it turns out that he himself is moving in a circle of fallacies. While denying the specific nature of the cholera virus, he explains this latter by the symptoms of the disease. Assuming, for the sake of argument, with Dr. Chapman, the particular disturbances of function in the cord and the sympathetic nerves to which the symptoms of cholera are due, how does this bring us nearer to the knowledge of what causes these particular disturbances? By saying, or even by showing, that such and such a disturbance in the function of the cord and sympathetic causes such and such a symptom of disease, we are not one iota nearer the answer to the question, Why did such and such a disturbance take place? what has caused it? The answer to this one wants to know, but this is not supplied by Dr. Chapman. It is quite true that a great many conditions are required to favour the outbreak and spread of cholera, *e.g.* conditions of temperature, water, atmospheric disturbances, soil, &c., &c., but all these conditions may be present without producing cholera, or typhoid fever, or any other similar disease. Why? Because *the actual cause* of the disease is absent. These two things, *viz.* secondary conditions favouring the outbreak and spread, and *the actual cause*, must be kept separate; but evidently Dr. Chapman has not arrived at this as yet.

The chapters VIII. to XV. describe the various methods of treatment of the disease, and they form the most important part of the book. E. KLEIN

Seaweeds, Shells, and Fossils. By Peter Gray, A.B.S., and B. B. Woodward. (London: Swan Sonnenschein and Co)

THE object of this book is to give to the young English collector a general knowledge how to set about collecting the more common seaweeds, shells, or fossils.

In the first case the subject is dealt with generally, classifying the different seaweeds and stating where each is most likely to be found, and, when found, the best way to press them and get them ready for the cabinet, the most economical way of making or obtaining which is given.

Secondly, shells are dealt with, descriptions and diagrams being added where necessary, together with a table of the more important genera, showing the approximate number of species belonging to each genus, and their distribution.

Thirdly, and lastly, different localities are pointed out where fossils are best found, and the best mode of arranging them in the cabinet. A table of the principal fossiliferous strata arranged in chronological order, with notes on the different formations mentioned in the table, and also of the principal divisions of the animal kingdom, are added to show the order in which the fossils should be arranged. S.

The Modernised "Templeton"; or, "The Practical Mechanic's Companion." By William Templeton. Revised and Modernised by Walter S. Hutton, C.E. (London: Crosby Lockwood and Co., 1886.)

TEMPLETON'S "Mechanic's Workshop Companion" is a work familiar to most mechanics and draughtsmen, having been considered for the last quarter of a century a useful book of reference by all connected with the management of engineering workshops and kindred trades. Books of this description require revising very often, and considering the enormous development of the mechanical sciences during the last few years no one will wonder on hearing that even "Templeton" has to be modernised to keep pace with the times.

The reviser tells us that he has endeavoured to follow as far as possible the lines of the original work, at the same time bringing all the information up to date. Much new matter has been added, giving information on air,

gas, water, and steam; methods of testing steam-engines and boilers; turbines and other water motors; the strength and weights of material; and miscellaneous information too numerous to give in detail.

The work has for a frontispiece an illustration of the fine compound locomotive "The Marchioness of Stafford," designed by Mr. F. W. Webb, the able locomotive superintendent of the London and North-Western Railway. On seeing this we are at once led to imagine that at last we have found a book giving recent data on locomotive engineering, and likely to fill a want seriously felt by those who study that most interesting branch of mechanical engineering. We are told to "see p. 360," to which we turn hoping to find a section devoted to locomotive work, having Mr. Webb's fine engine as an example of the latest advance. We find a third of a page giving the bare dimensions of the compound. Even the index makes matters no better, for the book contains no locomotive data whatever! Considering the thousands of mechanics engaged in this class of work, this is a great pity, and should be remedied in a future edition.

An excellent abstract is given of the results of experiments on riveted joints, with special reference to practical work, by Prof. Alexander B. W. Kennedy. This is most interesting and useful, and will well repay careful study by those connected with the manufacture of soft steel boilers and bridges.

The book contains all the usual tables, embracing every subject likely to be required by the intelligent mechanic or draughtsman, including extensive practical rules and data. Instruction is also given in the rudiments of arithmetic, algebra, and trigonometry. N. J. L.

