

of these papers was read before the Geological Society on the 24th of November, 1869, and appears in the February Number of the Society's Journal. (See GEOL. MAG. January, 1870, p. 36.)

Professor Owen is opposed to the union of Birds and Reptiles, and points to their affinity with the Implacental Mammals, in the development of the embryo, in the brain, in the circulating system, etc. He strongly combats the inference that warm blood is an essential accompaniment of an animal endowed with the power of flight, and cites the cockchafer as an instance of a living creature capable of powerful flight, whose body, nevertheless, only raises the thermometer one degree above the surrounding medium. He maintains that the constant accompaniment of hot-bloodedness is a non-conducting covering to the body, and that we may with certainty infer that *Archæopteryx* was *hot-blooded*, because it had feathers, not because it could fly. We think a happier illustration than the cockchafer (so dissimilar in bulk and organization from the Pterodactyle) is that of the shark and porpoise—the former cold-blooded, the latter warm-blooded—both performing the same muscular feats, in following a ship for days, with equal ease. (See Prof. Owen's Monograph, p. 73.)

Space does not admit of our pursuing this interesting subject, but we have said enough, we trust, to show that, taking a single Monograph out of the present fasciculus, forming Vol. XXIII., there is garnered a rich store of palæontological matter for those who care to benefit by the works of this useful Society.

EXPLANATION OF PLATE IV.

- Fig. 1. *Dimorphodon macronyx*, Owen ($\frac{1}{3}$ original). Restoration, copied from Prof. Owen's Monograph in Pal. Soc. 1870, vol. xxiii., pl. 20.
 Fig. 2. Probable outline of integument (greatly reduced in size).
 Fig. 3. Four of the tail vertebræ of the natural size, showing the fine parallel bony fibres which strengthen and render rigid the slender joints.
 Fig. 4. Foot of *Rhamphorhynchus* ($\frac{1}{2}$ nat. size) placed, for comparison, with hind limb of *Dimorphodon*, also copied from Prof. Owen's plate.
 Fig. 5. Side view of skull of Recent Saurian (*Lyriocephalus*).

II.—ON THE CHARACTER AND COMPOSITION OF LAVAS.

By G. POULETT SCROPE, F.R.S., F.G.S., etc.

I HAVE been gratified by observing of late the appearance among geologists of a more general appreciation of the study of volcanic phenomena,—using that word in its broadest sense, as comprehending not merely the occasional outbursts of vapour, ashes, and lava, but also the action of those subterranean forces, to which alone we are indebted for the existence, now or in former times, of any dry land whatever above the dead sea-level at which the agents of denudation would otherwise maintain the surface of the globe.

Perhaps the tendency of recent writers on this subject has been rather to indulge their imaginations by theorizing on the possible nature of the interior of the earth, than to examine carefully the facts observable on its surface. I venture, therefore, to recal those who show this inclination to wander from the legitimate path of in-

vestigation—namely, through the known to the unknown—to the necessity of acquiring some more definite notions than can be said to prevail at present, on the character and mode of production of the hypogene rocks which are exposed at or near the surface, before proceeding to speculate further on the condition of those that possibly exist at great depths beneath it.

Among the facts relating to Volcanic Geology which have not as yet, in my opinion, been satisfactorily determined by observation, although freely open to it, I may mention, as especially deserving of examination, the character and composition of lavas, as they exude from the vents or fissures opened for their issue through the solid crust. In the year 1856 I read before the Geological Society of London a paper on this subject (as well as on the mode of formation of Volcanic Cones and Craters), which does not appear to have attracted much attention from Geologists, but which I think contained suggestions worthy of their consideration. (See *Quart. Journ. Geol. Soc.*, vol. xii., p. 338.) If, indeed, we wish to form any reliable opinion upon the nature and character of the subterranean matter underlying the sedimentary rocks, surely a close examination of those portions of this matter which are in so many places rising to the surface and spreading themselves over it, is indispensable.

