which give him an advantage as an observer of phenomena both on sea and land, and the results of which are apparent in the volume before us. Mr. Clark's narrative relates to the years 1876-77, during which he resided for nearly two years in Buenos Ayres, Paraguay, and Uruguay. The two latter regions are yet sufficiently unknown to make any contribution from a competent observer who has visited them, welcome. The information, especially, which he gives us on Paraguay, is of much importance, and is a valuable addition to that obtained by Mr. Keith Johnston, in his visit two or three years ago. From a scientific point of view, perhaps the most valuable portion of Mr. Clarke's book are the numerous meteorological notes which he made both during his voyage out and his stay in South America. His knowledge of meteorology in its widest sense seems to us both extensive and accurate, and his observations on the instruments he used, on doldrums, tropical evaporation, and other such topics, are really interesting. But Mr. Clark knows something also of botany, as is evident from the frequent observations in this direction to be found throughout his volume. A whole chapter is devoted to the climate and meteorology of Buenos Ayres, important both from a scientific and practical point of view, as it is one of the great centres of emigration for South America. Many interesting sketches are given of the people and their mode of life in the various districts visited by Mr. Clark, and altogether his work is one of substantial value and real interest, and we trust it will find many readers.

Our Railways: Sketches Historical and Descriptive, with Practical Information as to Fares, &c., and a Chapter on Railway Reform. By Joseph Parsloe. (London: Kegan Paul and Co., 1878.)

MR. PARSLOE'S volume contains a large amount of very varied information on railways, their origin, their working; its object, he tells us, being to present a sketch of our railway system in its general details. The contents are so varied it would be difficult to give any idea of their nature without a lengthened notice. Mr. Parsloe goes back to the old days of stage-coaches, coming down to the origin of railways, then speaks of their construction, of navvies, working expenditure, signals, gauges, tickets, and a multitude of other topics all of much interest to the travelling public. The book is certainly both interesting and instructive.

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 ensure the appearance even of communications containing interesting and novel facts.]

The Formation of Mountains and the Secular Cooling of the Earth

THE letters of Mr. Wallace and Mr. Fisher in *NATURE*, vol. xix. pp. 121, 172, 244, 267, raise the question as to whether or not it is possible that the interior of the earth can be cooling more rapidly than the exterior. The following is an attempt to answer the query as to where the loss of temperature per unit time is greatest.

Sir W. Thomson (see Thomson and Tait, "Nat. Phil.," App. D) considers the cooling of "a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane." The solution given is—

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{kt}}} \frac{e^{-z^2}}{e} dz$$

where k denotes the conductivity of the solid, measured in terms of the thermal capacity of the unit of bulk;

V , half the difference of the two initial temperatures;

v_0 , their arithmetical mean;

t , the time;

x , the distance of any point from the middle plane;

v , the temperature of the point x at time t .

The above solution shows that for all values of the time when $x = 0$, $v = v_0$, so that the temperature at the medial plane is constant.

Then differentiating v with regard to the time we have—

$$-\frac{dv}{dt} = \frac{V}{2\sqrt{\pi k}} \frac{x}{t^{\frac{3}{2}}} e^{-\frac{x^2}{4kt}}$$

This expression is that required for the rate of cooling. We now wish to find where it is a maximum. Consider the function $x e^{-x^2}$; this is clearly a maximum when $\log x - x^2$ is a maximum, and by the ordinary rules this is a maximum when $\frac{1}{x} = 2x$, or when $x^2 = \frac{1}{2}$.

Hence it follows that $-\frac{dv}{dt}$ has its maximum value where $x^2 = 2kt$.

Now when the unit of length is a foot and of time a year, $k = 400$; hence $x = \sqrt{800t}$.

This formula shows that the seat of the maximum rate of cooling moves inwards as the time increases. If the time which has elapsed from the initial state be two hundred million years, or $t = 2 \times 10^8$, we have $x = 400,000$ feet, or a little less than eighty miles.

Sir W. Thomson shows, in his paper on the Secular Cooling of the Earth, that the solution of his ideal problem will be very nearly correct for the case of the earth, which is supposed to be a hot sphere cooling by radiation.

It follows, therefore, from the numerical result which is given above that the seat of the maximum rate of cooling must probably be something like 100 miles below the earth's surface.

