

marking distinctly the commencement and simultaneousness of the storm.

The disturbance in the vertical force commenced about May 14, 1h. 10m. A.M. (7h. 8m. G. T.), and terminated about 3h. 30m. A.M. Between this time and 5h. 45m. P.M. the trace was smooth, but between 5h. 45m. and 6h. 45m. a sharp deflection took place in the opposite direction, the maximum force occurring at 6h. 11m. P.M. (May 15, oh. 9m. A.M. G. T.); the deflection or increase amounted to $\frac{1}{3}\frac{1}{2}$ of the vertical force.

The vertical force trace did not exhibit any of the tremulous motion noticed in the two other curves.

I may state that a description of the Madison Magnetic Observatory, together with the discussion of the first year's observations and results, is nearly ready in MS. for publication.

Coast Survey Office,
Washington, D.C.,
January 6

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Migration of Birds—A Suggestion

I BELIEVE that the migration of birds is one of the most interesting problems to the many who dabble in the narrative branch of natural history. It must also be of some interest to those who study biology in its more strictly scientific form. And yet there appears to be but little accurate information on the subject; nor, as far as I can discover, have any attempts been made to collect careful simultaneous observations over a wide extent of country, which would appear to be the readiest, if not the only means of collecting reliable data.

I am therefore induced to suggest that you should devote a small space in NATURE weekly, at certain seasons of the year, to the record of observations made in all parts of the world, from which correspondents can be induced to send them, of the passage of any migratory birds.

The record should, I think, be in a tabular form, giving the place, the date of appearance or disappearance of any migratory bird, the name of the observer, and some few remarks that may appear pertinent, such as the state of the weather, the direction of the wind, the character of the night, whether dark or moonlight, in the case of birds which are supposed to travel at that time. The necessity for recording other facts will no doubt be suggested by those more conversant with the subject. From these suggestions and the experience gained in a short time, detailed instructions might with advantage be framed for observers. The observers should be careful to state whether they were in a position to know with certainty that the date given was that on which the birds really arrived or left, or whether it was the first time their attention had been called to the circumstance.

I should imagine you would soon have a large number of careful observers interested in the subject not only in the country but abroad also. Among these some means should be taken to enlist captains of ships, or other seafaring men. There appear to be few, if any, recorded observations of birds of passage being seen on the wing at sea—though I may be wrong in this, for I do not profess to have gone carefully into the subject. Still, it is curious that one does not hear oftener of flights of such birds as the woodcock, millions of which come to and leave this country every year, being seen at sea, over long stretches of which they are believed to fly.

J. F. D. DONNELLY

The Formation of Mountains

THE letter of the Rev. O. Fisher in NATURE, vol. xix. p. 266, is conclusive as to the more rapid cooling of the interior than the outer crust of a heated globe under the conditions of our earth, and I thank him for clearing up the point. But the question remains, whether the amount of contraction of the interior, and consequent crumpling of the crust, thereby produced in a definite time, is sufficient to account for the elevation of our mountains. It is necessary to take account of the following facts:—

1. That the greater part of the elevation of all our chief mountain ranges occurred during the eocene and miocene periods.

2. The warmer climates of those periods (certainly due to external and not to internal heat) would have tended to diminish the rate of cooling and consequent contraction of the earth.

3. The Rev. O. Fisher appears to have demonstrated that, even allowing for the total shrinkage due to the earth's cooling for the last hundred million years (from a mean temperature of 7,000° F.,

as calculated by Sir William Thomson), the amount of elevation thereby caused would be *very much* less than that of existing lands and mountains. But we know that these have been lowered by denudation, and again elevated many times over during that period.

The inadequacy of the alleged cause for the production of our existing mountains would therefore seem to be conclusively established.

ALFRED R. WALLACE

MR. WALLACE's letters raise three separate difficulties: How can the interior of the earth be cooling faster than its surface? How can this surface be cooling so slowly (or not cooling at all) as it is assumed to be? and, How can the different rates of cooling of interior and surface account for the corrugations observed in the surface?

As to the first question, Mr. Fisher's explanations should remove the difficulty.

As to the second question. So long as the amount of heat radiated from the surface into space equals the amount received by the surface from the sun and from the interior together, its temperature cannot fall. The rate of radiation depends on the difference between the temperatures of the surface and space. The supply of heat from the interior depends on the difference between the temperatures of the interior and the surface. Since the temperature of the interior is falling, the supply of heat from interior to surface must diminish. Hence the temperature of the surface must also fall. We cannot doubt that it does, though at present imperceptibly. The extreme slowness is due partly to the fact that the difference of temperature between interior and surface must many times exceed that between the surface and space, even if the latter be at absolute zero; partly to the low transmissive powers both of earth and atmosphere; partly to the immense mass of the earth, from which immense quantities of heat must pass away to lower the temperature of the interior but a trifle.

As to the third question, whether this small fall in temperature will account for observed corrugations, I will offer no opinion. The size of the earth must be allowed for. A diminution of a foot in radius would diminish the surface by more than nineteen square miles.

I do not understand why corrugations should be confined to the surface. I should expect them to extend as deep as the solid crust. They are possible in any stratum which is contracting more slowly than interior strata.

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Bees' Stings

CAN any of your readers inform me why the working honey-bee has such an imperfect weapon of defence as its sting manifestly is? For purposes of self-defence it is apparently worse than useless, for in nearly every case, almost without exception, the bee lays down its life with the sting. The possession of a sting therefore only leads to its own destruction instead of to its preservation so far as the individual bee is concerned. No doubt the hive generally gains an advantage from all its active members having stings and so indirectly do individual bees from the fact that the welfare of the hive, speaking generally, means the welfare of the individuals that compose it. Directly, however, the possession of a sting can only be a disadvantage to the individual bee, unless there are certain enemies from which bees after inflicting a wound can withdraw their stings and escape with life. This so far as my observations go appears to be very unlikely, and therefore no bee can have any knowledge from experience of what a weapon of offence he possesses for he has never used it, nor can he have knowledge from experience of the consequence of using it. All smaller pests bees attack with their jaws. Is it possible, then, that they are so intelligent as to be well aware of the power for mischief to themselves as well as to others which they carry about with them, and that it is only when they altogether lose control over themselves, either through severe pain or through terror lest their queen should be injured that they sign their own death-warrants on our hands and faces? In the death of a few worker-bees a hive suffers very little loss, perhaps none at all; yet it may have gained much in the shape of security from molestation. Are bees so intelligent as to know this fact and communicate it from one to another, or can their conduct be explained on the lower ground of instinct?

It seems that an interesting point is here raised which perhaps