must be some connection between the two classes of phenomena. The nature of this connection has been differently explained by different writers. But the purpose of this note is not to criticise existing theories, but to propose one, which I believe to be new, and to be capable of explaining why a sudden volcanic eruption must ordinarily be accompanied by earthquake shocks of greater or less violence (not necessarily always sensible), and why earthquakes may occur without any contemporaneous outburst.

In the preface to his "Physikalische Geologie," Bischof suggests that the phenomena observed in the laboratory should be taken as our guides to explain what happens in nature. Let us see, then, if in the laboratory we meet with any phenomena analogous to volcanoes and earthquakes.

When a reaction has to be performed in a sealed tube, and it is expected that much gas will be evolved, and consequently the pressure in the tube be much increased, it is one of the commonest precautions to draw out the tube to a capillary orifice before closing it. When this precaution has been neglected, and even although the point be allowed to blow itself out in the flame of a lamp, an explosion not unfrequently attends the attempt to open it. Let us consider the circumstances.

We have a tube whose walls are being pushed out by a very high pressure from within, which, however, it resists; but at the moment that this pressure is suddenly relieved at one point, the tube bursts. What is the cause of the explosion? It clearly cannot be the mere reduction of pressure. As long as the pressure was equally distributed over the walls of the vessel, we have seen that it was successfully resisted; as soon, however, as it was suddenly relieved at one point, a great inequality in the tension of the gas in the immediate vicinity of this point would be the result, the gas immediately at the opening assuming at once the atmospheric pressure, while that at, say, the eighth of an inch from it is at the tension of the gas in the tube. The practical effect of this sudden inequality of pressure would be to produce a tug on the mass of elastic fluid, which would cause the walls momentarily to tend to collapse, and this tendency to collapse would be transmitted through the glass as a wave. This wave would to a certain extent distort, and therefore weaken, the walls; and consequently, if the pressure inside were great enough, it would burst them; if not, the only effect would be that a shock would traverse the walls of the vessel, and the pressure would relieve itself by the orifice.

Now, suppose the vessel to be a subterranean cavity containing an atmosphere of very great tension, and that suddenly the envelope gives way at one point, what will be the result? Just as in the case of the glass tube, the sudden relief of pressure will, in the way indicated above, cause the walls to experience a momentary collapsing impulse, which will be propagated as a wave until extinguished by the imperfect elasticity of the crust. The sudden outburst will be a volcanic eruption, and the consequent collapsing shock will be an earthquake, which either will or will not be accompanied by rending of the crust, according to the strength of the walls and the greatness of the pressure.

It is, however, not necessary that there should be a visible volcanic eruption. For, suppose two such subterranean cavities at different pressures, separated from one another by a wall weaker than that which separates either of them from the outside of the earth; then, if the pressure in the one becomes so great as to burst the barrier between the two, the result will be an earthquake. And similarly, the pressure in the two thus united cavities may go on increasing until they burst into a third, and so on until they come to a vent, which is either open or weak enough to yield to the pressure. In this way an earthquake and an eruption may be in intimate connection with one another, although a considerable interval of time intervene between the occurrences, and the localities affected be at great distances from each other. And it is possible that some connection of this kind may have existed between the earthquake of Antioch and the eruption of Vesuvius, both having been extreme in their violence. Indeed, the whole series of disturbances, commencing with the earthquake in California and terminating with the eruption of Vesuvius, noticed by Mr. Corfield in *NATURE* of May 23, may possibly find an explanation under this theory.

The effect of sudden relief of pressure in weakening the walls of vessels explains many cases of explosion which otherwise appear anomalous. Thus, high-pressure boilers have been frequently observed to burst at the moment when the engineer turns on the steam.

In conclusion, the above sketched theory assumes nothing but what we know to be fact. We know that, at least in the neighbourhood of volcanoes, there must be subterranean cavities whose

atmosphere is at an exceedingly high-pressure, for we not only see it emitted from the vent, but it projects enormous masses of rock high into the air, thus testifying to the energy with which it was endowed. Further, given this high-pressure atmosphere, it is certain that, on its being suddenly relieved, it would communicate a shock to the crust, and this, on being felt outside, would be described as an earthquake. I think it is therefore clear that some earthquakes must be produced in this way. Of course this does not include the possibility of there being other causes of concussion which might produce similar effects.

