On the FORMATION *of* CRATERS, *and the Nature of the* LIQUIDITY *of* LAVAS. By G. POULETT SCROPE, Esq., M.P., F.R.S., F.G.S.

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Introduction.—It is now some thirty years since I published two works* upon the Phænomena of Volcanos, Active and Extinct. I described in them, as accurately as I could, by pen and pencil, what I had observed during a residence of some duration among the volcanic districts of France and Italy; and explained, in considerable detail, the laws which, from those observations, I believed to regulate the remarkable developments of subterranean energies usually called volcanic, which have played so important a part in the construction of the superficial crust of our planet.

The general principle on which I proceeded in the theoretical portion of these works was the same which had been previously employed by Hutton and Playfair, and was subsequently adopted, with signal success, by Sir Charles Lyell,—namely, to refer, so far as is possible, appearances the origin of which has not been witnessed, to such causes as are seen or known to produce analogous appearances in the present day,—instead of resorting for the purpose to imaginary hypotheses.

In the earlier volume of the two (the Considerations on Volcanos), however, I certainly overstepped this wholesome rule, by entering towards the conclusion of the work upon some rather crude speculations on a general theory of the globe; and this, together with defects of style and arrangement, and likewise of illustration, of which I became sensible only when it was too late to amend them,

* "Considerations on Volcanos," &c., 1825–6. "On the Geology of Central France," &c., 1826–7.

sufficiently accounts for the different reception these two works met with from geologists at the time. Neither, however, I presume to hope, were wholly without some beneficial result. At the period of their publication, the Wernerian theory of the precipitation from some aqueous menstruum, not merely of granite, and what were then called the primitive formations, but even of all the trap-rocks, still prevailed, and had the support of a large school of geologists in this country. I venture to think that the facts reported in my two volumes (especially those represented to the eye in the atlas illustrative of the volcanic remains of Central France) had some share in the final extinction of that German romance,—which some geologists as old as myself may remember to have been regarded almost in the light of a gospel-truth, and defended with all the acrimony of polemical controversy.

Some of the opinions, however, expressed in these works with respect to the laws that govern volcanic action, were severely criticised at the time. Others have been since opposed by rival theories. And, as these disputed questions have an important bearing on some of the most interesting problems of geology, I trust it may not be unprofitable to call the attention of our Society to the more prominent among them.

I will advert on this occasion to two subjects especially, viz.

I. The origin, or mode of formation, of volcanic cones and craters.
II. The nature of the liquidity of lava at the time of its protrusion from a volcanic aperture.

I. *Formation of Cones and Craters.*—In both of the works to which I have alluded, I referred the formation of those remarkable circular hollows, usually called craters, which are of such frequent occurrence in volcanic districts, to explosive aëriform eruptions, breaking their way through the superficial rocks; and that of the external more or less conical hill or mountain which generally, but not always, environs a crater,—and which, indeed, often occurs without a crater, but always characterized by the quâ-quâ-versal dip of its constituent beds of lava and conglomerates,—to the accumulation, round and above an eruptive vent, of its fragmentary ejections and the lava-streams poured out from it.

I considered this law to be without exception; attributing the differences in figure and structure apparent among volcanic cones to the greater or less number and violence of the eruptions to which they were owing,—some being the product of a single eruption, others of a vast number, often repeated through a series of ages,—to differences in the position of the orifices of discharge, whether from the summit of the cone, or its base, or any intermediate points, —and whether from under water, or in the air,—to the varying mineral character of the products,—and to the influences of subsequent degradation.

At the same time I remarked that the earthquakes which always more or less accompany volcanic eruptions render probable a certain amount of elevation in mass of the pre-existing superficial rocks; and moreover that the rents they cause in the solid substance of the cone of a volcano in repeated eruption, into many of which rents liquid lava will be injected from the column rising in the central chimney, and cool down afterwards into more or less vertical dykes of solid rock, must have added considerably to the bulk and elevation of such a mountain, by a sort of inward distension.

This was no closet-theory,—because, as respects the cone and crater of Vesuvius at least, I had the advantage, in the years 1818, 1819, and 1820, of watching with my own eyes the outward growth of that cone, through a series of almost continual eruptions of a comparatively tranquil character, which during those years added considerably to its height and bulk by external accretions of ejected scoriæ and lava-currents. These last, the lava-streams, issued from small cones and craters formed upon the solid platform which then composed the summit of the great cone, and dribbled slowly down its slopes, consolidating so rapidly there as in few instances to reach the base of the cone at all; although night after night they were to be seen flowing from the summit in streams of considerable breadth and bulk, and glowing with a bright light on its steep sides.

Afterwards, in the latter part of the year 1822, I had seen the upper portion of this solid cone blown into the air (by which it lost a full third of its height), and a crater of vast dimensions drilled through its axis by continuous eruptive explosions of twenty days' duration.

I had previously made a close examination of the cones and craters of Etna, the Phlegræan Fields, the Lipari Isles, Central France, and the Rhine district; and their appearances accorded so completely with the supposition of an analogous mode of formation in their instances, that, upon the principle of explaining the unknown by the known, it seemed impossible, or at least unnecessary, to imagine any other origin for them.

"Elevation," "Denudation," and "Engulfment" Theories of Crater-formation.—It was, therefore, with no small surprise that I have since found this simple and natural mode of production denied to all cones and craters—including those of Vesuvius itself; and an hypothesis substituted of their originating in some sudden elevation of previously horizontal beds around a centre,—not (it would seem) of eruption, but of maximum elevation. I allude, of course, to the "Elevation-crater theory" of MM. Von Buch and Elie de Beaumont.

