to disprove; but, so stated, it is not easy to apply to the immediate data. At the same time, the well-known experiments of G. J. Burch* afford some experimental justification of the hypothesis.—I am, etc.,

Loughton, Feb. 20th.

M. GREENWOOD, jun.

* *Phil. Trans.*, B, cxci. *Proc. Roy. Soc.*, lxxvi. *Ibid.*, B, 1905, p. 214.