Edinburgh, June 10

J. Y. BUCHANAN

On the Cohesion of Figures of Creosote, Carbolic and Cresylic Acids

WITH reference to the note by Mr. J. H. Spalding contained in *NATURE* of June 13, I am reminded by my friend Mr. Rodwell that some five or six years ago I showed him the cohesion figure of carbolic acid. A crystal of this acid was taken up on the end of a platinum spatula, and gently delivered to the surface of clean water contained in a clean glass; the crystal gave a few jerks, then suddenly liquified, and displayed its highly characteristic figure so well described by your correspondent. I may further remark that I showed this figure in the chemical section of the British Association, at Manchester, in September 1861, and a drawing of it is given in the plate which accompanies my paper in the *Philosophical Magazine* for October of that year. In this paper it is described as "an exaggerated form of the figure of creosote; the water seems to tear it to pieces; the crispations are amazingly active, and the disc quickly breaks up and disappears. Indeed, while a drop of creosote will endure five minutes on the surface of an ounce of distilled water in a small glass, a drop of carbolic acid will last only a few seconds on the same quantity of water. The cohesion figure is however quite characteristic of the substance, and cannot be for a moment mistaken for any other substance that I have examined."

Creosote, carbolic and cresylic acids, and newly-distilled oil of cloves, give remarkable figures of the same type, each of which is characteristic of the substance.

Mr. Spalding remarks that warm water destroys all action, by lessening, as he supposes, the adhesion of the liquids. I am sorry to have to object to this remark, but I have no doubt that Mr. Spalding was led to make it by employing unclean water or an unclean recipient. If distilled or even ordinary tap water be heated over a spirit lamp in a clean flask, and be poured into a clean vessel, the surface of the water is active at all degrees between the temperature of the air and just below boiling. On the surface of cold water a drop of creosote passes through the following changes:—(1) As soon as the drop is placed on the surface of the water the figure is formed for an instant; (2) it splits open and forms a kind of brittle arc, which (3) is shivered into a number of separate discs, each of which is a perfect cohesion figure of creosote. These figures perform their evolutions independently of each other, sailing about with rapidity, but never clashing with or disturbing each other. In the *Philosophical Magazine* for June 1867, figures are given of these different phases of the figure.

Now, if the water be heated to 100° Fah., or from that to 150°, a drop of creosote deposited on its surface produces a good active figure, but it does not split open or form the brittle arc above referred to; it sails slowly over the surface, firing off volleys of small globules in radial lines, and only when much wasted does it split into smaller systems. All this is what might be expected from the diminished surface-tension occasioned by the heat. Indeed, it is a beautiful illustration of the slight diminution of surface-tension in hot water as compared with cold. In consequence of not splitting open, the duration of the figure is greater on the surface of hot water as compared with cold.

Carbolic and cresylic acids are also very active on hot water, at all temperatures up to 210° Fah.

C. TOMLINSON

Highgate, N.

Hereditary Instinct

WILL you allow me to recount to your readers what appears to me to be a striking instance of the transmission of impression in animals?

A few years ago I bought in Skye a perfectly uneducated Skye terrier. The first accomplishment which I taught him was

that of "sitting up"—an accomplishment which he had great difficulty in acquiring. This was not owing to any stupidity on his part, for when he had once passed over this *pons asinorum* of dog-performances, he proved to be a very clever animal, and learnt many other tricks with great ease. He appears, however, never to have forgotten the pains which were taken to teach him his first trick, and to have judged therefrom that there is great merit in sitting up. Not only does he rely upon this as a last resource to move me to take him out, or not to whip him, but he judges that it must soften even the heart of an india-rubber ball. Sometimes when annoyed at his playing with this, his favourite toy, I have placed it on a chimney-piece, and turned my attention elsewhere. On looking round again I have seen my dog sitting up to the india-rubber ball, evidently hoping that it would jump down and play with him again. Perhaps he looks upon this ball as "animated by a living essence" (*vide* Chap. ii. of Darwin's "Descent of Man").

My dog is now the father of a family, and one of his daughters, who has never seen her father, is in the constant habit of sitting up, although she has never been taught to do so, and has not seen others sit up. She is especially given to this performance when any other dog is being scolded. Whether this is an instance of helping a fellow animal, of which Mr. Darwin gives such curious examples, or whether the dog simply hopes to avert the passing storm from her own head, the fact appears to me patent, that this dog has inherited the impression that sitting up has some special virtue for turning away wrath.

L. HURT

Alexandra Hotel, Harrogate, July 27

RECENT OBSERVATIONS IN THE BERMUDAS

AS my late visit to these islands has placed me in possession of facts relating to their original aspect of a somewhat conclusive nature, I deem it advisable to communicate such in a brief form, instead of awaiting the time requisite for the preparation of a more elaborate paper on the subject.