Sir Charles Lyell, M. Constant Prevost, and others, have amply refuted this unphilosophical theory; which, however, still appears to hold its ground to some extent on the Continent, through the prestige of the great names attached to it. It may, therefore, not be wholly useless to adduce some additional proofs of its unwarrantable character. But I must first be permitted to remark, that even Sir Charles Lyell, while supporting the view indicated above, of the generally eruptive origin of volcanic cones, has had recourse, in the case of some craters, to another agency, the influence of which I am induced to think he over-rates;—I mean the excavating power of the sea in forming what he calls "craters of denudation." This phrase, I think, he first employed in a paper on the subject read before this Society in December 1849. It is not repeated in the latest edition of his "Principles," and I imagine, therefore, that he is no longer desirous of maintaining its propriety.

I by no means doubt, that in the case of craters formed beneath the sea, or in such close vicinity to it as to allow its waves and currents to enter and sweep round their interiors, these circumstances must have considerably modified the result. In the former case, that of subaqueous eruption, the resistance of the water above the vent would probably tend to throw off the ejected materials over a wider area. And thus, perhaps, we may account for the vast horizontal dimensions of the great crateriform basins of Italy,—Bolsena, Bracciano, Albano, and others, evidently of submarine origin. In the latter case, that of subaërial craters to which the sea has had access through some lateral opening, no doubt great degradation of their internal slopes and cliffs, as well as of the outside, will have often taken place. Many, indeed, will have had their enclosure reduced to a mere skeleton, like Santorin. Some, like Graham's Isle, have been entirely swept away. But the question being as to the *origin* of these crateriform hollows, not as to the cause of any subsequent alteration of figure, this, I believe, may in every instance, without exception, be most reasonably referred to volcanic explosive eruptions. And, therefore, the employment of such a phrase as "craters of denudation," in contradistinction to "craters of eruption," can only lead to a wrong conception of the originating forces.

Where, indeed, is to be found a crater, the formation of which cannot be accounted for (making allowance for the subsequent modifications already referred to) by eruptive phænomena of the same character as those which have before the eyes of trustworthy observers repeatedly drilled enormous craters through the axis of the cone of Vesuvius?

Is it the vast size of some craters which should render such an origin incredible in their instances? For example,—of the Val di Bué on the flank of Etna, the Caldera of Teneriffe, that of Palma, Santorini, or the external crater of Barren Island; which measure some three, five, or even six miles in diameter? But the crater of Vesuvius, formed in 1822, before my eyes, by explosions lasting twenty days, measured a mile in diameter, and was more than a thousand feet deep. The old crater of Somma, which half encircles the cone of Vesuvius, is about three times as wide as the crater of 1822. Are we, then, on that account alone, to believe that it could not have been produced by an eruption of proportionately greater violence,—when, too, such an eruption is known to have occurred about the time this crater must have been formed, namely in the year 79, and to have overwhelmed three cities at the base of the mountain beneath its enormous fragmentary ejections? Is it not, on the contrary, much more in accordance with sound philosophy to ascribe the excavation of the old concentric crater of Somma to the same cause which but the other day was seen to excavate the new crater of Vesuvius, through the heart of the same mountain, than to invent for the former a different and fanciful process? But if Somma be admitted, notwithstanding its extent, to be a true crater of eruption, the same origin cannot be denied to that of Palma, Santorini, or others, on the ground of their size, which scarcely, if at all, exceeds that of Somma.

Sir Charles Lyell seems to doubt the Val di Bué being a true crater of eruption upon two grounds. First, because the beds composing the surrounding cliffs do not show the usual quâ-quâ-versal dip, but generally slope towards the sea. This, however, is merely the result of the eruption having broken out on one side of the central axis of the mountain,—a circumstance of frequent occurrence; and naturally so, because the old central vent is apt to be sealed up by the consolidated products of former eruptions, and the point of least resistance to the subterranean eruptive force will often, therefore, be a little on one side,—probably on a fresh point of a fissure broken through the flank of the mountain.

In fact, there must be a contest between the resisting powers of the sides of the mountain and of its upper part; and the weakest part, whichever it is, will give way, and be blown up.

Sir Charles's second reason is, that a sufficient amount of conglomerates is not to be seen on the mountain-slopes around the Val di Bué, to account for the vacuity. But, besides that he himself speaks of "enormous masses of scoriæ on the flanks of Etna," it should be remembered that the aëriform explosions, when long continued, triturate the ejected matters, owing to their repeated fall into and rejection from the crater, to such a degree as to reduce the greater part at length to an impalpable powder, which is carried by the winds to a distance, sometimes of hundreds of miles, and spread in a thin layer over an enormous area of sea or land. And, moreover, the larger the dimensions of any crater, the more powerful and enduring will have been, in all probability, the explosions, and the more thoroughly triturated, during the process of its gradual enlargement, would be the fragments thrown up by them.

I remember being exceedingly surprised, after the termination of the Vesuvian eruption of 1822, forming a continual fountain of stones and ashes some miles in height, lasting through twenty days, and in the end completely gutting the mountain, to find that the prodigious amount of fragmentary matter thrown out from the crater had coated the outer slopes of the mountain only to an average thickness of a foot or two at most. But then the ashes which day by day were reduced to a finer and at length to an impalpable powder, so fine as to penetrate the closest rooms in the houses of Naples, were borne to vast distances by the winds. Much, too, was carried down into the plain, or the sea below the mountain, by the torrents of rain (producing *lave di fango*, or mud-lavas), such as overwhelmed Herculaneum, and which accompanied, as usual, the paroxysmal eruption of 1822.

