DISCUSSION AND CORRESPONDENCE.

EXPERIMENTS SHOWING THAT THE RÖNTGEN RAYS CANNOT BE POLARIZED BY DOUBLY REFRACTING MEDIA.

TO THE EDITOR OF SCIENCE: I have, to-day, made experiments which conclusively show that the Röntgen rays cannot be polarized by doubly refracting substances.

On six discs of glass, 0.15 mm. thick and 25 mm. in diameter, were placed very thin plates of Herapath's iodo-sulphate of quinine. The axes of these crystals crossed one another at various angles. When the axes of two plates were crossed at right angle no light was transmitted; the overlapping surfaces of the plates appearing *black*. If the Röntgen rays be polarizable, the Herapath crystals, crossed at right angles, should act as lead and not allow any of the Röntgen rays to be transmitted.

On the screen covering the photographic plate were cemented the six glass discs carrying the Herapath crystals; also, three discs of glass overlapping so that the Röntgen rays had to pass through 1, 2 and 3 thicknesses of the glass. The screening of these glasses served as standards with which to compare the action of the rays which had passed through one thickness of glass and the Herapathites. On the screen was also placed a square of yellow blotting paper, $\frac{3}{4}$ mm. thick, on which were placed Herapath crystals.

The screen of compressed brown paper was impervious to two hours' exposure to a powerful electric arc light.

On exposing the screen with the six discs and paper square to the Röntgen rays, in three experiments, for $\frac{1}{2}$ hour, 1 hour and for $2\frac{1}{2}$ hours, and developing, *no traces whatever* could be detected of the Herapath crystals on the photographs of the glass discs or on that of the paper square. The contour of the paper was just visible, only by very careful scrutiny. The photographs of the glass discs carrying the Herapathites were circles of uniform illumination; not the least mottling could be detected. Through a magnifying glass these circles appeared with a uniform grain exactly like, in illumination and grain, the photograph of the glass disc having nothing on its surface.

The thinness of these crystals, their powerful polarizing property compared with their thickness, and their low density of 1.8 are the reasons why they do not at all screen (unlike calcite and tourmaline), the Röntgen rays. These well-known facts induced me to make these experiments on Herapathites. They have confirmed in a very satisfactory manner what Röntgen has shown by his experiments, viz., that the X-rays are not polarized by their passage through doubly refracting media.

ALFRED M. MAYER.

COLOR VISION AND LIGHT.

IN the current number of *The Psychological Review* Mrs. C. Ladd Franklin has written some very appreciative words regarding my article on 'Vision' in the new edition of Johnson's Cyclopædia, but takes exception in very considerate terms to two points which may be worth a moment's attention. The first is to the statement that the retinal cones are sensitive to variations of color chiefly. This was written in connection with an enumeration of certain optical defects common to all eyes; and, of course, there was no intention to imply that the cones are insensitive to that combination of color variations which produces the sensation of white light. Indeed, a previous sentence on the same page may be found which does away with all uncertainty. Nevertheless, the word 'specially' may very appropriately be substituted for 'chiefly.'

The second point is of more importance—a protest against the implication that physicists are satisfied with Helmholtz's theory of vision. My statement that "this theory, with slight modification, is now quite generally accepted by physicists," does not assert that they are necessarily quite satisfied with it. Our opinions are confessedly tentative in proportion to the difficulty of settling the matter by crucial experiments. It is safe to say that no physicist expresses his view upon this subject with any approximation to the confidence with which he asserts the truth of Ohm's law in regard to electric currents. He is compelled to base his statement upon authority; for, as Mrs. Franklin very rightly says, "the physicists have nothing to do with a theory as to what goes on in the retina and in the brain." The practical question for him, therefore, is to choose between authorities.

No scientific man who has lived during the nineteenth century has been more successful in widely different fields than Helmholtz. During the last dozen years the words physicist and electrician have become differentiated; but both were applicable to him as a distinguished representative. As a mathematician he had few equals. All physicists regarded him as an exceptionally strong physiologist. Whether their view is shared by the psychologists it would perhaps not be proper for a physicist to say. While the domain of the physicist is now fairly well differentiated from that of the psychologist, it is not yet possible to separate the psychologist from the physiologist. If the physicist has been too ready to accept Helmholtz's view on a purely psychological topic, he is to some extent excusable in view of the high position attained by Helmholtz as an investigator in subjects about which the physicist is by special training capable of forming an opinion. No one will maintain that Helmholtz was infallible; but the aggregate of demonstrated mistakes made by him was so small in proportion to the number of important discoveries accomplished that his record may be safely compared with that of any living psychologist.

Upon what experiments, either crucial or even moderately satisfactory, can the psychologist to-day base any definite conclusion as to what goes on in the retina or in the brain during the act of vision? Can it be confidently said that we are yet much wiser than our grandfathers were in relation to this elusive problem? These skeptical questions are not meant to imply any lack of esteem for the valuable work which has been done in psychology, or of admiration for the great ability that is at present directed toward the solution of the difficulties which the psychologist boldly attacks. In accepting the hypothesis of Young that three different sets of nerves respond to the three fundamental color sensations Helmholtz fully recognized its uncertainty. He considered it equally probable that each fibril might serve for three activities completely distinct and independent of each other. (*Handbuch der physiologischen Optik*, p. 292.) This theory has been found so satisfactory from the physicist's standpoint that it is hard to see what advantage would be gained by rejecting it until something else is presented that can be established on better evidence. The case is quite analogous to the physicist's acceptance of the all-pervading, elastic, incompressible ether as the medium through which physical energy is propagated. The existence of some sort of medium in space has to be postulated as a necessity of thought; its properties we infer from the phenomena which are explained on the given assumption. The acceptance is provisional only; we are ready to abandon it as soon as better evidence is presented in behalf of some other theory. Thus far there has not been even a suggestion of better evidence.

