Experiments in dynamics succeed those on statics, in which velocity, acceleration, inertia, work, kinetic energy, and centrifugal force are measured. The properties of vectors are here developed, though not so fully as might be desired, and occasionally there is a vagueness of language which must give trouble to a student who tries to think clearly.

Remaining chapters deal with the efficiencies of simple machines; the application of the link polygon; the drawing of reciprocal figures for jointed plane frames; and the determination of Young's modulus and of moments of inertia. In addition to the examples appended to each chapter, the authors give, at the end of the book, more than three hundred miscellaneous examples, and copies of recent examination papers, both theoretical and practical. A good index is provided, and teachers will find this book very helpful indeed.

Air and Health. By R. C. Macfie. Pp. vii + 345. (London: Methuen and Co., 1909.) Price 7s. 6d. net.

This is a very readable work, containing much useful information. As the subject is dealt with in a manner which renders it interesting and easy of comprehension, even to people who may have no acquaintance with chemistry and physics, it should appeal to a wide range of readers; but it will prove of special service to the medical student of hygiene and preventive medicine, and the medical writer doubtless had this end mainly in view. The subject of the CO_2 respiratory impurity in the atmosphere is treated at some length. In this connection the author states (p. 133) that "Anyone who compares his power of mental work in a pure and in a CO_2 laden atmosphere, even if the latter be dry and cool, will find in the latter a considerable diminution, showing that at least nerve metabolism is affected." This statement implies that the CO_2 in a badly-ventilated room (as ordinarily understood) is capable, *per se*, of producing results which more recent experimental work demonstrates to be due to the combined effects of the altered physical conditions of the air—the increased moisture and temperature, in a stagnant atmosphere. The important subject of the composition, &c., of sewer air is inadequately discussed in eight lines.

Mr. Spence's suggestion that all the smoke of a city should be conducted by the sewers to a few enormous chimneys, where it could be completely burnt (p. 155), appears to be commended by the author. Certainly Sir B. W. Richardson proposed to adopt this method in his model city of Hygeia, but it is inconceivable that it could be efficiently applied. The various methods of freeing air from trade dusts and gases might have been included with advantage in such a work, and the volume seems incomplete without some reference to analytical methods—even if the reference were confined to the more simple practical tests for the more important gaseous impurities of the atmosphere of dwelling and working rooms.

But notwithstanding these deficiencies, the matter is, generally speaking, so well dealt with that the work may be confidently recommended for study and reference purposes.

The range of treatment embraces eighteen chapters. These deal with the composition of air; the discovery of the atmospheric gases; respiration; air-pressure in relation to life; the temperature, humidity, and thermolysis of air, and health; air and solar radiation; electricity and radio-activity, and their relation to climate; impure and polluted air; dust and germs; ventilation, draughts, and "colds"; artificial atmospheres; the open-air treatment of consumption; and open-air schools.

NO. 2101, VOL. 82]

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Natural History Museum.

IN reference to the article on the Natural History Museum in NATURE of January 20 (p. 343), in which Sir Archibald Geikie is represented as saying that the questions put to him in the letter from the Speaker of the House of Commons "were entirely in reference to the relations between the Trustees and the Museum," historical accuracy demands that it should be stated that the questions of Mr. Lowther were two in number (see NATURE, December 16, 1909, p. 196). They were:—(1) "Whether the Board of Trustees, acting through its Standing Committee, is in your judgment the best authority for the government of such an institution as the Natural History Departments of the British Museum?" and (2) "Whether, under the existing statutes and rules, the scientific management of the Natural History Museum suffers any detriment from its association with the Museum at Bloomsbury?"

It is clear that in his communication to you Sir Archibald Geikie has overlooked Mr. Lowther's second question, with which alone (so far as these two questions are concerned) the present agitation has concerned itself. We have always admitted that "a system of control by Trustees is the best" (see NATURE, December 30, 1909, p. 254). You were therefore quite right in your impression that the main point of contention was the complete separation of the governance and administration of the Natural History Museum from the other collections in the British Museum. Mr. Carruthers's letter does not touch the point at issue, except in so far as it seems to show that Prof. Huxley was in agreement with our view that "a system of control by Trustees is the best." He says nothing to show that Prof. Huxley came to disagree with the conclusion to which he had twice put his signature, once after a prolonged inquiry by Royal Commission, that it was of fundamental importance that the governance and administration of the two Museums should be separate.