Indeed, if we consider the statements adduced on good authority, of the prodigious distances to which ashes, and even large fragments of lapillo and of pumice, have been occasionally borne away from some of the volcanoes of South America and the Pacific (as, for example, in the eruption of Coseguina in 1835, and of Galongoon in 1822),—distances of more than a thousand miles (a large segment of the circumference of the globe), the whole of which intermediate space must have been strewn with them (and, in the first of these instances, it is said, to the depth of ten feet at the distance of twenty-four miles from the volcano), we may well conceive that eruptions productive of such an enormous amount of ejected matters may (nay, must) have blown into the air entire mountains of a magnitude far exceeding that of Vesuvius and Somma itself, or the bulk of matter wanting in the Val di Bué, and left in their place craters of corresponding dimensions.

Sir Charles Lyell suggests (as others have done before him), in regard to some of the largest known craters, another possible origin, which he calls *Engulfment*—that is, the subsidence of the upper part, or a large area, of a volcanic mountain into some abyss suddenly opened beneath. With respect to this supposition, without attempting to dispute its possibility, I must say that I am not aware of any such process having been ever witnessed by any credible observer so placed as to be able to distinguish between engulfment and ejection; and consequently that it were well to be cautious in admitting the occurrence of such a phænomenon, if the ordinary mode of action be sufficient to explain the facts really observed. We possess reports, it is true, of eruptions and earthquakes in Java, Sumatra, the Andes, and elsewhere, having caused the disappearance of the entire summit of a mountain, leaving a vast cavity in its place. But this is precisely the result that was observable after the eruption of Vesuvius in 1822. And in that instance we know there was no subsidence. The leading example usually adduced of such immense (supposed) engulfments is the truncation of the lofty cone of Papandayang, in Java, by an eruption in the year 1772. There, it is always said, a great area of the volcano "fell in and disappeared," swallowed up in the bowels of the earth, together with forty villages and their inhabitants. Such are the phrases usually made use of on these occasions, and very naturally so, by alarmed and unscientific observers. But recent explorers, especially Professor Junghuhn, have stated that these towns and villages of Papandayang were not swallowed up at all, but buried, like Pompeii, under the ejectamenta of the volcano; and Dr. Junghuhn, therefore, very properly refers the truncation of the mountain to eruptive explosions, rather than to subsidence.

It is, no doubt, quite conceivable, that within a volcanic mountain some internal reservoir, or subterranean lake of liquified lava, coated over by a crust of hardened rock or the accumulation of fragmentary matter, may be *tapped*, as it were, by an earthquake, and empty itself out of an aperture in the side of the mountain at a low level, leaving a cavity, which another earthquake, or the explosion of vapour and gases accumulated within it and increasing in temperature, may cause to burst, like a vast bubble,—the overlying crust of rocks falling inwards. But such a supposition is, in the present state of our knowledge, purely conjectural, and unwarranted, if, as I have endeavoured to show, the ordinary phænomena of eruption suffice to account for the formation of the largest known craters. If it is to be resorted to in any case, it would be, perhaps, in that of the very small pit-craters, occasionally met with in volcanic districts, such as the Gour de Tazana, and the lakes Pavin, Du Bouchet, and Servières in Central France. But even these show marks of explosive eruption in the scoriæ sprinkled around their banks. And the occurrence of even a single scoria is certain proof of some explosions having taken place from a body of liquid lava beneath; though, as I have said, this *may* have been accompanied or followed by engulfment. Perhaps the singular character of the crater of Kilauea, in Owyhee, may be thought to claim for it an origin in subsidence rather than eruption. It is described as a vast sudden depression in what would otherwise be almost a level plain, on the side of the gently sloping volcanic mountain of Mauna Roa. It has an irregularly oval form, from three to five miles in diameter, and is usually encircled by vertical cliffs some hundred feet high. Its bottom consists of a lake of lava, on some points (which occasionally change their situation) in continual ebullition, and at a white heat; but coated over for the most part by an indurated crust upon which it is often possible to walk. Sometimes, however, the incrusted portion is in the centre of the lake, forming a rough platform, surrounded by a circle of incandescent and seemingly fused lava,—sometimes the outer circle forms a solid shelf, within which an inner basin of lava boils at a greater or less depth below its edge. It is evident, from the interesting story of this crater given by Professor Dana, in the 'American Journal of Science,' as gathered from the relations of various observers during nearly a century past, that the surface of a vast boiling lake of subterranean lava existing here, rises and sinks at irregular intervals of several years in duration; sometimes filling the entire cavity, and even pouring over its outer margin sheets of a very liquid lava, —sometimes sinking to a depth of a thousand feet or more,—especially when some outburst from a lower vent, or chain of vents, has *tapped* the internal reservoir. But, however interesting the characteristic features of this crater, both from the facilities it affords for observation, and the great scale on which they are developed, they do not seem to me to prove the origin of the cavity other than that of ordinary craters. The phænomena of Kilauea are not so exceptional as, at first view, might be supposed. Visitors who looked down into the great Vesuvian crater for a few years after its formation in 1822, saw pools of liquid and incandescent lava at its bottom, and small cones of scoriæ thrown up by an almost constant ebullition. The difference in the violence of the explosions, and in the amount of ejected scoriæ, arises, no doubt, as Professor Dana very justly observes, from the difference in the relative liquidity of the lavas,—those of Kilauea being very liquid, those of Vesuvius much more viscid and unyielding*. So also during the Vesuvian eruption of 1753,

* Dana, 'American Journal,' 1850, vol. ix. p. 383.

persons who ventured to the summit of the cone observed jets of liquid lava thrown up from the surface of a mass which occupied the bottom of the crater, and conducted itself exactly in the manner of a liquid in ebullition. Spallanzani remarked a similar appearance within the great crater of Etna in 1788. In the volcano of the Isle of Bourbon, Bory de St. Vincent describes a source of very liquid and glassy lava ceaselessly and somewhat tranquilly boiling over in concentric waves from the summit of a dome-shaped hillock composed of its overflowings.