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. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Foreign Fishery Boards

IN reference to recent statements in NATURE on this subject, it will interest your readers to know that in Italy the Government has constituted a Fishery Board, which, my friend Prof. Giglioli, of Florence, tells me, is actively employed in advising the executive and in inquiring into the grievances and difficulties of fishermen, and the suggestions for improvement of fisheries; it has also recently, at the expense of the Government, taken practical measures in the stocking of lakes with fish, and in the cultivation of sea-fish. This Commission is a branch of the "Ministry of Agriculture, Industry, and Commerce," which corresponds to our Board of Trade. *The members of the Fishery Commission, with the exception of the first three named below, are exclusively scientific men.* They are as follows:—The Permanent Under-Secretary of State for Agriculture; the Permanent Under-Secretary of State for Commerce; a distinguished lawyer; M. Minni, of Venice; Dr. Renier, of Chioggia (representing the fishermen of this island); M. Friedlander, of Comacchio (specially acquainted with the peculiar fish-culture of this district); Prof. Giglioli, Florence; Prof. Targioni-Tozzetti, Florence; Prof. Costa, Naples; Prof. de Vincentis, Taranto; Prof. Canestrini, Padua; Prof. Pavesi, Pavia; Prof. Issel, Genoa.

The Commission meets from time to time in Rome. The questions submitted to it are brought forward and referred separately to one or two members, who are requested to draw up a report on the particular subject thus referred. The report may take several months, and involve experiment or research, or it may be a simple matter. The report when presented is discussed by the whole Commission. The conclusions and recommendations which it embodies are modified by vote of the majority, and it is obvious from the constitution of the Board that the scientific experts have the voting strength.

The members of the Board or Commission are paid travelling

expenses and 25 francs a day each during its sessions in Rome, or other fixed place of meeting.

The constitution of this Board is admirable, since it secures a fair representation of the leading scientific men of Italy. The Italian Government does not, it seems, refer such questions exclusively to one individual, but endeavours to obtain a consensus of the scientific opinion of the country.

Hôtel de la Ville, Florence

E. RAY LANKESTER

Fabry's Comet and Barnard's Comet

NOT having seen any mention of the rapid apparent growth of the tail of Fabry's comet, probably some of your readers are not aware to how great a length it extended. On April 26 occurred the first fine night after a very unusual series of overcast ones, and about 14h. G.M.T. I was surprised to see the tail reaching up to, or at least to within 1° of, δ Cassiopeiæ, a distance of 38° from the place given in the ephemeris for the nucleus, which was far below the horizon; and the tail would doubtless have been visible to a greater distance but for the brightness of the Milky Way. The following night, about 10h., it reached at least up to the Cluster in Perseus, a distance also of 38° from the predicted position of the nucleus; it was very narrow both nights. The next night, which was pretty fine, I failed to find any trace of the tail.

The principal tail of Barnard's comet is also very narrow: on May 1 its length was $4\frac{1}{2}^{\circ}$, as seen with a pair of field-glasses. With the telescope this comet had also a faint tail *nz*, about 16° long, making an angle of 65° or 70° with the other.

Sunderland, May 7

T. W. BACKHOUSE

"Pumice on the Cornish Coast"

STEAMER-CINDERS, similar to those referred to by Mr. Whitaker in NATURE for April 29 (p. 604), occur frequently on the Falmouth beaches; but as there seemed to me little probability of their being mistaken for pumice, I did not refer to the matter in my communication to your columns (April 15, p. 559).

Mr. Murray tells me that the pumice I found is felspathic, and that from its form and diminished buoyancy it had evidently been a long time in the water. The fragment was sent by him to Mr. Whitaker, who at once recognised its true character and its distinction from the steamer-cinders observed by him on the Suffolk coast, one of which he sent to Mr. Murray to satisfy him as to their very evident source.

H. B. GUPPY

95, Albert Street, Regent's Park, N.W., May 8

THE VELOCITY OF LIGHT

I.

[A reinvestigation of this important constant has recently been published by Prof. Newcomb. Before we state his methods and results we think it well to reproduce the following admirable historical notice with which his monograph commences.—ED.]