It does not, of course, necessarily follow that the seat of the maximum rate of contraction of volume should be identical with that of the maximum rate of cooling; yet it seems probable that it would not be very far removed from it.

The Rev. O. Fisher very justly remarks that the more rapid contraction of the internal than the external strata would cause a wrinkling of the surface, although he does not admit that this can be the sole cause of geological distortion. The fact that the region of maximum rate of cooling is so near to the surface recalls the interesting series of experiments recently made by M. Favre (of which an account appeared in *NATURE*, vol. xix. p. 103), where all the phenomena of geological contortion were reproduced in a layer of clay placed on a stretched india-rubber membrane, which was afterwards allowed to contract. Does it not seem possible that Mr. Fisher may have under-estimated the contractibility of rock in cooling, and that this is the sole cause of geological contortion? G. H. DARWIN

Storm Warnings

A NEW YORK telegram occasionally announces that a cyclonic storm will probably reach the coast of Europe in a few days.

Such warnings are often of great value; but many storms are deflected in the Atlantic, while others—without having touched the American coast—come unannounced with destructive violence.

A floating buoy might be constructed to serve the purpose of a marine observatory, when placed in the usual track of storms at a sufficient distance from exposed coasts to be useful for warnings for ships in and near harbours.

The chief meteorological "elements" which are of essential significance in such a case are the height and changes of the barometer, and the varying force and direction of the wind.

If an experimental buoy were fixed by means of a slightly elastic cable about eighty miles off Valencia Observatory, and connected therewith by submarine telegraph wire, a slight modification of the aneroid lodged therein would enable the observer on shore to determine to about a tenth of an inch the height and changes in its readings.

A wind-vane in connection with a magnetic bar, and presenting a disk to the air-current, might be made the means of registering approximately the force and direction of the wind.

If the plan were successful, other meteorological facts might be determined by passing a current through mechanical indicators attached to each piece of apparatus.

Changes of temperature, electric conditions, and rainfall, might ultimately be brought within the scope of such a plan of telegraphic registration, and three or four floating observatories might be arranged at considerable distances apart.

The problem thus presented to the mechanician is the construction of apparatus such that in passing an electric current successively through indicators specially devised for each instrument, readings could be made and announced to all concerned.

By such means all coasts liable to be visited by progressive storms might have timely warning of danger.

The cost of such work would be very small in comparison with the saving of life and property concerned.

The Board of Trade might be induced to offer a substantial reward for the most efficient models of such floating stations.

The essential feature of this proposal is, that new instruments should be devised as entirely different in form from those in use as the aneroid is from the old barometer.

There is no reason to doubt that whenever instruments are devised in which the passage of an electric current can be made through the indicators, it will be as easy to take readings of meteorological instruments at the distance of a thousand miles as when in sight, and with sufficient accuracy for the purposes in view.

A HUTTON BURGESS

The Dissociation of Sal-Ammoniac—An Experiment

ALL chemists admit that when sal-ammoniac is volatilised the vapour consists, if not wholly, at least in great part, of hydrochloric acid and ammonia gases in the free state. But this fact, so far as I am aware, is very seldom, if ever, demonstrated experimentally by teachers. The following modification of Pébal's original experiment renders this proof very easy and available for lecture purposes:—

The stem of a long clay tobacco-pipe is passed loosely through a couple of perforated corks fitted into the two extremities of a piece of ordinary combustion tubing about a foot long. The tube contains in the middle a small lump of sal-ammoniac, and

near each end a strip of blue litmus paper. When the middle of the tube is heated the vapour of the sal-ammoniac surrounds a portion of the pipe-stem. If, now, a rapid stream of air or any other indifferent gas is sent through the pipe, it is found to be strongly charged with ammonia, so that it answers freely to all the usual tests. At the same time the litmus papers contained in the glass tube become red owing to the accumulation of hydrochloric acid in the residue. This experiment, of course, depends upon the diffusion of the lighter ammonia through the clay more rapidly than the hydrochloric acid also present.

