preliminary paper, and that Mr. Lockyer intended to follow up and did follow up his suggestion with the requisite observations. But at that time neither Mr. Lockyer nor myself knew to what extent the spectroscope would throw light upon these red flames. What service I did was in stating my opinion that those red flames were probably gases, while the suggestion as to how to detect them was due to Mr. Lockyer. Had the red flames proved to be solid particles, the spectroscope would have afforded only meagre negative evidence of their existence.

Our ignorance on this point suggested to us the propriety of a query. It was only in this respect that the query was ambiguous, that is to say there was no doubt that the method suggested by Mr. Lockyer would throw a great deal of light upon the red flames if they proved to be gaseous, but the only doubt in our minds was whether or not they were gaseous.

4. I have tried as well as I can in justice to both of us, to remember and reproduce what took place in the conversation in 1866 between Mr. Lockyer and myself, and to show the reasons for the form in which Mr. Lockyer's suggestion was put. Probably Mr. Huggins announced it afterwards in a more complete form, and probably it has since been announced in a yet more clear and complete form, if this be possible. For we know that as a rule discoverers and inventors have no great power of expression, and if the prize is to be given for the clearest possible utterance of a truth, it will be very seldom won by the discoverer, but will very frequently be obtained by the popular writer. Nevertheless I fail to see that Mr. Lockyer's original query was at all ambiguous in the sense that Mr. Proctor suggests. How, I should like to know (adopting the words of the query), could the spectroscope afford us evidence of the existence of the "red flames at other times than those of a total eclipse, unless by dispersing the reflected light."

5. Mr. Proctor asks, To what end are we to inquire whether Mr. Lockyer would or would not have detected the lines without the information derived from the eclipse observations? As far as I can understand the question I quite agree with Mr. Proctor. I do not think the inquiry ought ever to have been made. When, however, it was suggested that Mr. Lockyer derived aid from the Indian observations it was surely allowable for him to deny the imputation.

6. I have sometimes thought that discussions too frequently arise from the attempt to compare together the labours of different men, when all that is necessary is a statement of facts and dates. It is a matter of fact that the Indian observers at a particular date and under particular circumstances, made certain observations and derived certain results. It is likewise a matter of fact that Mr. Lockyer, at a date slightly later and under certain other circumstances, made observations of another kind, from which he also derived certain results. Different minds are differently constituted; one will think more of the Indian observations, another of those of Mr. Lockyer. But why should the two be compared together, unless some good object is to be gained by the comparison?

7. Now I cannot see why Mr. Proctor in his letter should have made the comparison at all, but since he has done so, I must be allowed to object to his method of making it. "Surely," he says, "on this matter we must assign Mr. Lockyer only the credit arising from the fact that, possessing an instrument which made the work child's play, he saw the lines by the method described by Huggins nine months before in detail, and depending on a principle which Huggins had stated fully two years and nine months before." I have already endeavoured to dispose of the latter part of the statement, and indeed Mr. Huggins has disposed of it himself; but with regard to the first part of it, I conceive that the possession of an instrument which made the work child's play, forms one element of the credit due to Mr. Lockyer. At first his instrument was defective and unable to do what he wished, but his conviction that something was to be made out of the sun, if examined by a powerful spectroscope, was so strong, that after much delay on the part of the optician employed, and sundry other discouragements, he at last procured for himself an instrument with which he saw the red flames at the very first trial.

8. I should not have alluded to the concluding paragraphs of Mr. Proctor's letter were it not written by one who holds a high official position in the Royal Astronomical Society. To my mind it is most deplorable that the secretary of this society should conceive himself at liberty to make in a public journal unsupported statements reflecting discreditably upon a distinguished member of that society. It is bad enough when two

private men of science abuse each other, but it is not to be tolerated when a high official of a society of standing descends into the arena. If he insists upon doing so it is surely not unreasonable to request that he will in the first place divest himself of his robes of office.

For my own part I have thought that Mr. Lockyer has attached only too much importance to the little help I gave him when we conversed together in 1866.

BALFOUR STEWART

Mr. Mallet's Theory of Volcanic Energy

WILL you allow me to make a few remarks upon Mr. David Forbes's critique, which has but just now met my eye, though published in NATURE of February 6 last, upon my translation of Prof. Palmieri's "Incendio Vesuviano," and more especially upon that gentleman's animadversions upon my views as to "the true nature and origin of volcanic heat and energy," a brief and incomplete account of which I have given in the "introductory sketch" prefaced by me to that translation.