WHEN it became clearly understood that vision was not an immediate perception of objects by the eye, but was produced by the passage of an entity called light from the object to the eye, the question of the time which might possibly be required for this passage became one of interest to physical investigators. The first proposal for an experimental investigation of this question is due to Galileo.¹ He suggested that two observers, each holding a lantern, should be stationed at a distance apart, in sight of each other. Each should be supplied with a screen, by which he could, in a moment, cover or uncover his lantern. One observer should then uncover his lantern and the other uncover the other the moment he perceived the light from the first lantern. The interval which elapsed after the first uncovered his light, until he perceived the light of the second, would be the interval required for the light to go and come, plus the time required for the second observer to perceive the light and make the required movement. This experiment was tried by the Florentine Academy, and of course resulted in a

conclusion that the time required was insensible, since we now know that it was far below any interval that could have been detected by so rude a method.

It is, however, interesting to notice that, rude though this experiment was, the principle on which it was based is the same which underlies one of the most celebrated methods used in recent times for the attainment of the same object. Two very simple improvements which we might have imagined the Academicians to make in their experiments are these:—

Firstly, to dispense with the second observer, and in his place to erect a mirror, in which the first observer could see the image of his own lantern by reflection. The time required for the second observer to perceive the light and uncover his lantern would then have been eliminated from the problem. The interval sought would have been that between the moment at which the observer uncovered his lamp and the moment at which he perceived the reflection.

Secondly, to use the same screen with which he uncovered his own lamp, to cut off the returning ray from the distant mirror, and thus obviate the necessity of an uncertain estimate of the interval between his muscular effort in removing the screen and his perception of the return flash of light. If the image was perceived before he could cover his own eye with the screen removed from the lamp, it would show that the interval of passage was less than the time required to make a motion with the screen. This interval might have been reduced almost indefinitely by having both lines of sight as near together as possible.

Had these improvements been made, the Academicians would have had, in principle, Fizeau's method of measuring the velocity of light by the toothed wheel, a tooth being represented by the screens. To realise the principle more fully, the two lines of sight should have been rendered absolutely coincident by reflection through a telescope. It does not, however, appear that any effort to put the question to a severer test was made until the subject was approached from a different point of view. It was probably considered that the passage was absolutely instantaneous, or, at least, that the velocity was above all powers of measurement.

The subject was next approached from the astronomical side. In 1676 Roemer made his celebrated communication to the French Academy, claiming that observation of the eclipses of the first satellite of Jupiter did really prove that light required time to pass through the celestial spaces.¹ He found 11m. to be the time required for light to pass over a distance equal to the radius of the earth's orbit. Dominique Cassini, while admitting that the hypothesis of Roemer explained the observed inequality, contested its right to reception as an established theory, on the ground that the observed inequality might be a real one in the motion of the satellite itself.²

Continued observation showed that the time assigned by Roemer for the passage of light between the earth and sun, or "the light equation" as it is briefly called, was somewhat too great. In 1809 it was fixed by Delambre at $493^{\text{m}}.2\text{s.}$, from an immense number of observations of eclipses of Jupiter's satellites during the previous 150 years. This number has been received as a definitive result with a degree of confidence not at all warranted. In 1875, Glasenapp, then of Pulkowa, from a discussion of all available eclipses of Jupiter's first satellite between 1848 and 1870, showed that results between 496s. and 501s. could be obtained from different classes of these observations by different hypotheses.³

¹ *Paris Memoirs*, tome i. p. 212, and tome x. p. 575.

² *Ibid.* tome viii. p. 47. Poggendorff (*Geschichte der Physik*, p. 656) quotes Maraldi as also contesting Roemer's explanation on the ground that a similar inequality should be found depending on the position of Jupiter in his orbit. The ground here taken was quite correct, the only fallacy being the assumption that such an inequality did not exist.

³ This paper of Glasenapp's was published only in the Russian language as an inaugural dissertation, and in consequence has never become generally known.

⁴ Poggendorff, *Geschichte der Physik*, p. 402, where reference is made to the *Saggi* of the Florentine Academy.