After-Images
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second surface, ground to fit it with accuracy when the mirror is in proper shape. If the mirror rested directly in contact with this second surface, no advantage would be gained, since the backing itself would bend as readily as the mirror. Therefore between the two is inserted a thin stratum of some elastic substance. Mr. Henry has found a fine sheet of flannel to give the best results. The effect of the sheet is to diminish the flexure of the mirror by a fraction depending on its stiffness and on the elasticity of the flannel. Theoretically it may be considered imperfect, because, in order to act, some stiffness is required in the mirror itself. A perfectly flexible mirror would bend just as much with the flannel as without it. But the flexure of the mirror can, it appears to me, be reduced to quite a small fraction of its amount. Moreover, I see no insuperable objection to the superposition of two systems of the kind; the mirror resting upon a stiff disk, which is itself supported upon a second one. This plan has been entirely successful in the cases in which it has been applied. Mirrors up to twelve inches in length show not the slightest flexure when moved into all practical positions. Unfortunately it has not yet been tried with reflectors of a larger size.

SIMON NEWCOMB.

AFTER-IMAGES.

THAT one cannot well contribute to a subject unless he knows something of its literature is illustrated afresh in a painstaking article by Mr. Sydney Hodges, in the October number of the *Nineteenth century*, on 'After-images.' Mr. Hodges has discovered for himself the fact that the after-images of bright objects are in general colored, and that they change color as they gradually fade away in the dark field of vision when the eyes have been covered. He has very carefully observed the phenomena in his own case; and he comes to the conclusion, that, in all cases of such after-images, "the color of the image is produced, not by the tint of the object we look at, but by the amount of light thrown on the retina, either by the greater or less intensity of light in the object itself, or by the amount of time during which one looks at it." This remarkable result is, however, reached by experiments that cannot prove it: for in all of them the conditions are too complex; namely, in all the important cases, our experimenter observed the bright object for a comparatively long time before covering the eyes. The common theory of these phenomena, however, assumes, that, after such a continued observation, the causes of the colors in the after-image are decidedly complex; and their complexity may be such as to render a complete explanation of the phenomena wholly impossible. Therefore the only simple way to begin observing the phenomena is to get instantaneously produced after-images, and to observe the order of colors in them as they disappear: for the common theory is substantially, that the separate nervous elements, whatever they are, that respond to the different wave-lengths, or that produce, when excited, the three primary color-sensations, recover from the after-effects of excitement with different degrees of rapidity, and again, if continuously excited, yield to exhaustion with various degrees of speed; so that the color of the after-image at each instant, since it must depend on the mixture of the different after-effects in the different elements, must vary as these elements return, each at its own rate, to the condition of rest, and must so depend, not only on the rates of recovery of each element, but also upon the degree of exhaustion that each element has undergone during the time of stimulus. Hence the simplest case would be the one where the degree of previous excitement was as nearly as possible equal for the different elements, — a case which would be realized best through momentary stimulus. But if the stimulus is continued ten or twenty seconds, then the after-image will be further affected by the rates at which the different elements have tended to get exhausted; and if these rates are themselves quite different, as is likely, then the after-image will be determined in its successive colors, not only by the different rates of subsidence of excitement in the elements, but by the different degrees of previous exhaustion: and all this may possibly so complicate things as to make the phenomena of the after-image seem wholly out of relation to the color of the original object. And thus any such uniformity as our author notices will be of little worth, unless we know just the conditions of time and illumination, and unless we observe the results with very many persons; and even then the facts may turn out to be too complex for us to explain, so that no light will be thrown by them on the theory of after-images.

All this Mr. Hodges could have found stated or implied in many places. The phenomena have been much observed and discussed. Helmholtz gives the older literature in § 23 of his Physiological optics, and himself declares that it is impossible, by reason of the complexity of the phenomena of fatigue, to give a complete explanation of these phases. Wundt, in the *Physiologische psychologie*, while not agreeing as to the theory with Helmholtz, still holds to an explanation somewhat analogous; and he considers, that, to avoid confusion, one must clearly separate the cases of instantaneous stimulation from the more complex ones, in which, as he implies, fatigue and other causes may affect the phenomena (*Op. cit.*, bd. i., p. 438, of 2d ed.). But of such separation our author is ignorant, and confuses all the phenomena in one mass together; so that observations that might easily have been made really valuable for the theory cannot well be used in their present shape at all, and can only raise in the casual reader's mind a false hope that a law has been found, when, in fact, as it is stated, the supposed law of our author is false, and is at once contradicted by the observation, added by himself in a footnote, that the "solar spectrum does produce the complementary colors in the after-image." For if the so confidently proclaimed law did not turn out true for saturated colors, the simplest of all perceptions of color, why did not our author suspect that he was on dangerous ground? As for the rest of his article, it is really no contribution to science, but contains an effort to refute the doctrine of fatigue in favor of some quite unintelligible explanation of after-pictures, and to edify the reader by general reflections.