A. SEDGWICK.

2 Sumner Place, S.W., January 30.

Markings on Mars as seen with Small and Large Telescopes.

THAT large telescopes are liable to less accurate definition of planetary markings than small ones can easily be verified in the following manner, and tested for any given occasion. The spurious disc and rings made of a star by a telescope is a real image, due to the interference of the light-waves—as real an image, although produced by a different cause, as that of a planet's disc with its markings. If atmosphere and glass be perfect, the image consists of a round disc, encircled by concentric and continuous rings of light. The only difference in the image with different apertures is that the larger the aperture, the smaller the disc, and the closer and thinner the rings. If this image assume any other form, it is either because the objective is poor, which is commoner with objectives than is supposed, as Hartmann's tests have shown,¹ or because the seeing is defective. In proportion as the seeing is bad the rings of the image begin to waver, then break up into fragments, a sort of mosaic, and finally end in an indeterminate assemblance of points. In certain kinds of bad seeing the parts may seem quite steady, but that the mosaic exists is proof positive of poor seeing.

Now this image as made by different apertures may be compared either by observing with different telescopes at the same place and time or by diaphragming down a large objective. When this is done it becomes at once evident that the smaller aperture always gives the more accurate definition of the optical disc and rings in spite of the theoretical greater resolvability of the larger glass. If,

¹ The Lowell Observatory glass has been tested by this method, and proves to yield the best images of any yet examined. (Bulletin in preparation.)

now, under the same conditions of seeing in which the rings appear continuous—their real form—in the smaller glass, the aperture be increased, the rings will be seen to break up into detached masses. It is very rare that they ever appear otherwise in a very large glass, for the seeing is almost never good enough thus to show them, accuracy of definition demanding much better air for a large than for a small aperture.

Now what happens to the lines of the image of a star—for the rings are simply circular lines—must happen in the same manner to planetary lines. They too must break up into a mosaic in a large glass whenever the rings do. This shows that planetary lines would necessarily assume a mosaic in a large glass contrary to their true form of continuous linearity.

PERCIVAL LOWELL.

Lowell Observatory, Flagstaff, Arizona.

Colour-blindness.

IN the summary of the case of Mr. John Trattles published in NATURE of January 27 the expression is used:—"Dr. Ettles, who had examined Mr. Trattles, and was of opinion that he was not colour-blind, was also present."

That is only relatively accurate, and it is in the reservation that the real point lies. I examined Mr. Trattles on May 10, 11, and 12 last year. His spectrum range included both B and H lines—actually, 0.75μ in the red to 0.39μ in the violet. His sense of colour was less good about 0.54μ than in the rest of the spectrum. Indeed, it was what one might call "bad." It was the presence of this defect which caused him to give such contradictory replies at Sir Wm. Abney's examination. My certificate, which was read in the House of Lords by Lord Muskerry in the debate referred to, viz. June 30, 1909, expressly stated that Trattles was competent to navigate a ship. It did not state that his colour vision was in all respects perfect.

Sir Wm. Abney and those associated with him concluded that, as the result of their tests, Trattles was "completely red-blind." I saw in those tests simply a confirmation of my own conclusions. We were at one in the symptoms, but we strongly differ in the diagnosis. If the Board of Trade starboard light were a yellow-green, Trattles would be unfit; but it is a blue-green, and he sees it perfectly.

As to red, he is anything but scoterythrous; that is what comes of being obsessed with a colour-vision theory.

One other point, the "recondite test of simultaneous contrast colours" is very much open to criticism. A simultaneous colour contrast is not a "colour" in the ordinary sense; it possesses no dominant wave-length; it is an optical illusion. Is an optical illusion so accepted and clear a test that it may be used as a touchstone to determine whether a man's career shall be ruined or not?