Circular form of Craters.—A consideration which has not, perhaps, been sufficiently adverted to by geologists speculating on the origin of volcanic craters, is the cause of their invariably circular or nearly circular figure. If I am right in attributing their formation exclusively to aëriform explosions, it follows that each is in fact simply the external orifice of a more or less cylindrical bore drilled through the pre-existent rocks by repeated discharges of highly expansive aëriform fluids (probably for the most part steam) forcing their way upwards at some weak point; and that it is to the equal pressure in all directions of the expanding fluid that the circular form of the section of this orifice is due,—the same cause, in fact, which gives a spherical form to a bubble of air or gas rising through water. Indeed the eruptive explosions must be considered as occasioned by the rise of a succession of enormous bubbles from a great depth in the fluid lava below. Each single explosion attests the bursting of such a bubble from the surface of the liquid mass of lava in the vent. In moderately tranquil eruptions these succeed each other at considerable intervals. In the case of Stromboli, I noted that about five minutes usually occurred between every two explosions. When the eruption assumes a violent character, as in the Vesuvian one of 1822, the eructations, for such they are, succeed each other so rapidly as to produce an almost continuous roar, like the blowing-off of a thousand steam-boilers. And each explosion gives birth to one of those great globular volumes of white vapour, which, rolling over and over each other as they rise in the air in a vast column, occasion one of the most remarkable and magnificent appearances of a paroxysmal volcanic eruption. In the midst of these clouds of snowy vapour, a black column of stones, scoriæ, and ashes may be seen to shoot up to a vast height, generally attended with copious discharges of electricity generated by the friction of the ejected fragments, and forming a singular contrast to the jet of aëriform matters.

In some rare cases it is possible to witness the actual rise and bursting of these great bubbles of vapour. Spallanzani on his visit to Stromboli in 1780 saw the liquid surface of lava at a white heat within the orifice of the volcano surge alternately upwards, and after bursting like a great bubble, fall back again out of sight. In 1819 I was myself able to witness the same interesting phænomenon probably from the same position, a high point of the external crater-rim which overlooks the vent. At each belch, a shower of tattered fragments of lava, torn from the surface of the bubble as it broke, rose into the air with a cloud of vapour and a fierce roar; while steam seemed to be at intervals blowing off from another neighbouring vent. Hoffman, who visited the same volcano a few years later, describes in minute detail precisely the same phænomena.

The vast size of some craters, already noticed, may afford a notion of the enormous volumes of gasiform matter that must have been discharged through them at the time of their formation by continuous explosions lasting for weeks and even months; since each individual bubble of vapour must have been of a magnitude to fill the entire horizontal section of the crater; and even for some time to aid in enlarging the area of this aperture by violent pressure against its rocky sides. The prodigious force with which they ascend, and therefore the great depth at which they are generated, may be judged from the vast vertical height, measured in miles, to which they have been seen to shoot up a continuous columnar fountain of ejections, consisting not merely of scoriæ and ashes, but often of rocky fragments of great size.

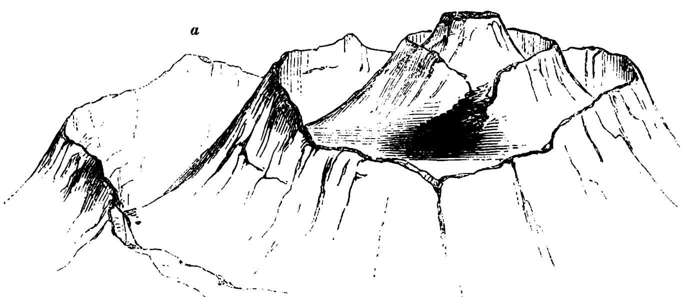
These, by their mutual friction, as they alternately fall back and are thrown up again, become, as already has been said, greatly comminuted; and the source of the explosive vapours having sooner or later exhausted its energies, the accumulation of these ashes in the vent at length appears to stifle their further development, and quiescence for a time ensues. [I am speaking here, of course, of the case of such a paroxysmal eruption as I had the advantage of witnessing in 1822.]

I have said that every crater is more or less circular in figure; but, since the orifice of discharge will almost necessarily be opened on the least resisting point of some fissure broken through the solid preexisting rocks, we might expect its section to be often lengthened in the direction of this fissure, and consequently to be rather oval than strictly circular. And this expectation is justified by observation. Sometimes two orifices have been opened upon the same fissure so near together that their craters or cones intersect each other. In the range of Puys of Auvergne and the Velay such examples are frequent. And in the eruption of 1850 of Vesuvius two craters were formed on the summit of the cone divided only by a narrow ridge; their common horizontal axis coinciding with the line of the great fissure, which in the preceding year had been visibly broken through the side of the cone towards the north-east. Sometimes aëriform explosions take place from openings upon lateral fissures, and produce those minor, or (as they are often called) parasitic cones, of which several examples occur on the flanks both of Vesuvius and Etna. At other times, the explosions are confined to the central vent of the volcano, the lava alone welling out, perhaps, at some lateral orifice. This, indeed, is the normal character of these phænomena. And it is this habitual predilection (as it may be called) of volcanic eruptions for the same identical vent, that occasions in so many instances the heaping up of some vast mountain mass above and around it, subject to the occasional blowing up of the central portion, to be re-formed again and again by subsequent eruptions.

