and with the reduction of the stress, the curve drops, until very shortly the specimen is ruptured, and the apparatus comes to a standstill. The other three curves are those given by a piece of boiler-plate and of a specimen of muck-bar, and are very good examples of the value of the autographic method. As is well known, both boiler-plate and muck-bar are decidedly non-homogeneous; and as a result we have curves here that are exceedingly irregular, especially after passing the elastic limit. While they bear a general resemblance to the previous ones, they are full of points of inflection, turning and twisting about, and giving one an idea that the specimen consisted of a bundle of threads or fibres which gradually parted under the action of the stress, giving any thing but a constant and uniform action.

In conclusion, a word as to the practical accuracy of the lever testing-machine may not be out of place. The machine under consideration has been subjected to severe use for nearly two years, during which time its sensitiveness, even when loaded, has not risen so high as the least reading on the poise. From this, and from long practice in similar scale-work, it may be safely stated that the testing-machine, with proper care, may have an exceedingly long life. The attainment of absolute accuracy in any department of investigation, would, if it were possible, be an extremely desirable result; yet even our best experiments are simply close approximations to the truth, and it will be granted that it is desirable to make all of our improvements commensurate towards an absolute standard of accuracy. It is of no importance to carry the weighing-power of the testing-machine beyond the possibility of the measurement of the bar. For example: supposing the tests most frequently made are those of bars having about a square inch of cross-section. In a piece of iron an error of a thousandth of a square inch of cross-section corresponds to a possible inaccuracy, in the stress produced on the bar, of fifty pounds; while the corresponding quantity in a steel bar corresponds to about seventy to ninety pounds. There are very few lathes in the country in which it is possible to turn a bar so exactly that it shall be perfectly round, and that there shall be no variation from one end to the other of more than a thousandth of a square inch. There are few men that are capable of manipulating any lathe to produce such a result; and there are still fewer gauges that are capable of measuring even a perfect bar so as to exclude the possibility of an error as great as a thousandth of a square inch. Now, if it be impossible to measure our bars to within an error of fifty to a hundred pounds in the testing-machine, is it of any importance to refine the machine beyond this reading? In the lever system of testing-machines it is perfectly possible to obtain a machine which will uniformly and constantly give readings which shall not have a greater variation than from five to twenty pounds; and, if our bars can only be measured to fifty or a hundred pounds, would it not be wiser to spend money in refining the gauges rather than in refining the machine? Again: when the consideration of the tests on the full-sized members occurs, or bars direct from the rolls, carrying with them the scale, and other imperfections from the mill, the possibility of measuring to a thousandth of a square inch becomes absurd, and two or three hundredths is the nearest approximation that can be made. In making a test of an ordinary I-bar, of, say, five or six inches of cross-section, it is certain that the bar has any thing but an absolutely uniform section from end to end; and how long, may it be asked, would it take to measure that bar from end to end, so that the least cross-section could be obtained for the record? And again: in actual experience it has been frequently found, that, having obtained what is supposed to be the least cross-section, the test-piece may break in a totally different place. It will be conceded that practical engineers care very little for test-records beyond the hundredth's place of figures; and what the country wants at the present time, is not so much testing-machines constructed with a theoretical refinement of accuracy, as a large number of practical machines, so that one may be located in every iron-works in the country, and means to carry on the experiments and to obtain from these machines a practical knowledge of what America's constructive materials really are.

A. V. ABBOTT.

NEW METHOD OF MOUNTING REFLECTORS.[1]

IT is well known to all who have given attention to this subject, that the optical performance of great reflecting-telescopes has not been proportional to their size, and that the mechanical difficulties of keeping a large reflector in proper figure in different positions have been apparently insurmountable. A plan of supporting a large mirror, devised by Mr. Henry, has been adopted in Paris, which it is hoped may obviate this difficulty. It consists, in principle, in supporting the mirror upon a

[1] Extracted from a report to the secretary of the navy on improvements in astronomical instruments.

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 wavelengths, 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,