

sense and reasoning power which I believe them to possess, and also to the assumption that in the case supposed they would recollect merely the odours, not the objects the presence of which these odours had indicated. I imagine that animals know, just as well as we do, that some sights, sounds, and smells are caused by permanent, others by evanescent or changeable causes. The smell or sound of a flock of sheep would indicate to a dog the presence of an actual flock of sheep, just as surely as the sight of them would do, and he would no more lose his way because those sheep were not in the same place the next day or the next week, than he would had he travelled the road on foot with his eyes open. The smell of a wood, of a farmyard, of a ditch, a village, or a blacksmith's shop, with the more or less characteristic sounds accompanying these, would tell the dog that corresponding objects were there just as surely as the sight of them would do. On his return he would recognise the objects, not the smells and sounds only, and he would be no more puzzled by the absence of certain moveable objects he had recognised by smell than he would be had he seen them. I quite believe that mistakes would often be made owing to the discontinuousness of sufficiently characteristic odours; but the process of "trial and error," suggested by F.R.S., would be constantly used, and this is in accordance with the length of time usually taken in these journeys, often very much longer than would be required for a return by the shortest route and at moderate speed.

A friend has communicated to me a most remarkable fact, of a different character from any which have been referred to during the course of this discussion; and as I have it at first hand and took the exact particulars down as narrated to me, I think it will be of value. Many years ago, my friend lost a favourite little dog. He was then living in Long Acre. Three months after, he removed to a house in another street about half a mile off, a place he had not contemplated going to or even seen before the loss of the dog. Two months after this (five months after the dog was lost) a scratching was one day heard at the door, and on opening it the lost dog rushed in, having found out its master in the new house. My friend was so astonished that he went next day to Long Acre to an acquaintance who lived nearly opposite the old house (then empty) and told him his little dog had come back. "Oh," said this person, "I saw the dog myself yesterday. He scratched at your door, barked a good deal, then went to the middle of the street, turned round several times, and started off towards where you now live." My friend cannot tell, unfortunately, what time elapsed between the dog's leaving the old and arriving at the new house. If every movement of this dog could have been watched from one door to the other, much might have been learnt. Could it have obtained information from other dogs (and that dogs can communicate information is well shown by Mr. A. P. Smith's anecdote in your issue of three weeks back)? Could the odour of persons and furniture linger two months in the streets? These are almost the only conceivable sources of information, for the most thorough-going advocates for a "sense of direction" will hardly maintain that it could enable a dog to go straight to its master, wherever he might happen to be.

Not to trespass further on your space, I would venture to hope that some persons, having means and leisure, would experiment on this subject in the same careful and thorough way that Mr. Spalding experimented on his fowls. The animals' previous history must be known and recorded; a sufficient number of experiments, at various distances and under different conditions, must be made, and a person of intelligence and activity must keep the animal in sight, and note down its every action till it arrives home. If this is done I feel sure that a satisfactory theory will soon be arrived at, and much, if not all the mystery that now attaches to this class of facts be removed.

ALFRED R. WALLACE

The Origin of Volcanic Products

I HAVE not yet had the advantage of seeing Mr. Mallet's translation of Palmieri's late work on Vesuvius, but have read with interest Mr. Forbes's review thereof and Mr. Mallet's reply in NATURE of Feb. 6 and March 20. I have no desire to enter into a controversy, but as I have for the past fifteen years taught and defended a theory of the origin of volcanic products identical with that now maintained by Mr. Mallet, I may be permitted to say a few words. That the source of all such matters was to be found not in the earth's nucleus but in sedimentary strata, was taught by Referstein in his *Naturgeschichte des Erdkörpers*, in

1834; and again, doubtless independently, by Sir J. F. W. Herschel in 1837; while, for my own part, I was led to the same conclusion before I became aware of the views of either of my predecessors, solely from a consideration of the varying composition of plutonic rocks and of the stony and vaporous products of volcanic action. To the views of Herschel I first called attention in the *Canadian Journal* for March 1858, and again in the *Quar. Geol. Journ.* for November 1859, pp. 488-496, § vii.).

In the first of these I have said: "If we admit that all igneous rocks, ancient plutonic masses, as well as modern lavas, have their origin in the liquefaction of sedimentary strata, we can at once explain the diversities in their composition. We can also understand why the products of volcanoes in different regions are so unlike, and why the lavas of the same volcano vary at different periods. We find an explanation of the water and carbonic acid, which are such constant accompaniments of volcanic action, as well as the hydrochloric acid, sulphuretted hydrogen, &c." The nature of the reactions between siliceous, calcareous, and aluminous strata, holding carbonaceous matter, gypsum, sea-salt, &c., was then discussed, and the products of their transformations under the influence of water at an elevated temperature considered. In both of these papers referred to, the inadequacy of the views of Phillips, Durocher, and Bunsen, to explain the origin of these various products, was maintained.

In the *Geological Magazine* for June 1869, I returned to this subject in a paper on "The Probable Seat of Volcanic Action," where, after repeating and enforcing the above views, I said: "Two things become apparent from a study of the chemical nature of rocks; first, that their composition presents such variations as are irreconcilable with the simple origin generally assigned to them; and second, that it is similar to that of the sedimentary rocks whose history and origin it is, in most cases, not difficult to trace." In what follows I endeavour to show in the latter the source of such "eruptive rocks as peridotite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen."

Mr. Mallet has, however, made a very important advance in this theory of volcanic action by pointing out a source of heat independent of the cooling nucleus. Referstein had supposed heat to be generated by chemical action in the sediments, and his view has lately been brought forward, in a modified form, by Leconte; but this I have always rejected as untenable. The chemical actions supposed to be involved in the processes would consume rather than generate heat. I have hitherto followed Herschel and Babbage in regarding the heat as directly derived by conduction from an incandescent nucleus, but Mr. Mallet has now shown that the work expended in the crushing of the strata which takes place in certain regions of the globe where the contraction which attends the slow refrigeration or the globe is displayed in corrugations of the crust, is more than adequate to explain volcanic heat. To this it must be added that, inasmuch as the crushing process takes place in strata which, from their depth, are already at an elevated temperature, the heat developed by the mechanical process comes in to supplement that derived by conduction from the igneous centre. Vose had already, in a general manner, pointed out the same thing, suggesting in terms which are, it is true, wanting in scientific precision, the notion that the mechanical force at work in the crushing of the strata was the source of heat. This, however, in no way detracts from the great merit of Mr. Mallet, who may rightly claim "to have been the first to apply weight, measure, and number to volcanic theory," and we await with great interest the publication of his quantitative results. Apart from his thermo-dynamic theory, however, his views of volcanic action are apparently identical with those of Referstein and Herschel, to which I have for many years been endeavouring to give form and consistency. I may here call attention to a paper, "On some Points of Dynamical Geology," published in the *American Journal of Science* for this month (April 1873), in which I have already alluded to the foregoing questions, and to the endeavours which I have for fifteen years been making "to reconstruct the theory of the earth on the basis of a solid nucleus." I have there rehearsed the views which I have all this time maintained as to the causes which determine the process of corrugation of the earth's crust, the accumulation of sediments, and the development of volcanic activity in certain regions of the earth; thus giving a theory of the geological and geographical distribution of past and present volcanoes.

T. STERRY HUNT

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