WILLIAM A. TILDEN

The Sting of the Bee

IN "The Origin of Species," p. 242, fourth edition, Mr. Darwin says, "If we look at the sting of the bee as having originally existed in a remote progenitor as a boring and serrated instrument, like that in so many members of the same great order, and which has been modified, but not perfected, for its present purpose, with the poison, originally adapted for some purpose such as to produce galls, subsequently intensified, we can, perhaps, understand how it is that the use of the sting should so often cause the insect's own death; for if, on the whole, the power of stinging be useful to the social community, it will fulfil all the requirements of natural selection, though it may cause the death of some few members." In a lecture given as it happens, this day ten years ago, I ventured to suggest that

bees may have derived advantage, not in spite of the fatal condition annexed to the use of their sting, but from that condition itself, since "it may have proved expedient for a creature to be armed with a weapon capable of inspiring terror, yet so contrived, that its possessor should of necessity be peaceful towards its neighbours." It is very certain that many gentle-hearted human beings wage remorseless war upon wasps, who would never think of harming a bee or a bluebottle. On the other hand there are many mischievous persons ready enough to trifle with the feelings of a bluebottle, who keep at a respectful distance from a bee, simply because they know it possesses a certain power of revenge. In this way the sting is not, as your correspondent "R.A." is inclined to think, worse than useless to the individual bee, but an effective protection, albeit rather as a shield than a sword. What is needed for its efficacy is not so much intelligence in the bee as in those who would otherwise attack the bee, and though to the individual bee a single experience ending in its own death could be of no avail, yet the other animal, the wounded survivor in the fray, would have its understanding wonderfully quickened to the advantage of all bees it might meet in the future.

If, then, the bee is actually better off with its imperfect sting than it would be with one theoretically more perfect, it may be scarcely worth while to inquire whether a more effective weapon could or could not be developed on the principles of natural selection. But assuming that under given circumstances bees would derive advantage if the sterile workers had stings which they could use without sacrificing their own lives, the very statement of the hypothesis implies that a swarm, in which such workers were developed, would have an improved chance of surviving in the struggle for existence. Enemies would be more certainly vanquished; food would be more securely stored or in greater abundance; and thus the particular strain which had produced the improved variety would be more likely than others less favoured to be transmitted to future generations. The power of producing the better-armed warriors would be transmitted just as the power of producing the worse-armed warriors is transmitted, neither in the one case nor the other through the warriors themselves.

THOMAS R. R. STEBBING

Tunbridge Wells, February 1

Fossil Forests and Silicified Trunks

IN NATURE, vol. xix. p. 257, the discovery of fossil forests in the spring region of the Yellowstone River is referred to. I have visited the United States National Park, and its geysers, and observed exactly how silicified trunks *in situ* originate. All geologists suppose that this must have happened beneath water, and consequently Mr. Holmes supposes a constant alternation of land and sea throughout a long period of subsidence. My observations show the contrary, as silicified trunks originate only in air, never in water. The siliceous hot water of the geyser basins runs off periodically in another direction; if it comes to a forest, then all green leaves, all bark, and most of the branches fall off, but the trunks remain erect. Now the siliceous water rises by capillary attraction in the stem, but only on the outside of the trunk does the siliceous acid become solid by drying in the air; from the outside the silicification of the wood cells enters very slowly to the inner part; the trunks are mostly struck down by the wind before the inner part gets petrified, and then the inner part shows no ligneous structure, is only filled with foreign matter, or sometimes with other minerals, or it is hollow, for the inner wood decays. The white silicified wood is for a long time soft, less coherent than common wood, and if such trunks fall down into water, as I observed, they never get hard. Those white forests without leaves, bark, and branches, are not rare around the geysers. With my observations accord all characteristics of silicified trunks, *i.e.* such carbonaceous trunks excepted, that consist only of outfilled matter, stone kernels, for all real silicified trunks are barkless, leafless, branchless, often with inside hollow or partly filled, and always found along with common opal derived also from geysers.

Is silicification of trunks with well-preserved structure possible beneath water? No proof has yet been given. And further, would it be possible for stems, which are lighter than water, to remain *in situ* and erect by sinking under water? Scarcely—only if previously silicified and heavy.

Besides, most statements of travellers on fossil forests relate to the tropics. I saw several on my voyage round the world, which consisted only of stems lying together.

Leipzig-Eutritzsch, January 28

OTTO KUNTZE