The result of the irregular alternation of these paroxysmal explosions and subsequent gradual expulsions of new matter is the appearance, so common in volcanic mountains, of a minor and central cone with its crater, rising within the circumference of some larger crater of earlier date, or in its immediate vicinity. The walls of the latter crater are of course often broken down on one or more sides (generally on the line of the original fissure);—perhaps reduced to a mere segment of its original circuit, by the combined operation of volcanic convulsions and aqueous erosions. Whoever will take the trouble to examine carefully an accurate map, on a sufficiently large scale, of almost any volcanic district (such, for example, as Vesuvius and the Phlegræan Fields, Etna and the Lipari Isles, the Roman Territory, the Grecian Archipelago, Madeira, Teneriffe, the Azores, Bourbon, St. Helena, Barren Island, the Leeward Isles, &c.), will see numerous unquestionable examples of this law by which crater is formed within crater, and new cones upon the ruins of old ones.

History of Vesuvius.—At the risk of repetition, I must be permitted to illustrate this law by the trite, but instructive, example of Vesuvius,—which only comes so often before us because from its proximity to Naples it has been open to more constant and accurate observations than any other known volcanic mountain. What, in brief, is the history of this volcano during the last century? Precisely one hundred years ago, in the year 1756, Vesuvius possessed no less than three cones and craters, one within the other, like a nest of boxes, besides the great encircling crater and cone of Somma (fig. 1). Sir W. Hamilton gives us a drawing of its appearance in this state.

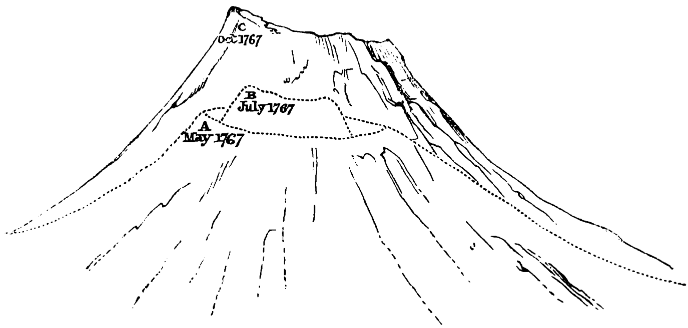
Fig. 1.—*Outline-sketch of Vesuvius as it existed in* 1756. (After Sir W. Hamilton.)



a. Somma.

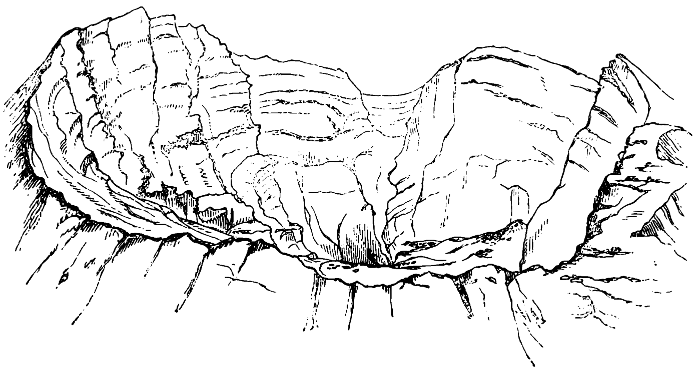
By the beginning of the year 1767, the continuance of moderate eruptions had obliterated the inmost cone and increased the intermediate one, until it very nearly filled the principal crater (fig. 2, A, B). An eruption in October of that year, 1767, completed the process, and re-formed the single cone into one continuous slope all round from the apex downwards (fig. 2, c). The dotted lines in fig. 2 (after Hamilton) represent the shape of the outer and inner cones before this eruption, and the space between them and the firm outline represents the amount by which the cone was in the intervening

Fig. 2.—*Outline-sketch of Vesuvius as it appeared in October* 1767; *with dotted outlines of its form in July and in May of the same year.*



ten years augmented in bulk and height by the ejectamenta of that eruption. An interval of comparative tranquillity followed, until, in 1794, the paroxysmal eruption occurred, described by Breislak, which completely gutted this cone, then solid, lowered its height, and left a crater of great size bored through its axis. Later eruptions, especially that of 1813, not merely filled up this vast cavity with their products, but once more raised the height of the cone by some hundred feet. When I first saw it in 1819 the top formed a rudely convex platform, rising towards the south, where was its highest point. Several small cones and craters of eruption were in quiet activity upon this plain, and streams of lava trickled from them down the outer slopes of the cone. So things went on until October 1822, when the entire heart of the cone was again thrown out by the formidable explosions I have so often referred to, and a vast crater was opened through it; while the cone itself was found to have lost several hundred feet from its top. In fact, nothing but an outer shell of it was left (fig. 3). Eruptions, however, soon recommenced. In 1826–7 a small cone was formed at the bottom of the crater, and, continuing in activity, had reached a height which rendered it visible from Naples in 1829, when of course it must have nearly filled up the crater. In 1830 it was 200 feet higher than the crater's rim; and in 1831 this cavity was completely filled, and the lava-streams began to flow over it down the outer cone. In the winter of that year a violent eruption once more emptied the bowels of the mountain, and left a new crater, which soon began to fill again from ejections upon its floor; and by the month of August 1834 this crater had been in its turn obliterated, and lava overflowed its edge towards Ottaiano. In 1839 the cone was again cleared out, and a new crater appeared in the shape of a vast funnel, accessible to its bottom, which for a few years then remained in a tranquil state. In 1841, however, a small cone began to form within it, and increased so rapidly, that in 1845 it was visible from Naples above the brim of

Fig. 3.—*Crater of Vesuvius after the Eruption of October* 1822.



the crater, which soon after was completely filled. And the cone from that time went on increasing in bulk and height from the effect of minor eruptions, until in 1850 one of a violently explosive character opened the two deep craters on its summit, of which I have already spoken. The more recent eruption of May last, being confined chiefly to a prodigious efflux of lava from the outer side of the cone, unaccompanied by any extraordinary explosive bursts from the summit, has not altered materially the form impressed upon it in 1850.