On previous occasions I have always regretted my inability, from lack of time, to look more closely into their geological character in the hope of discovering some satisfactory clue to their primitive condition. I was aware that in different parts of the islands road cuttings and well borings had revealed layers of red earth at certain depths below the surface, the consistence of which was similar to that now forming the present surface soil, and it did not require much force of imagination, after personal inspection, to conceive that such layers of red earth were first formed by the decomposition of vegetable matter which grew upon former surfaces, and became covered to their respective depths by accumulated masses of drift sand, which from natural causes hardened into more or less compact sandstone. But these different layers were but a few feet beneath the surface, and so, although interesting as throwing light upon the gradual elevation of the land by drift material forming over them, yet they afforded no evidence of a contrary nature—viz., the *submergence* of the Bermuda group. Indeed, I have always been led to suppose from appearances that the whole group was the result of an upheaval of the ocean bed slightly above the water level, and a gradual elevation afterwards by means of drift matter aided by the consolidating agency of reef-building zoophytes encircling the whole with a barrier reef, and by isolated patches gradually filling up the space within. The investigations, however, which I have recently been able to make, tend I think to prove that the barrier reef encircling the islands which has hitherto been considered an atoll is merely the remnant of the more compact calcareous rock which formed the shore of a much more extensive island group than that now existing.

My views in this respect are borne out by the following facts:—The barrier reef, as far as I have inspected

it, is merely ordinary calcareous rock coated with serpulæ, nullipores, &c., the reef builders only working in the sheltered waters between the reef and the shore in three to eight fathoms. About two years ago submarine blastings were carried on at the entrance of Hamilton Harbour, and at a depth of over six fathoms a cavern was broken into which contained stalactites and red earth. Again, within the last few months, I have, through the kindness of his Excellency Major-General Lefroy, C.B., F.R.S., the present Governor, been placed in possession of still more satisfactory information. During the past two years extensive submarine blastings have taken place inside an artificial harbour, situate at the western extremity of the islands, for the purpose of forming a bed of sufficient depth for the reception of the "Great Bermuda Dock," which attracted so much attention off Woolwich when launched some three or four years ago. The excavations extended to a depth of fifty-two feet below low water mark. At forty-six feet occurred a layer of red earth two feet in thickness, containing remains of cedar trees, which layer rested upon a bed of compact calcareous sandstone. Here we have the first satisfactory evidence of the submergence of an extensive deposit of soil once upon the surface, and that to the depth of forty-eight feet below the present low water level, which consequently grants an equal elevation above it in former times. Now, on carefully surveying the Bermuda chart, we find that an elevation of forty-eight feet will bring the whole space which intervenes between the present land and the barrier reef, now covered with water, above the water level. This attained, what more is required to prove the former extent of the island group before the present submergence to the present barrier reef? But having clearly ascertained beyond doubt that the Bermudas were once forty-eight feet higher than at present, will any one be bold enough to deny them a greater elevation? I have reason to believe that they once extended in a south-westerly direction—not only out to the reef, but to a greater distance. There are some rocky ledges about twenty to twenty-five miles from land in that direction, known as "The Flatts," lying in about thirty-five to forty fathoms water; and, singularly enough, in the very oldest maps of the Atlantic, copies of which I have consulted in the British Museum, "The False Bermudas" are put down about this position. Is it unreasonable to suppose that a low lying group of islets did actually exist here in former times? Again, in Smith's "History of Virginia," which gives an excellent account of the islands in the early part of the seventeenth century, it is stated among other notes upon their natural history that flocks of crows, no doubt the same species (*Corvus Americanus*) which now inhabits them, were in the habit every evening of winging their flight from the main island towards the north. This observation, which from its simplicity I should the more readily believe to be a true statement, would clearly prove the existence of land in that direction at no great distance; for the habit of this bird to leave its roosting place for distant feeding grounds during the day, to return at random, is one of its well-known characteristics.

Taking these matters into consideration, I see everything to support the supposition that the Bermudas once presented a much more extensive aspect than they do at present, and certain additional evidences which I hope to bring forward shortly in a collected form will, I conceive, tend to confirm my impression that the restricted terraqueous area lying within the limits of the outer barrier reef is merely the summit of one of a range of islands which extended in somewhat semicircular form for a distance of seventy or eighty miles, and which have suffered submergence to a depth only to be correctly ascertained by borings, which might be successfully accomplished under the auspices of the Imperial Government at a trifling expense.

J. MATTHEW JONES