Mr. Forbes commences (p. 260) with an important error as to a matter of fact, by referring to "Mr. Mallet's Dynamical Theory of Volcanic Energy," as published in the Proceedings of the Royal Society for 1872.

My paper as above was read in abstract only in June last to the Royal Society, and being reserved for probable publication in the Phil. Trans., nothing but the most meagre and incomplete abstract has appeared in the Proceedings of the Royal Society.

I have given a somewhat fuller, but still most incomplete, account of it in my "introductory sketch" above referred to. My paper, and the full statement of my views, with the proofs which give them validity, have thus as yet never been published at all, nor even verbally stated publicly, the paper being still—after eight months, I regret to say—in the hands of the referees.

When published—as the paper in some form will no doubt be—Mr. Forbes will find that his objections—so far as I can gather anything tangible from their statement in NATURE—have been anticipated, and I believe completely answered, in my paper, along with others, better founded, because based on fact, which, as it appears to me, Mr. Forbes's objections are not.

Mr. Forbes uses "theory" and "hypothesis" as though quite the same (p. 260). My hypothesis is simply undeniable, being no more than that our globe is a terr-aqueous planet subject to secular refrigeration; and upon that hypothesis I have built up my theory, that the evolution of volcanic heat is a necessary result, upon acknowledged physical laws of such refrigeration; and that from such heat so evolved—as the *primum mobile*—come in train all the other recognised volcanic phenomena: chemical, as in the fusion and combination of the constituents of rocks into definite and indefinite compounds, decomposition of many solid, liquid, and gaseous bodies; mechanical in the elevation and throwing out at volcanic vents of all or any of these: water, air, and the chemical elements known to exist in the crust of our earth being the only conditions needed, in addition to heat, to account for all that we know as to volcanic phenomena, as now active in our planet.

I claim to have been the first to apply "measure, number, and weight" to volcanic theory; and when men of science generally shall have had access to my statements, I trust it will be admitted that I have not shrunk from rigidly testing by calculation the adequacy of the source I assign for volcanic heat, nor of the mechanism through which its subsequent effects are produced, as seen in volcanic phenomena. I am at the disadvantage that I cannot expect to cumber your pages by reference to those proofs—they are the less necessary here, however—inasmuch as my reviewer, though affecting to discredit the adequacy of the origin I assign for the "quantity of heat requisite to melt up such vast volumes of rock matter as are known to proceed from volcanoes," does not present a single argument against it, and, in fact, appears unwillingly to admit it. He however proceeds to object generally to my views by the following statements. They are so vague and inconclusive that, as far as possible, I shall endeavour to condense them in his own words:—"Admitting that the conversion of the mechanical force into heat is sufficient to effect the melting part of the operation, there remains the greater difficulty of explaining the chemical and mineralogical features which characterise volcanic phenomena. For although mechanical force is admitted to be convertible into its equivalent heat, which in turn may 'cause' chemical action, still no such forces, alone or in combination, can transmute one

chemical element into another, or give rise to products having at all times a definite chemical and mineralogical constitution, out of the incongruous materials likely to be met with" in the crust of our globe.

"Our knowledge of the mineral characters of that crust does not admit the supposition that the substance of the rocks occurring in so many different parts of the world could in all or in any but solitary instances when fused by mere heat afford products identical with those of known volcanoes.

"Volcanic products, no matter from what part of the world derived, are *identical in chemical or mineralogical constitution*; a result which indicates that they must be derived from some one common source, and not mere local accidents, as Mr. Mallet's *hypothesis* would require us to assume.

"For these and other reasons [none of which are suggested] it does not seem probable that this *hypothesis* will receive the adherence of either chemist, mineralogist, or geologist." The italics are mine.

Now I cannot help remarking that Mr. Forbes shows his very slender acquaintance with the vast subject he so jejune disposes of, by the very language he employs; *force* cannot be converted into heat, equivalent or otherwise; *work*, i.e. pressure producing motion, is transformable into heat, and every other known form of energy. But passing this as well as the reiterated confusion between hypothesis and theory, I ask, is it a fact as stated, that volcanic products whencesoever derived, in time and in space over the whole globe, are *identical in chemical or mineralogical constitution*. I affirm unhesitatingly that it is not a fact, and I appeal for confirmation of my denial to every authoritative work on the subject, and to every well-informed mineralogical chemist.