It is thus seen that within the last 100 years the cone of Vesuvius has been five several times gutted by explosive eruptions of a paroxysmal character, viz. in 1794, 1822, 1831, 1839, and 1850; and its central craters formed in this manner as often gradually refilled with matter, to be again in due time blown into the air. Meanwhile the old external crater of Somma is itself becoming choked up by the accumulation of all the lava-streams and fragmentary matter that are expelled towards the northern and outer side of the cone. It would be, therefore, in exact accordance with the habit of this volcano (as of volcanic mountains in general), if, after some further period either of quiescence or of moderate activity, the entire cone of Vesuvius should be blown up by a more than ordinarily violent paroxysm, and the crater of Somma itself reformed.

With this well-authenticated history of the mountain within our knowledge, would it not be wholly unphilosophical to deny (except upon such grounds of impossibility as have never been adduced) that the larger containing crater in the case of Vesuvius (and the argument applies to other similar volcanic mountains) had the same origin as the smaller contained ones; and that the external cones were produced in the same manner as the internal, and similarly constituted ones? And therefore those who refuse to believe the former to be of eruptive origin must be prepared to extend their incredulity to the latter. Indeed the elevation-crater theorists usually do not shrink from this consequence. With them the cone of Vesuvius, and that of Monte Nuovo itself, were not the products of eruption, but of elevatory expansion by a single shock. Obviously, it ought to follow, that no volcanic mountain was ever in eruption at all, that the whole is an ocular illusion; at least, that the lava-streams we see pouring for weeks and months from the summit of a cone and hardening there, and the enormous showers of fragmentary matter which, during equally long periods, we see thrown up from the crater and falling on the surface of the cone, do not, even in the lapse of ages, add to its bulk or tend by their frequent repetition to compose the substance of a volcanic mountain, but, by some unaccountable process, disappear without leaving a trace behind. I own that, to my mind, such an hypothesis is wholly unintelligible. I see in the ordinary phænomena of a volcanic mountain, such as I have described them in the brief record of the principal eruptions of Vesuvius during the last century, a simple and natural process by which such a mountain is gradually built up; and, having observed this mode of formation going on in some instances before my eyes, I think it reasonable to apply it to explain the mode of formation of other mountains of the same class, with their cones and craters, old and new, central and lateral, or parasitic; and making allowance, as I said at first, for a certain amount of internal accretion and elevation, by means of intrusive dykes and earthquake shocks, I know nothing in the appearance, figure, or structure of any volcanic mountain yet discovered, which such an ordinary and observed mode of formation will not account for.

II. *The nature of the liquidity of lavas.*—So much for that branch of my subject,—the formation of cones and craters. I wish now to ask attention to some circumstances respecting the mode of emission and nature of the lavas that proceed from them. I have already spoken of the comparatively tranquil manner in which some lava-streams are seen to well out from the flank of a volcano, or its summit, and the probability that differences in the liquidity or viscosity of the heated matter at the time of its efflux may occasion corresponding differences in the character of the phænomena. Observation confirms this expectation; and it has been remarked, that the very liquid and vitrified lavas, such as those of Kilauea and Bourbon, are poured out more or less tranquilly without any very violent explosions, their imprisoned vapours evidently escaping with comparative ease, while the more viscous and ultimately stony lavas, possessing a minor degree of liquidity, and consequently not allowing so easy a passage to the vapours that rise through, and struggle to escape from them, are protruded with fiercer explosive bursts, and the ejection of far greater quantities of scoriæ and other fragmentary matters.

This observation, coupled with other reasons to which I shall presently advert, led me to an opinion expressed in the works above referred to, that the ordinary crystalline or granular lavas (making exception of the vitreous varieties), although at a white heat at the moment of their emission from a volcanic vent, are not in a state of complete fusion; that a large proportion, at least, if not all, of the crystalline or granular particles of which, when cooled and consolidated, they appear composed, are already formed and solid, their mobility being aided by the intimate dissemination through the mass of a minute but appreciable quantity of some fluid,—in all probability water,—which is prevented from expanding wholly into vapour by the pressure to which it is subjected while within the volcanic vent, or in the interior of the current, until that pressure is sufficiently reduced to allow of its expansion in bubbles, or its escape through pores or cracks, by which it passes into the open air from the surface of the intumescent lava.

I was strengthened in this opinion by several concurrent considerations:—

1. If all lavas are (as they are usually supposed to be) in a state of complete fusion when they issue from a volcano, how is it that they do not all present the same glassy texture which is seen in some, the obsidians, pitchstones, and pumiceous lavas especially, and in the ropy, cavernous, filamentous basalts of Kilauea, Iceland, and Bourbon, and which these very crystalline and stony lavas themselves put on when melted under the blowpipe or in a furnace? The usual answer is, that the granular and crystalline texture is acquired subsequently to emission by slow cooling; and the experiments of Gregory Watt and Sir James Hall are cited in support of this assertion. In the present day, probably the process by which Messrs. Chance and Co., of Birmingham, devitrify a mass of fused basalt (from the Rowley rag, near Dudley) by causing it to cool slowly in an "annealing furnace," would be considered as a strong confirmatory fact.