We are far from being fully persuaded of the truth of the common theory, and have nothing ourselves to add to the discussion of the subject, save the present note of warning to solitary observers of mental phenomena. Let us all observe, by all means, and independently; but let us know what other people have said, or at least what the greatest men have said. Mr. Hodges is actually capable of believing and saying, at the outset of his article, such words as these: "I should add, that brief references to after-images with closed eyes may be found in Helmholtz's great work on Physiological optics, in Dr. Foster's Text-book of physiology, and in a few other works; but the fact that neither of them contains any detailed experiments (?) such as I am about to describe, induces me to hope," etc. And this Mr. Hodges could write, presumably with Helmholtz's book, § 23 and all, before him. What he is about to describe we have indicated. He looked at a window, and then covered his eyes; afterwards he tried the sun, colored cards, etc.; then he asked two or three people to try similar experiments; and then he wrote his article. And now who shall say that every intelligent man understands how to use even the best-known and best-arranged books? And why should the pages of the *Nineteenth century* be thus occupied? JOSIAH ROYCE.

LAKES OF THE GREAT BASIN.

As the geological observations given in a recent paper by Prof. E. D. Cope[1] relate to a region somewhat familiar to me, I venture to offer the following comments.

Under the heading of 'Preliminary observations' it is stated that the geologists of the Fortieth-parallel survey have shown that Lake Bonneville existed during tertiary time. It must be known to every one, however, who has read vol. i. of the reports of the survey mentioned, that this lake is there classed as quaternary: it has been so regarded by all geologists who have made any considerable study of the surface geology of Utah. Lake Lahontan is supposed, with good reason, to have been contemporaneous with Lake Bonneville, and therefore also of quaternary age. Recent observations tend to prove that the last great rise of these lakes was later than the greatest extension of the Sierra-Nevada glaciers, and perhaps synchronous with the Champlain epoch of the Atlantic coast.

Lake Bonneville was not named by the geologists of the Fortieth-parallel survey, as stated by Professor Cope, but was first so designated by Mr. Gilbert.[1]

The list of lakes given as now existing in the Lahontan basin should also include Honey Lake, California, as the valley in which it occurs formed a bay of the old lake with over three hundred feet of water. A map, showing the outline of Lake Lahontan as recently determined, will appear in the third annual report of the U. S. geological survey.

The prediction "that it will be shown that a third lake existed in Oregon, north of the supposed northern boundary of Lake Lahontan," has proved correct only in part. A geological reconnoissance conducted by myself in this region in the spring of 1882 has shown that the Great Basin, north of the hydrographic rim of Lake Lahontan, was divided during quaternary time into not less than ten independent hydrographic areas, each of which held a lake of small size, as compared with Bonneville and Lahontan.

The statement that "the lakes of the Great Basin in Nevada and Oregon diminish in alkalinity as we approach the Sierra Nevada Mountains," meets with a notable exception in Moro Lake, California, which lies at the immediate base of the highest portion of the mountains, but is yet, according to an analysis of its water made for me by Dr. F. W. Taylor, far more alkaline than any of the lakes of the Lahontan basin, excepting the soda-ponds at Ragtown, Nev.

Professor Cope also says, that "the lakes most remote from the mountains are not inhabited by fish, their only animal population being crustacea and the larvae of insects." That this conclusion is too broad is illustrated by the life of Humboldt Lake, which is inhabited by both fish and mollusks, and also that of Ruby and Franklin lakes, situated still farther eastward, which abound in molluscan life. That the freshness of lakes, and consequently their inhabitability by fishes and mollusks, do not depend on their relation to mountains, or even on the existence of an outlet, can be shown by numerous examples in the Great Basin. The only explanation of the apparent anomaly of an enclosed lake of comparative freshness (with less than one per cent of saline matter in solution) in the nearly desiccated basin of a far larger lake, which never overflowed, has been suggested by Mr. Gilbert.[2] His hypothesis is, that such lakes owe their freshness to complete desiccation and the burial of the precipitated salts beneath plaza deposits. When water re-occupies such a basin, the imprisoned salts may not be redissolved. It is evident that this process might take place in any part of an arid region like the Great Basin, whether it be near or remote from mountain ranges.

The locality mentioned on p. 137 as having furnished fossil remains is included within the still distinct beach-lines of an ancient lake which once filled the Christmas Lake and Silver Lake valleys. The shells collected at this locality by myself have been

[1] On the fishes of the recent and pliocene lakes of the western part of the Great Basin, and of the Idaho pliocene lake (*Proc. acad. nat. sc. Philad.*, June, 1883).

[1] Wheeler survey, vol. iii. pp. 88, 89.

[2] Second ann. rep. of U.S. geol. surv., p. 177.