In the paper above referred to I showed how little foundation there is for the generally received opinion that all lavas, as they issue from a volcanic vent, are in a state of complete fusion, like melted glass or metal, and that it is only through a process of slow cooling they afterwards assume a crystalline texture. I pointed out that there is good reason on the contrary for believing that the greater number, if not all, of the crystals observable in lava after cooling and hardening, (and the microscope discloses them where they are not visible to the naked eye), existed there in a more or less complete form previously to its emission. Some of the facts mentioned in support of this view I may briefly recapitulate. Such are, the extremely stiff and tenacious character of most lavas, as they issue at a white heat from the volcanic orifice, making it difficult to thrust a pointed iron rod into them; the absence of a vitreous texture, even in their superficial portions, or in the scorïæ, torn seething hot from the surface of the liquid matter within the vent by gaseous explosions; the instantaneous consolidation of exposed surfaces in cellular or porous slabs or cakes, which on fracture are found to have the same crystalline texture as the interior of the current; the cracked and more or less vitrified aspect of the felspar crystals of many trachytes; the broken and dislocated appearance of the leucites, felspars, and other crystals in many basalts; the frequent arrangement of the longest axes of such crystals in the direction of the rock, that is, of the movement of the lava when liquid; the finer grain often exhibited towards the tail or extremity of a current than at its source, as if the crystals had been broken up by friction as the matter moved on; the brecciated lavas which appear to have enveloped numerous fragments of the same equally crystalline material without any fusion even of their finest angles, etc.

Of course in these remarks I am referring solely to those lavas which after cooling exhibit a crystalline texture; not to the vitreous lavas, such as the obsidian and pumice streams of the Lipari Isles, Japan, the Andes, Hawaii, and Bourbon, or the Pearlstones of Hungary and South America. The very fact of the existence of these vitreous lava-flows—having a glassy texture not only on their surface but throughout their mass—affords a negative proof that the stony and crystalline lavas could not have been ejected in the same state of complete vitreous fusion as the former, or why are they not equally glassy and homogeneous throughout, having evidently cooled under the same circumstances of exposure to the atmosphere? In the Lipari Isles there are to be seen thick currents of obsidian and pumice side by side with lavas, produced by the same volcano, composed of an aggregation of interlaced crystals, chiefly labradorite, as large as those of ordinary granite. A rock very similar to this latter composes the mass called the Monte Olibano, near Puzzuoli, seventy feet in thickness, which has flowed over beds of loose ashes from the crater of the Solfatara into the sea. Though scoriaceous near its surface, it is as largely crystalline there as in the interior of the rock.

But, it may be asked, if lavas are already crystalline when they issue from a volcanic vent, how is their fluidity to be accounted for? I reply, Firstly, that lavas vary greatly in this character; some, though extremely viscous, having the fluidity of honey or of mud, so as to flow rapidly down a steep slope, and spread, on moderately flat surfaces, into wide and thin sheets; others possessing such a low degree of liquidity as to coagulate in thick beds (like that of Olibano just referred to,) even on very steep slopes, and, when emitted upon a nearly level surface, to accumulate in bulky hummocks over and around the vent, such as the Puy de Dome and the neighbouring trachytic bosses in Auvergne. It will be found on examination, I believe, a general fact, that the more crystalline lavas show signs, in their bulky forms and other circumstances, of their having been less liquid when propelled from the vent than the more fine-grained; and also that the vitreous lavas exhibit marks of the greatest fluidity—other circumstances, especially their specific gravity, being the same. For it is certain, as might indeed be expected, that the heavier basio or augitic lavas have generally spread further and in thinner beds than the lighter, or acid (trachytic) ones.