But there is no fact more certain than this, that the superficial portions, at least, of a lava-current flowing in the open air, do not cool slowly. On the contrary, they are rapidly, I might say instantaneously, upon their exposure, consolidated and cooled down to a temperature which permits them to be handled and even walked upon without damage. How is it that this scoriform crust, or the solid cakes and slabs which so instantly form upon every exposed surface of lava, nay, even the scoriæ which are tossed up in a liquid state by the eruptive jets, and harden while yet in the air before they fall, exhibit on fracture no glassy texture, but much the same earthy or stony grain, and occasionally crystals of considerable size in the solid matter separating their cellular cavities, as is found in the interior of the current which is known to have cooled very slowly? How is it that some lava currents are stony throughout, others vitreous throughout, as, for example, some of the large pumice-streams of Lipari, Iceland, and the Andes?

I have recently visited the manufactory of the Messrs. Chance, at Oldbury, near Birmingham, for the purpose of examining the mode in which the basalt used there (and which is the same upon which Mr. Gregory Watt experimented) conducts itself in their furnaces, and I found, that when the liquid and fused contents of a furnace at a white heat is poured out upon a brick or other floor into the open air, so as to represent a stream of lava flowing out of a volcanic vent, it consolidates throughout, whatever its bulk, into a homogeneous and purely vitreous black obsidian, in fact, an absolute glass, with a conchoidal fracture and sharp cutting edges. It is only when made to consolidate very slowly in an oven kept at a high temperature for some days, that it assumes the deadened and semi-crystalline texture of the manufactured article.

If this process be interrupted, it is found to have commenced by the formation, at numerous points within the vitreous mass, of globular concretions about the size of a small pea, of a lighter colour than the base, and having a pearly lustre and radiated structure. The multiplication and confusion of these crystallites or sphærulites ultimately destroy the glassy character of the substance altogether, and give to it a pearly semi-crystalline texture, without, however, restoring the far more crystalline aspect of the basaltic rock. A similar change may be often observed to have taken place in nature among the vitreous lavas, which pass into pearlstone and pitchstone by the formation of the same kind of sphærulitic concretions, and of course there is no question as to the complete state of fusion in which such lavas have been produced. But there is no trace of such a process in any of the ordinary earthy, and stony or crystalline and porphyritic lavas. I am not aware of a single current from either Etna or Vesuvius having ever exhibited, even on its most rapidly cooled surfaces, any passage into true obsidian, or sphærulitic pearlstone, or any portion of such vitrifactions. A pellicle, or glaze, of a semi-vitreous appearance coats the surface in some parts, or lines the cellular cavities; but it seems evident that the bulk of the matter could not have been at the time of its emission in that thoroughly fused condition which it assumes when melted in a furnace or under the blowpipe.

2. It struck me that temperature does not alone determine the fusion or liquefaction of substances; and that compression may prevent the liquefaction of a solid at a high temperature, just as it prevents the vaporization of a liquid, in the common experiment of boiling water at a lower temperature in a rarefied atmosphere. If so, the intense pressure to which heated lava must be subjected before it rises from the bowels of the earth to discharge itself on the surface, intensified by the reaction of its own expansive force from the confining surfaces, might perhaps prevent its complete fusion, however high the temperature.

3. I had long been impressed by the vast volumes of aqueous and other elastic vapours evidently discharged from every volcano in eruption, and to all appearance the chief agents in the expulsion of lavas from the bowels of the earth. That this vapour is liable to be developed in every part of the mass of lava is shown by the formation of vesicles throughout its substance wherever the pressure is so

reduced as to permit their expansion; for instance, in the superficial portions of a current; and in some lava-currents throughout the entire mass.

The experiments of Mr. Knox, related in a paper read before the Royal Society in 1824 *, had taught me that water in an appreciable quantity is mechanically combined with the elementary particles of all the crystalline rocks of igneous origin. The question, therefore, arose,—Might not the water thus intimately disseminated through a mass of crystalline lava, although at an intense temperature, remain unvaporized, owing to the still greater intensity of the pressure by which it is confined while yet within the bowels of the earth? and would it not under these circumstances exert an intense expansive force upon all the confining molecular or crystalline surfaces between which it lies, and thus occasion a tendency to separation among these solid particles whenever the compressing forces were relaxed, or the temperature increased sufficiently, so as to give a certain degree of mobility to these particles *inter se*, and an imperfect liquidity to the mass composed of them? And, supposing the intumescence thus occasioned to raise any portion of this semi-liquid matter into the open air, would not the instantaneous absorption of caloric from the contiguous particles, that must accompany the vaporization of this water, and its escape in bubbles or pores and through cracks, owing to the nearly absolute cessation of pressure, account for the sudden cooling down and *setting*, or consolidation, of the exposed surfaces, without having undergone complete fusion (except in the case of mere superficial films), notwithstanding their previous intense temperature, amounting even to a white heat?

This supposition seemed to me to account satisfactorily, not only for the absence of a vitreous texture even in superficial portions of many lava-streams, and their instantaneous consolidation on exposure, in cellular or porous slabs and cakes, but also for several other characteristics of igneous rocks, not easily to be reconciled with the idea of their having always issued from the earth in a state of absolute fusion; such, for example, as the cracked and 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 their longest axes in the direction of the bed of the rock, that is, of the movement of the lava when liquefied; the finer grain often exhibited towards the tail or extremity of a current than at its source, the brecciated lavas which appear to have enveloped fragments in great number of the same material without any fusion even of their finest angles. So also might be explained the more or less spongy, porous, and loosely crystalline texture of many trachytes, and their disposition in thick beds or dome-shaped bosses, attesting their protrusion in a very imperfect state of liquidity, more resembling the intumescence of some kinds of dough in an oven than the fusion of metal in a furnace.