Are the ancient basalts and trachytes identical with the modern ones, or with each other in different localities? Are the modern lavas of extinct or active volcanoes all over the world, or even at closely adjacent places, all identical? Are the highly liquid lavas of Otaheite, producing "Pele's hair," the same with the terrane lavas of Etna? Are the trachytes of Auvergne—the *Pierre de Volvie* so well known to French engineers, the same as those of Pozzuoli and of the Rhine? Are the products of the same volcanic basin ever identical—say the pumices and obsidians of Pouza, Ischia, and Lipari—with the innumerable varieties of lava of the same localities, and of the phlegrean fields and of Sicily? Are even the lavas of the same cone and during the same eruption always identical? Palmieri in the very report Mr. Forbes reviews, says (p. 105), "The *qualitative* analysis of the (Vesuvian) lavas always presents the same elements," with certain exceptions; "but with respect to *quantitative* analysis, two specimens of the same lava have their constituents in different proportion." Nay more, are even the crystallised minerals of definite chemical constitution, as segregated from the lava pastes or glasses, *identical* all over the world, or even for any one cone, or often for any one eruption? No, there is just that general similarity in the constitution of volcanic products that we should expect to result from the heating more or less, or the fusion together of the "incongruous" beds, siliceous, aluminous, magnesian, calcareous, mixed more or less with metallic oxides, or other compounds, and often with carbon, boron, and other elements all variously superposed or mixed, which constitute the known crust of our globe, along with most probably the materials of crystalline or other rocks below, the nature of which we can but guess at. There is just the same general similarity that is seen in the slags of all metallurgical furnaces, which fuse very similar materials to the volcano, but with a preponderance of some metal also. Are the slags of the same iron blast furnace *identical* even for any two tappings, though the working and charge is apparently the same? No, and for very obvious reasons. Are even the crystallised minerals that segregate from these slags, from the same furnace assumed, worked in the same way, always identical?

If Mr. Forbes will study Von Waltershausen's early and able work on Lavas, one scarcely known in this country, Zirkel's "Petrographie," Senft's "Kristallmischen Felsgemengtheile," and above all Blum's "Handbuch der Lithologie," amongst many others he will soon see how very baseless is the supposed fact on which he so confidently relies for his objection to my origin of volcanic heat.

But let us for a moment assume that it were a fact that all the volcanic products all over the world, and for all geologic periods, were "identical in chemical and mineralogical constitution," how would that form any relevant objections to the thermo-

dynamic origination which I have assigned to volcanic heat. Or how would it help the old notion of a very thin crust and a universal molten ocean beneath, or the exploded one, that chemical action produces the heat, and the heat produces the chemical action.

Whatever be the origin assigned to the heat, it is within or below the heterogeneous solid crust of 10 to 40 miles thick, which we are acquainted with. More or less of that heterogeneous crust must be melted up, along with the material assumed everywhere the same of the molten ocean coming up through it from beneath.

It is proveable on merely hydrostatic grounds that the mass of such a molten ocean, if of materials such as are found in our earth's crust, could not be everywhere and in all latitudes identical in construction; but were it so, it must get contaminated and modified in passing through and melting the "incongruous" ducts of the crust bringing it to the surface.

On the other hand absolute identity everywhere in the volcanic products, did it exist, could prove nothing in favour of a universal ocean of ready-made lava as the source or origin of volcanic heat; but only that whatever might be the origin of that heat it was so circumstanced as to act on materials everywhere the same, and that these reached our surface without any action or mixture with the "incongruous" crust through which the molten matter came. There is just the same room for "local accident" in one case as in the other. A reviewer should at least be sure of his facts before he brings them into hostile array.

But again, what are the grounds for the assumption that according to my views, volcanic products of whatever sort are "the result of fusion by mere heat." I point out a true cosmical and thermo-dynamic origin for the heat itself, but under the actual conditions of volcanic foci and ducts, chemical actions must take place, as a consequence of the heat, and together vary the results of the reactions of all the materials present in fusibility, in aggregation, in their solid, liquid, or gaseous states, in their molecular conditions and in their chemical and crystallogenic combinations. This objection as little applies to anything I have written, as the irrelevant suggestion, that the transformation of energy "cannot transmute one chemical element into another."

I believe this disposes of whatever is tangible or worth remark in Mr. Forbes' strictures.