Secondly. So long ago as 1825, in the first edition of my Treatise on Volcanos, I suggested that the fluidity of lavas, even when composed for the most part of ready-formed crystals or granules, might be accounted for by the presence, throughout the mass, of a certain amount of interstitial water—of course at the temperature of the lava itself, and therefore probably in the state of minute globules, such as the experiments of M. Boutigny exhibited, and tending to flash into steam on the least relaxation of the pressure upon them. Hence the vesicles and air cells that form in the upper portions of lava streams, and often burst from them in jets of vapour. Hence, too, the instantaneous cooling and consolidation of these exposed

portions; the escaping steam carrying off an immense amount of caloric. The finer the grain the more readily would the struggling vapour be enabled to collect and rise in bubbles, and hence the more vesicular structure of the glassy lavas; while in those of coarser or more crystalline grain, the expanding steam would be likely to remain longer entangled in the magma, causing it to assume a spongy or loosely granular texture, and to swell up like a mass of dough or paste in an oven; while the vapour would ultimately escape by filtration through the pores of the rock, rapidly consolidating on exposure, and splitting up by the formation of shrinkage cracks into such rude prismatic blocks or cakes as are seen to characterize the surface of lava-streams of this coarse grain.

One word upon the varying mineral and chemical characters of Lavas. I cannot but think that far too much importance has been attached to these distinctions, especially by the German geologists. By many of these, as in the instance of Baron von Richtofen, whose classification of Volcanic Rocks was lately reviewed by me (*GEOL. MAG.*, Vol. vi., p. 518), these differences, in their minutest peculiarities, have been laid down as determining the relative age of the respective rocks. There can be no greater source of error. It is certain that many varieties of trachyte and basalt, and rocks of intermediate mineral character—that is to say, with a greater or less proportion of acid or basic elements in their composition—are often found succeeding each other as products of the same volcano, in no definite series; sometimes one class, sometimes another, having been first ejected. Nay, they are to be seen occasionally, though rarely, to pass into each other in the same mass, just as some granites are found locally passing into syenite, and this again into greenstone. There are even lavas, as for example that called *Peperino*, so much employed in buildings at Naples, in which zones or lenticular blotches of different mineral character alternate throughout the rock, the augitic matter having apparently separated itself from the more feldspathic by a process of segregation during the efflux of the lava. And there need be little doubt that what has taken place in this instance on a small scale has frequently occurred on the large one, within the focus of a volcano, during the, perhaps repeated, processes of alternate fusion and re-crystallization, to which a mass of subterranean lava has been probably exposed, under varying circumstances of temperature and pressure. (See *Volcanos*, pp. 129–132.)

If these views are correct—and it is for younger field-geologists than myself to prove or disprove their truth by a close examination of volcanic districts—they cannot but throw much light on the nature and character of the heated material that underlies the crust of the globe, and makes itself known, both in outward flows of lava, and in the penetration of the fractured crust, by intrusive veins, dykes, and protruded bosses of crystalline rock—in the deeper-seated syenites, granites, porphyries, and serpentines, no less than the trachytes, greystones, and basalts of sub-aërial eruptions. I hope, therefore, to be excused for repeating here ideas on this subject,

formed half a century back, and published almost as long; but which still ask for corroboration or disproof from more competent observers. They have, I venture to think, an important bearing on the history of the changes traceable on the surface of our planet, and ought to be worked out, before any justifiable attempt can be made to solve the problem of its sub-cortical character.

P.S.—While the above was going through the press I have read the paper on the Liquefaction of Rocks, by Dr. Sterry Hunt, in the February number of this Magazine. My thanks are due to Dr. Hunt for the generous manner in which on this, as on former occasions, he has acknowledged, and even taken pains to vindicate for me, such small merit as I may claim for the original advancement of the suggestion—adopted by Dr. Hunt—as to the effect of variations of temperature and pressure in promoting alternate liquefaction and reconsolidation in a mass of mineral matter beneath the crust of the earth.

Dr. Hunt points my attention to a notice, by the Rev. O. Fisher, in the number of *Scientific Opinion* for October 27th last, which had escaped my observation. In it Mr. F. expresses a doubt as to the idea above referred to having been advanced in the *first* edition of my work on Volcanos (1825), he having searched through that volume without finding it. I beg, however, to refer him, or any one whom the point may interest, to p. 26 of that volume, headed “Effect of Increase of Temperature or Reduction of Pressure.” Mr. Fisher will see, on reference to this and the following pages, that the passage quoted by Dr. Hunt (p. 60 *supra*), from the edition of 1862, only expresses, in more concise language, the idea here enunciated; which, indeed, forms the staple of my whole argument.

