an unknown cause" ("Occasional Papers," p. 4), and worked out the subject thoroughly.

In the *Comptes Rendus* for Oct. 18, 1841, a portion of a letter from Agassiz to Humboldt was published. Here he lays claim to the discovery without mentioning the name of Forbes. He speaks of it as "le fait le plus nouveau que *j'ai* remarqué." Forbes felt deeply annoyed at this conduct of his friend, but contented himself with publishing his own discovery. A rupture between the two friends now commenced. About this time M. Guyot recollected that he had described this appearance in 1838 to the Geological Society of France, at Porrentruy ("Agassiz Etudes," p. 207). Several people had seen the same thing previously. Among others, Sir David Brewster writes as follows:—"The Mer de Glace is like the waves of the sea, as if they had been fixed by sudden congelation; when the ice is most perfect, which is on the sides of the deep crevices, the colour is a fine blue. There is an appearance of a vertical stratification in the icy masses stretching in the direction of the valley in which the glacier lies. . . . The surface of the glacier exhibits also the appearance of veins exactly like blocks [?] of stone" (*Journal*, 1814). In 1820, M. Zummstein saw it ("Bibliothèque Universelle," 1843). Col. Sabine and M. Elie de Beaumont had also seen it ("Travels in the Alps," p. 29). But though seen it had not been studied, nor did any printed description of it exist. M. Guyot did not even print an abstract of his communication. It remained an isolated, unprinted, forgotten fact until Forbes appeared upon the scene. Professor Tyndall has most justly said that neither Forbes nor Agassiz knew of it in 1841 ("Forms of Water," p. 187). Yet though, as has just been proved, Forbes pointed it out to Agassiz in 1841, the latter tried to show that he had known of Guyot's observation (letter from Agassiz to Forbes, "Life of Forbes," Appendix B), and endeavoured to give the credit to Guyot rather than to Forbes (his own claims having been now disproved). If it be true that he knew what Guyot had done, then (1) why did he not mention it to Forbes and Heath, both of whom affirm (in contradiction to the statements of Agassiz) that Guyot's name was not mentioned? (2) Why did he not perceive the importance of the structure? (3) Why did he say that it was superficial? (4) Lastly, how could he reconcile it with his conscience to describe it to Humboldt as "le fait *le plus nouveau* que *j'ai* remarqué?"

The facts show (1) that Forbes was seriously wronged by the conduct of Agassiz; (2), that he discovered independently the veined structure; (3) that he was the first to study the subject and give it its true place in reference to glacier theories. I have limited myself to the accusation contained in the letter of Mr. Alex. Agassiz. Whether he is correct in his appreciation of the estimate put upon Forbes' labours, in Dr. Tyndall's last popular work, I need not at present discuss. I know so well to what conclusion a comparison of that book with the writings of Forbes and other workers on glacier theories would lead, that I leave it confidently to the judgment of those "fair-minded investigators" of whom Mr. Alex. Agassiz speaks.

GEORGE FORBES

P.S.—Mr. Heath's testimony, to which I have referred, is given in the following extract from a letter dated Trinity College, Cambridge, Feb. 25, 1842:—"I will witness—1st, that he (Agassiz) knew nothing about it; 2nd, when he did see it he said it was superficial *sand*; 3rd, that he was the last to believe that it went to any depth. I think your account very true, and not claiming one jot more than fully belongs to you."

Cambridge, May 20 G. F.