If now the psychologists can all agree upon some theory which is quite as consistent with known facts, and which involves less violent assumptions than does the theory of Young and Helmholtz, the physicists will assuredly be ready to welcome what seems to be new truth. To criticise is much easier than to construct. There is practical unanimity among the physicists just at present, but for the psychologists the same can by no means be said. For some time the leading competitor of the Young and Helmholtz theory was that of Hering—a theory which is less simple, and based on assumptions quite as difficult. But we are now informed that "there is one important university in this country in which the theories of Helmholtz and Hering have both been definitely given up, and particularly in the physical department." Granting this, the physicists elsewhere are justified in asking what they should now accept, and what are the positive grounds for acceptance. Several new theories of vision have been propounded within the last few years. One is by Ebbinghaus (*Theorie des Farbensehens*, 1893); another, which is very attractive, is due to Mrs. Franklin; and still another, by Nicati, has been brought forward within the last few months. This is somewhat bewildering for the physicists, who must be modest enough to wait until the psychologists come to an agreement among themselves. It may be true that the Helmholtz theory is preëvolutionary and prepsychological; but the physicists have their hands too full to stop and examine all these competing theories. To test them is the privilege of the psychologists. Pending the establishment of some one of these new theories by an exhibition of approximate unanimity among the psychologists, the rest of us will be apt to content ourselves as best we may with the theory of vision that has thus far seemed no more objectionable than its successors, and which is fortified by the authority of the greatest German physicist of the nineteenth century.

We are fully aware of certain facts in the history of science that may quite naturally be cited in this connection. The great authority of Newton caused more than a century of delay in the acceptance of the undulatory theory of light. The modification of this theory by Maxwell received but a small share of the credit it deserved until Hertz published the experimental evidence upon which light was shown to be very probably an electro-magnetic phenomenon. As soon as any new theory of visual perception is established upon evidence comparable with that brought out by Hertz, if it conflicts with the Helmholtz theory of vision, this will become of only historic interest, like the emission theory of light. Its fate, however, has not yet been sealed.

In this connection it may be permissible to express my hearty accordance with the views set forth by Mrs. Franklin in a recent contribution to *The Nation* regarding the desirability of greater precision in the use of the word 'light.' The meaning of a word is determined by custom rather than argument. But custom may be gradually modified if those who have occasion most frequently to use a special word or form of expression will agree among themselves to guard against ambiguity. No careful physicist at present includes the ultra-violet or infrared ether vibrations among light vibrations. The distinction between luminous and non-luminous energy waves is generally accepted and applied. But we need to habituate ourselves to the use of the term 'light-sensations,' rather than 'light,' when reference is made to what is carried to the brain by the optic nerve, whether the origin of the sensation is found in luminous, electric or mechanical energy. The American sense of linguistic æsthetics may be depended upon to prevent the adoption of such cumbrous unhyphened compound words as are tolerated by our German friends. But the scientific demand is for clearness combined with accuracy, for an application of the doctrine of conservation of energy in the giving and taking of ideas. Whatever differences may exist between the physicist and psychologist regarding the explanation of light-sensation, they can certainly clasp hands and agree not to deceive each other by unnecessary vagueness in the use of language.

W. LE CONTE STEVENS.

THE PHILADELPHIA BRICK CLAYS, ET AL.

I HAD not thought there was occasion for responding to the article of Prof. G. Frederick Wright (SCIENCE, No. 59, p. 242), until inquiry concerning the truth of the matters touched upon began to be made by correspondents. I shall not now take space to state the case fully, but only to say that the term 'Columbia,' as used by Prof. Wright, and indeed as it has been generally used in the past, is a somewhat ambiguous one. It has been made to cover formations, chiefly extra-glacial, widely separated in time, ranging indeed from the beginning of the glacial period nearly to the present. The Jamesburg formation of New Jersey falls within the limits of the Columbia, according to this usage, but the term Jamesburg has never been extended to the extra-morainic drift discussed somewhat fully in the New Jersey geological report of 1893. Most of the Jamesburg deposits of New Jersey are, I take it, relatively young, as indicated by Prof. Wright's citation from my report. But if I interpret rightly, there are remnants of a much older division of the 'Columbia' formation, not referred to explicitly in the report from which Prof. Wright quotes. These remnants are in scattered patches, and are quantitatively unimportant; but they are, as I believe, very significant. If present interpretations be right, there was very extensive erosion after the deposition of the formation of which these patches are remnants, this erosion antedating the deposition of the great body of material which passes under the name of 'Columbia.' Just where in the complex 'Columbia' the 'Philadelphia brick clays' belong, is a question I have nowhere