Dr. Hunt, however, differs from me in one important respect, believing that, under the supposed circumstances, the effect of increased pressure would be exactly the reverse of that which I suppose; pressure, in his view, promoting, not preventing, liquefaction, by “favouring the solution of the water-impregnated mass, solution being, with few exceptions, a process of contraction.” I do not dispute the chemical theory on which this proposition is based, and, indeed, the partial solution of some of the mineral constituents of the subterranean matter (especially the silver) is involved in the idea of its igneo-aqueous fusion. But it is not inconsistent with this belief to suppose that the tendency to general liquefaction in the mass, possibly occasioned by such partial solution under increased, and the converse solidifying influence of diminished, pressure, may be more than counteracted by the opposite tendency of such portions of the super-heated interstitial water as exist at the time in the state of vapour, to contract in volume in the first, and to expand in the last case. It seems to me that all the phenomena of volcanic action are best explained by this hypothesis, rather than by the alternative which Dr. Hunt proposes. It is consistent with observation that as lava rises upwards in a volcanic vent, its effervescence—that is, its tendency to liquefaction and elastic expansion—increases with a diminution of pressure, even though so slight as

that attested by a fall in the barometer, as is seen in the case of Stromboli. And it is difficult to believe that this process is reversed in the interior of the volcano. On this point I will say no more, but only refer to my paper in this Magazine, Vol. V., p. 537.

Before concluding, I ought, perhaps, to notice the challenge addressed to me by Mr. Fisher (p. 45 *supra*), who, attributing to me, with justice, the opinion that "the motive power in the elevation of mountain chains is the pressure from below of matter expanded by an accession of heat," opposes to this view what he calls the contrary one, viz., that the effect in question is produced by "the crumpling of the crust through lateral pressure caused by a general cooling of the globe; the elevation of portions of the crust, by this process, occasioning a diminution of pressure and consequent liquefaction in the subjacent mass." My reply is, firstly, that my view is built solely on recognized facts, such as the proved upward transmission of heat from below the earth's surface, and the undeniable inference from known laws of physics, that the rate of this transmission, in other words, the subterranean iso-thermal plane, must vary locally with those variations in the thickness and conducting powers of the sedimentary deposits on the surface, which we know to be always taking place, (p. 308, *Volcanos*). I have purposely abstained from theorizing upon any mere *assumption*, such as that from which Mr. Fisher deduces his view, viz., the secular cooling of the entire mass of the globe, and its "consequent contraction within a solidified crust"—an assumption which, in the present state of our knowledge, is not, I think, sufficiently justified to afford a reliable postulate in geological speculations, though I am quite aware of its popularity and plausibility. At the same time I may observe that this view of the primary cause of the elevation of mountains (which is that of Cordier, Elie de Beaumont, and many others) is not inconsistent with mine, viz., that the direction taken by the lines of fracture in the earth's crust, under the supposed circumstances, and consequently the positions of the alternate areas of elevation and depression, must be determined by the local conditions of comparative tension—that is, of temperature—in the matter beneath, and of the resistance in the crust above; the elevated areas consequently coinciding with those in which the expansive influence of subterranean heat predominates over the sum of resistances offered by the crust above; and the depressed areas to those in which these conditions are reversed. That intense heat was the chief agent in the upthrust of the crystalline axes of most mountain chains is proved, I think, by the signs they exhibit of having *melted their way into* the overlying rocks, which they at the same time appear to have upheaved. While the volcanic or trap rocks, on the contrary, seem to have risen through fissures with scarcely any disturbance or metamorphism of the sides of these. The former I consider the direct effect of subterranean expansion from increase of heat; the latter its secondary effect, due to the opening of lateral avenues of up-flow by the action of the first force. (See *Volcanos*, ed. 1862, p. 273.)