He ends, however, in a manner unusual I should think for an unbiased reviewer, by uttering a sweeping prophecy, "for the reasons given [and above repeated], and others not brought forward, it is not probable that this hypothesis [i.e. my theory of volcanic energy] will receive the adherence of either chemist, mineralogist, or geologist." Prophecies, especially as to matters of science, are dangerous things, and the prophets would do well to rule by Swift's advice, as to utterances of another class, and never record one that can be falsified within a short time. It so happens that I am able already to explode Mr. Forbes's. Writing to me under date of Dec. 24 last (1872), Dr. Haughton, Professor of Geology of the University of Dublin, says—"I gave two lectures during the last term (Michaelmas) which were attended by many of the fellows, in which I developed your theory of volcanoes and gave you full credit. The whole system hangs well together and must make progress." Prof. Dana (in noticing this same book which Mr. Forbes so uncompromisingly condemns), as editor of the *American Journal of Science*, for the month of February, 1873, at p. 151, says: "His paper is the most important contribution to this department of geological dynamics that has ever been brought forward, and the work above-mentioned [i.e. Trans. of Palmieri] is by his share of it more than doubled in value." In a letter to me, Feb. 11, 1873, Prof. Dana says: "Your views throw great light on a hitherto mysterious department of geological science, and I have no doubt that they will speedily gain general acceptance."

I could add like expressions from some others. Now both Haughton and Dana are at once chemists, mineralogists, and geologists. They are both, also, sound physicists and mathematicians. So this prophecy is already falsified, and I am consoled with the notion that my theory of the nature and origin of volcanic energy may survive the unreserved condemnation to which Mr. Forbes has sentenced it, on the grounds that I have above examined, and without his ever having had access to the paper itself criticised by him. I might refute, too, some criticism on the book itself, which I cannot avoid calling unfair, and also not founded on fact. It is not true, for example, that the first forty-six pages of my introductory sketch "are but an abstract of my

previously published investigations into the theory of earthquakes" (p. 260). The reviewer may not have been able or not taken the trouble to distinguish the old from the new; but as a fact, the greater part of those forty-six pages is of matter never before published.

So also it is scarcely candid to object that "no reference is found to any of the Continental men of science who have done so much for terrestrial vulcanicity," which is contrary to the fact, for I have referred by name or by their labours to the few who have in any way advanced our knowledge as to the nature and origin of *volcanic heat*, without noticing that within that scope only was I by space obliged to confine myself, as stated in pp. 48, 49, 54, 76, &c.; the phenomena occurring at volcanic vents, which have chiefly engaged the attention of Continental and all other volcanic authors being avowedly outside my limits, and, I might add, but too often of secondary importance.

The nomenclature generally of my "Translation of Palmieri" is said to be objectionable, because such terms as sulphide of potass and terrochloride of ammonia are encountered. I have looked through the pages since without being able to discover these dreadful terms. However I am ready to take the reviewer's word that such a slip in proof correcting may be found in some place, and I humbly bow to such microscopic, profound, and valuable criticism, though, as stated, the conclusion is a good deal wider than its premises.

ROBERT MALLETT

Enmore, The Grove, S.W., March 5

Effect of Resistance in modifying Spectra

IN a review of M. Guillemin's work "The Forces of Nature" which appeared in last week's *Athenaeum*, the following reference, by M. Guillemin, to the experiments of M. Mitscherlich is quoted: "Suivant ce physicien il arrive que la presence de certaines substances dans une flamme a pour effet d'empêcher de se produire les spectres des autres substances, d'entendre leurs raies principales." The English editor adds that the effect "may probably be explained by the observations of Frankland and Lockyer."

In relation to this subject of the extinction of the bands of one metal by another, you will perhaps permit me to quote a paragraph from one of the lectures which I have recently had the honour of delivering in the United States. The arcs of thallium and silver had just been compared, and their similarity of colour pointed out. The power of prismatic analysis to show that, notwithstanding the apparent identity of colour, the arcs really belonged to two different metals, was then demonstrated. The metals were afterwards subjected *together* to the action of the Voltaic current, and it was shown that the band of thallium fell midway between the two bands of silver. Hence the similarity of colour. The lecture then proceeds thus:—

"But you observe here another interesting fact. The thallium band is at first far brighter than the silver bands; indeed the latter have wonderfully degenerated since the bit of thallium was put in. The reason of this is worth knowing. It is the *resistance* offered to the passage of the electric current from carbon to carbon that calls forth the heating power of the current. If the resistance were materially lessened, the heat would be materially lessened; and if *all* resistance were abolished there would be no heat at all. Now thallium is a much more fusible and vaporisable metal than silver, and its presence facilitates the passage of the current to such a degree as to render it almost incompetent to vaporise the more refractory silver. But the thallium is gradually consumed; its vapour diminishes, the resistance consequently rises, until finally the silver bands are rendered as brilliant as at first."

