

fair complexion, and such information as I have been able to obtain tends to confirm this opinion.

It is, however, unnecessary to go so far afield as the colonies to study the effects of migration. It confronts us at our very doors. The British people is largely a migrated community not yet fully adapted to its environment. When, however, the greater migrations into this country ended with the Normans, and local communities of mixed origin settled down into pastoral and agricultural pursuits, local types were gradually evolved through the circumscribed character of the said communities. These local types were, so to speak, specialised for particular climatic and social conditions, a variety of factors determining the preponderance of one or other racial type in surviving individuals.

In this way considerable progress must have been made towards a natural equilibrium of adaptation. At the beginning of the industrial era we were probably the most virile people in Europe. The rise of industrialism, however, together with greater facilities of transit, has signalled the disintegration of rural communities, the dislocation of rural types, and their migration into the cities, where the aggregation of mixed elements has been subjected to the stress of natural selection in a severe form pending the evolution of an industrial type.

Major Woodruff's suggested investigation among the "failures" has much to recommend it, but the difficulty lies in obtaining the coöperation of a sufficient number of adequately trained observers. When similar investigations have been attempted trouble has constantly arisen from disagreement as to standards, criteria, &c., and the inability to procure uniformly accurate returns. Your annotation closes with the suggestion that "the production of a higher mental type depends probably, at any rate in great degree, on first acquiring a physique perfectly adapted to its climatic environment." But surely the human brain has achieved its extraordinary development through man's long contention with environments to which he was *not* perfectly adapted, having left the regions of his origin to spread himself over the earth. It is only when he is crushed by too severe or sudden a change that he degenerates.

I am, Sir, yours faithfully,

Hampstead, N.W., Feb. 20th, 1911.

J. S. MACKINTOSH.

## A COMBINATION TEST FOR COLOUR VISION.

*To the Editor of THE LANCET.*

SIR,—Some of your readers may remember seeing an article which I published in THE LANCET of July 15th, 1893, with this title. This article described a lantern which I had brought out, and which in many particulars anticipated the lantern which Dr. F. W. Edridge-Green described in his very interesting Hunterian lectures on Feb. 1st and 3rd.

My telechrome, as it is called, consists of an apparatus in which six colours—yellow, blue, pale green, signal-green, rose, and red—are shown by transmitted light to the patient through two apertures (one at a time, of course). One of these apertures is the perspective, when seen at 6 metres, of a 6-inch lamp at 100 yards; the other of the same lamp at 2000 yards. With the exception of purple, which I did not consider necessary, the colours are almost identical, it would appear, with those of Dr. Edridge-Green's.

My test possesses two points, however, which I think have an advantage over Dr. Edridge-Green's. The first is that the illumination can be readily varied, and this is important; while the second point is that the reflected colours from, say, a set of Holmgren's wools are shown to the patient at the same time—and he is required to compare the luminous colours of the telechrome with the non-luminous colours of the wools. If he should be faulty in either case the test detects the fact.

In other respects the telechrome may be used much as Dr. Edridge-Green uses his lantern. I also employ a "fogging" glass—and originally had a neutral tint modifier as well, but discarded it eventually, as I found that simple ground glass was apparently quite sufficient. May I add in conclusion that my telechrome has been used regularly at the Admiralty for many years, with (I am told) excellent results? Staff-Surgeon Preston, R.N., spoke very favourably of it upon the Commission on Colour Vision in 1892—as may be seen in the report of

that committee published as a Blue-book—and it has continued to be in use ever since.

I am, Sir, yours faithfully,

A. ST. CLAIR BUXTON,

Consulting Surgeon to the Western Ophthalmic Hospital.

PS.—An illustrated leaflet on the telechrome may be obtained from Messrs. Curry and Paxton.

Mansfield-street, W., Feb. 16th, 1911.

## GIRLS' SCHOOLS, GAMES, AND NEURASTHENIA.

*To the Editor of THE LANCET.*

SIR,—It seems to me that Dr. Robert Jones has, in his letter in THE LANCET of Feb. 4th, to use a homely but expressive metaphor, got the wrong sow by the ear. It is true that in accounting for neurasthenia and disinclination for domestic life in girls educated in high-class schools, he mentions two factors—the nature of their studies and the nature of their games—but his letter is subsequently devoted wholly to inveighing against the latter. Now, if neurasthenia and disinclination for domestic life are particularly prevalent among this class of girls, I contend that it is more likely to be due to the first factor. Among the ancient Greeks, a people noted for even balance between and perfection of mind and body, the women were trained in gymnastics, and particular importance was attached to ball games. But is it among such girls, whose splendidly improved physique is the constant theme of all sufficiently long-lived observers, that neurasthenia, at any rate, is most prevalent? My experience is that it is among such a class as Board School mistresses of inferior physique, who have been exposed to severe mental competition, without the time and opportunity for games, that it is more often to be found.

I am, Sir, yours faithfully,

GILBERT E. MOULD.

The Grange, near Rotherham, Feb. 9th, 1911.

## EYE-STRAIN IN RELATION TO GENERAL HEALTH.

*To the Editor of THE LANCET.*

SIR,—In the article on the above-mentioned subject, contributed to this week's LANCET by Dr. James Hinshelwood, I find the following: "In fact, my experience is that the small amounts of astigmatism give rise to symptoms of discomfort more frequently than the high degrees. The explanation, I think, is that a person with a high degree of astigmatism makes no effort to see better, as his vision is so defective, *but that the smaller degrees of astigmatism, only causing a slight blurring, induce the patient to make a continuous accommodative effort to counteract it and get clear images, and hence cause continuous ciliary strain.*" I do not wish, in criticising a very interesting paper, to be captious, but I certainly demur to that part of the extract I have italicised. The inference here plainly is that certain isolated fibres of the ciliary muscle are capable of independent action, so that a static corneal astigmatism can be neutralised by a dynamic lenticular astigmatism. The ciliary muscle (so far as its meridional fibres are concerned) is one muscle controlled by one centre and not several muscles controlled by several centres. This being so, how can one part act independently of other parts? Will Dr. Hinshelwood kindly inform your many ophthalmic readers upon what recent investigations he bases his statement?

I am, Sir, yours faithfully,

London, Feb. 18th, 1911.

KENNETH CAMPBELL.

## THE HYPERÆMIA TREATMENT OF UNUNITED FRACTURE.

*To the Editor of THE LANCET.*

SIR,—I am led by Mr. A. E. Barker's interesting narrative in THE LANCET of Feb. 4th, giving a series of cases occurring in his practice in which the hyperæmia treatment of ununited fracture was strikingly successful, to relate the following example of its application.

A middle-aged gentleman, the victim of melancholia, leaped from his bedroom window, a height of 30 feet, falling on the lawn, and striking in his fall his left forearm on the