Perception and Instinct in the Lower Animals

THE suggestion made by me in your issue of February 20, that animals which had been deprived of the use of their eyes during a journey might retrace their way by means of smell, had the effect of letting loose a flood of illustration, fact, and argument bearing more or less directly on the question; and as the stream now seems to have run nearly dry, I ask permission briefly to review the evidence adduced, so far as it affects the particular issue I brought forward. Several of the writers argue as if I had maintained that in all cases dogs, &c., find their way, wholly or mainly, by smell; whereas I strictly limited it to the case in which their other senses could not be used. The cases of this kind adduced by your correspondents are but few. The first, and perhaps the most curious, is that of Mr. Darwin's horse; but, unfortunately, the whole of the facts are not known, As Mr. Darwin himself pointed out, the horse may have lived in the Isle of Wight, and been accustomed to go home along that very road. I would suggest also that the country might resemble some tract in the neighbourhood of his own home; or that the horse, having been brought from home by a route and to a distance of which it had no means of judging, thought its master was riding home on the occasion in question, and therefore objected to turning back. Anyhow, the case is too imperfect to be of much value as evidence in so difficult a matter. "J. T." (March 26) quotes the case of the hound sent "from Newbridge, county Dublin, to Moynalty, county Meath," thence long afterwards to Dublin, where it broke loose, and the same morning made its way back to its old kennel at Newbridge. I can find no "Newbridge, county Dublin," although there is a Newbridge, county Kildare, which is 26 miles from Dublin, on a pretty direct high road. That the dog never attempted to return during its "long stay" at Moynalty seems to show that some special facilities existed for the return from Newbridge. What they may have been we cannot guess at in the total absence of information as to the antecedents of the dog, the route by which he returned, and the manner in which he conducted himself on first escaping in Dublin.

The next case, of the two dogs returning from Liverpool to near Derby, is vague, and also without necessary details. It happened 50 years ago, and the only evidence offered as to the mode of the dogs' return is that "*it is said* they were seen swimming the Mersey." "N. Y.'s" case (April 24) of the dog who "did not make haste back," and therefore could not have returned by smell, is also most inconclusive. The distance was only 20 miles, and we know nothing of the route the dog followed, or the time it took. How do we know the dog did not wait the three weeks till it saw someone it knew living at or near its former house, and followed that person? This appears to me to be an exceedingly probable way of accounting for many of these returns where the distance is not very great. This brings me to the case of Mr. Geo. R. Jebb, who seems to have gone to the trouble of making an experiment which, with a little more trouble, might have been very complete and satisfactory. The dog was taken by rail very circuitously from Chester to a place 10 miles from Chester. It "hung about the station for about an hour and a half," and in three hours more arrived at its home. But we are still left totally in the dark, both as to the route it took or the process by which it decided on that route. What is required in such experiments is, that a person not known to the dog should be ready to watch and follow it (on horseback), noting carefully on the spot its every action. We should then perhaps know why it "hung about the station" an hour and a half before commencing its journey home, and afterwards, whether it showed any hesitation as to its route, and whether it followed the road or went straight across country. A few experiments carefully made in this way, at distances varying from 10 to 30 miles, and with a thorough knowledge in each case of the animal's antecedents, would, I venture to say, throw more light on this interesting question than all the facts that have been yet recorded. The only experiment of this kind I have met with is in the work of Houzeau ("Etudes sur les Facultés Mentales des Animaux"), and it is so curious that I give the passage literally. He says (vol. i. p. 156): "I have succeeded in making young dogs of five or six months lose themselves on first going out with me. They would begin by seeking for my trace by smell; but not succeeding in this, they would decide to return home. If there was a path, they followed the route by which they had come. If it was an untrodden virgin country, they shortened the circuits they had made in coming, but did not altogether depart from them. One would say that memory furnished a certain number of points which divided the route, and they went towards these by memory of directions. Thus inscribing chords to the curve by which they had come, they returned to the house." M. Houzeau's general conclusion from a considerable body of observations made with this point in view is, that animals find their way by exactly the same means as man does under similar circumstances, that is, by the use of all their faculties in observation of locality, but especially by a memory of directions and by a ready recognition of places once visited, which serve as guide-posts when they are again met with. This seems to me a very sound theory, and quite in accordance with all that is known of the manner in which savages find their way.

The more general objections to my little theory which are made in your leading article appear to depend on the denial, to such animals as dogs and horses, of that amount of common 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 thoroughgoing 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 irreconcileable 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 peridolite, 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 of 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

Institute of Technology, Boston, Mass., April 25