In the spectra of mixed substances derived from the electric spark the action here referred to must come frequently into play. If neither the fact, nor its proposed explanation, be new, I would thank you to commit this document to your waste-paper basket.

JOHN TYNDALL

Royal Institution, March 1873

Perception in the Lower Animals

THE theory of taking olfactory notes by the way, as suggested by Mr. Wallace in explanation of the faculty possessed by animals of finding their way home, seems to meet with general acceptance amongst your correspondents; yet it totally fails to account for those instances in which the animal finds its way back by quite a different route to that by which it was taken away.

A good example is given by "F. R. G. S.," in the last number of *NATURE*; the anecdote of his riding-horse, by Mr. Darwin, also seems to illustrate this point. In an article on the "Consciousness of Dogs," in the *Quarterly Review*, of last October, the following remarkable instance, amongst others, is mentioned on indisputable authority. A hound "was sent by Charles Cobbe, Esq., from Newbridge, county Dublin, to Moy-nalty, county Meath, and thence, long afterwards, conveyed to Dublin. The hound broke loose in Dublin, and the same morning made his way back to his old kennel at Newbridge, thus completing the third side of a triangle by a road he had never travelled in his life."

Now as Mr. Wallace's theory does not explain these and similar instances, it clearly cannot be received as a solution of the question. Moreover, not only does the faculty exist in other animals not remarkable for their sense of smell, but we find it in cases where this sense has nothing to do with it. Take, for example, the direct homeward flight of the carrier pigeon. Under the same head may be brought the migrations of birds and fishes, and the habits of the turtle, as mentioned by Mr. Darwin.

The writer in the *Quarterly* suggests a sense of the magnetic currents of the earth—a sort of internal mariner's compass in fact. But it is difficult to see how this could have helped the dog to find its way from Dublin to Newbridge, for instance, unless it was also able to consult a map so as to ascertain the relative position of the two places.

It seems then that the problem still remains unsolved. Either we must extend almost indefinitely the range of smell and sight; or, we must suppose the existence of some peculiar sense of the nature of which we are ignorant, which enables its possessor to retain, as F. R. G. S. expresses it, "a constant perception of the bearing of its old home."

J. T.

Bath, March 17

POSSESSION ISLES

AS the idea of occupying Possession Islands as a station for observing the Transit of Venus has been lately propounded, I have been requested to communicate to *NATURE* the results as to its climate, which we have obtained in this office from the logs of H.M.S. *Erebus* and *Terror*, which we are now re-discussing with a view to publication.

Possession Isles are in lat. $71^{\circ} 56' S.$, long. $171^{\circ} 7' E.$ H.M.S. *Erebus* and *Terror* were within lat. 70° to $72\frac{1}{2}^{\circ} S.$, and long. 170° to $175^{\circ} E.$ from 10th to 17th January, 1841. During these eight days the mean height of the barometer was $29\cdot143$, mean temperature of the air $29^{\circ}\cdot7$, and of the sea $30^{\circ}\cdot5$; the wind was variable, but chiefly from S. and SSW., force 6; the weather was clear ten times, cloudy twenty times, overcast eighteen times, from forty-eight double sets of four-hourly observations, while snow was noted nine times, and squally weather ten times.

The ships were within the same area on 20th and 21st February, 1841; and, during these two days, the mean height of the barometer was $28\cdot920$ inches, mean temperature of the air $23^{\circ}\cdot5$, of the sea $30^{\circ}\cdot1$; wind WSW. to SE., force 9 to 5; the weather was cloudy and overcast.

In addition I am permitted to enclose a letter from Dr. Hooker, which he kindly sent me in reply to my inquiries as to his reminiscences of his visit to these inhospitable regions, and which he has allowed me to publish.

Meteorological Office

ROBERT H. SCOTT

Letter from Dr. Hooker

Possession Island, or rather Possession rock, is in a very inaccessible position. The chance of landing a well-equipped party upon it when reached, and the prospect of its subsequent removal by ships, if landed on, is very small. In any case I feel little uncertainty as to what would be the fate of a party left there for the winter, and the prospect of their seeing the transit would be absolutely *nil*.

To reach it we "took the pack" January 3, 1841, and had not penetrated it till the 9th, aided at last by a furious gale. We then discovered South Victoria, and traced its coast from lat. $70\frac{1}{2}^{\circ}$ to lat. 78° , without finding a spot where it was possible to approach the shore. During the