WILLIAM ETTLES.

34 Wimpole Street, Cavendish Square, W.,
January 30.

WE regret that we were in error in stating that Dr. Ettles was of opinion that Mr. Trattles was not colour-blind. It is interesting to know that he found the colour vision "less good" at a point in the spectrum not far removed from the region where the so-called "red-blind" person has a so-called "neutral band." The accurate determination of the spectrum range and of the deviations from the normal within that range constitutes the fundamental problem in discriminating colour-blindness. The accuracy of the determination, however, depends upon the methods adopted and the precision of the application of these methods.

With regard to the simultaneous contrast test, Dr. Ettles states that it is an optical illusion. If this be admitted, it is constant in its character in all normal individuals; hence any deviation may be fairly regarded as indicating a pathological condition of colour vision. As physiologists we cannot admit as a valid argument against the test that it is difficult or impossible satisfactorily to correlate the physical facts with the physiological manifestations.

NO. 2101, VOL. 82]

But is it necessary or advisable that the divergence of opinion on the exact conditions of Mr. Trattles's colour vision should be further emphasised? The case has been adjudicated upon, and has amply demonstrated the need of reform either in the tests or in the application of the tests. The Board of Trade cannot, and ought not, to accept any risk of allowing a man with possibly dangerous defective colour vision to pass the examination. It is better that slight hardship should fall upon a few individuals than that many lives should be endangered; but the hardship must be minimised, and this object will be best attained by ensuring that the individuals shall be eliminated as early as possible in their careers. Hence it is of the utmost importance that the first examination shall be authoritative and conclusive.

THE WRITER OF THE ARTICLE.

Records of the Earthquake of January 22.

ON January 22 the Kew Milne horizontal pendulum recorded a large earthquake, of which the preliminary tremors commenced at about 8h. 52.6m. a.m., and the large waves at about 8h. 56.1m. The limits of registration, 17 mm., were exceeded between 8h. 58m. and 8h. 59m., and again at 9h. 0.4m. Synchronous with both maxima there were burrs on the magnetic declination trace. The largest, which simulated an oscillation of $1.5'$ in declination, commenced about 8h. 57m., and lasted about five minutes. The movements on the declination trace were unquestionably seismic in character, and represented the mechanical effect of the principal earth tremors. The horizontal and vertical force traces were not sensibly affected.

CHARLES CHREE.

The National Physical Laboratory (Kew Observatory Department), Richmond, Surrey, January 27.

An Earthquake Phenomenon.

A CURIOUS phenomenon connected with the earthquake of January 22 was that the maximum movement was accompanied by a sudden tilt. The amount of this was approximately one second of arc, its direction being towards the north-west. This would correspond to a rise of the ground on the south-east. It took place at about 8 a.m., when the booms of five horizontal pendulums were suddenly displaced from their normal position. Those oriented east and west were swung to the north, whilst those at right angles moved to the west. Pendulums in rooms 80 yards apart were displaced similarly. From 12.45 they crept back somewhat intermittently towards their original position, which they reached about 4 p.m.

Whether this indicates a local change in the dip of the strata (chalk) on which my instruments are founded or a more extended change of level cannot be stated with any certainty. At Bidston a pendulum oriented east-west was displaced towards the north, and at West Bromwich a pair of pendulums swung more on one side of their normal position than on the other. Their behaviour suggested a displacement similar to that observed at Shide. Permanent changes in the ground near the origin of a large earthquake are common.

J. MILNE.

Shide, Isle of Wight.

The Preparation of Silicon. A Warning.

A COMMON method of preparing silicon is by mixing silica and magnesium powder in molecular proportions and heating until the following reaction takes place:—

The majority of text-books recommend the use of silica in the form of silver sand or ground quartz, but they do not say it is absolutely necessary, or even desirable, that it should be in this form, and one well-known treatise states that precipitated silica and pure magnesium powder will yield very pure silicon. This authority adds that if the experiment is performed with precipitated silica the reaction is attended by a brilliant flash of light.