And here let me remark, that Dr. Daubeny, and some other writers on volcanic phænomena, have spoken of the vesicles or air-

* Phil. Trans. 1825.

bladders in lavas, as being proofs of their having been in a state of complete fusion. But have the loaves baked in our ovens been in fusion? The comparison of a cellular scoria with a loaf or a French roll will show that vesicles of precisely similar appearance to those of lavas are producible in substances of a pasty consistence, which owe their liquidity to an aqueous vehicle, the heat applied being only sufficient to develope the contained gases. Other kinds of baked cakes are porous rather than cellular, and aptly represent the texture of the earthy and porous trachytic lavas.

Plutonic rocks.—This theory as to the nature of the liquidity of many lavas appeared to me so reasonable, that I proceeded to examine its applicability to the still more generally crystalline plutonic rocks, from the alteration of which by heat lavas are usually supposed to derive. I asked myself, what would probably be the effect on a mass of granite, for example, containing water intimately combined with its molecular particles, and confined beneath overlying rocks and seas, under circumstances of intense compression, and at the same time high and increasing temperature? Surely a tendency to intumescence, which, wherever, and in proportion to the extent to which, it takes place, must elevate and fracture the overlying rocks, and likewise disintegrate more or less the crystalline particles of the swelling mass, through the irregularities of their internal movements and mutual friction. Many of the crevices broken through the neighbouring rocks would be injected by the intumescent matter. Some may be sufficiently enlarged to allow of its forcing its way into the open air as a lava, perhaps accompanied by eructations of the gases and vapours developed in the lower parts of the mass, or, should the liquefaction not be sufficient to admit of the rise of aëriform bubbles, as matter of a porous, pasty, or glutinous consistency, perhaps even semisolid in texture and bulky in form.

It might happen that, circumstances occasioning in turn the preponderance of the compressing over the expansive forces (by reason, for example, of a diminution of temperature), portions of the subterranean crystalline mass will, after a partial intumescence of the kind supposed, return to a state of solidity. The result may be a more fine-grained rock, owing to the partial disintegration of the crystals; or, if the disintegration had proceeded sufficiently far, new mineral combinations might take place. Indeed, Watt long since proved that the particles of even apparently solid rocks are capable, through changes in temperature, of internal motion sufficient to admit their rearrangement according to polarity, that is, of crystallization. Still more likely is this result to occur on the condensation or escape of any fluid which had previously kept them from contact with each other, since the crystalline polarity can only exert itself within minute distances. And thus might be accounted for the frequently observed passages of granite and gneiss into syenite, greenstone, trap, or trachyte, and the varieties of mineral composition which these rocks at times exemplify. So also the transitions from the larger crystalline grain to the finer, and the dykes and veins which these rocks so often contain themselves, or intrude into their neighbours. So too the finer grain of the sides, or selvages, of such dykes might be owing to the greater disintegration of the crystals by friction along these sides as the matter was driven through them.

Another problematical fact which this theory of an aqueous vehicle in heated granite would account for, is the usual appearance of the quartz in this rock, not in crystals, but as a paste or base, seeming to be moulded upon the crystals of felspar. Had the rock crystallized from a state of fusion, the felspar, being far more fusible than quartz, might have been expected to be the last, not the first, to crystallize. But if the water disseminated through the rock were supposed to have taken the quartz into solution by aid of the alkalies present in the felspar, the fluid vehicle would in fact become a liquid or gelatinous silicate; and upon consolidation would naturally mould itself on the felspar crystals, or appear as a paste to them. I adduced the hot siliceous springs of Iceland and other volcanic districts as proofs that heated water under such circumstances could dissolve silex.

Those who will take the trouble to refer to the 2nd, 4th, 5th, and 6th chapters of my 'Considerations on Volcanos,' will see that the above is a brief summary of the arguments there put forth, perhaps at too great length, and in a form which may have hindered their obtaining at the time of their publication the attention which I believe they merited.

Certain it is, that they were at that time, now thirty years back, neglected, or generally discredited. I was told that my views were "unchemical." I was represented as asserting incandescent lava to be "cold or thereabouts" *. The igneous and the aqueous origin of certain rocks had been so hotly contested, and fire and water were usually considered so antagonistic, that it seemed at first view an absurdity to imagine that both could be combined in a substance seemingly in fusion. Probably also the idea was scouted at first through the notion that water could not be present within an incandescent mass of lava without causing it to explode like a mine; which might of course be the result of any considerable body of water being localized at one point. But the view I entertained, as has been explained, was that the water (and to some extent, perhaps, liquefied gases), to which I attributed much of the liquidity of some lavas, was disseminated throughout its mass, occupying minute interstices, and in intimate, though probably mechanical, combination with every molecule,—indeed intercalated between the plates even of its solid crystals; and moreover that the pressure to which the rock was subjected while beneath the earth was so enormous as to prevent the vaporization of these minute portions of liquid anywhere except at points where the intensity of temperature and consequently of expansive force overcame the resisting forces, and thereby caused either the formation and rise of great bubbles of vapour from the lower depths of the subterranean lava-mass, or the inflation of minor bubbles and pores throughout it, or at least in the superficial portions which by intumescence were forced into the open air.

Of late, however, views precisely in accordance with the theory

* Westminster Review.

printed by me in 1824 have been put forward, and have attained extensive adhesion among continental geologists.

M. Delesse has proved by experiment the solubility of the silex of rocks in heated water containing either of the mineral alkalies. And, indeed, the manufacture of artificial stone is now carried on in this country (Messrs. Ransom's process) by saturating loose sand with an artificial hydrate of silica. Huge blocks of flint, I understand, are thrown into the hot alkaline water, and melt down like so much sugar.

Again, the experiments of Boutigny have shown that water at a white heat remains unvaporized, in the form of spheroidal globules, in which form it is obvious how readily it would communicate mobility to the solid particles among which it was entangled; and how (according to these experiments) it might flash into bubbles of vapour on the reduction of its temperature by exposure to the air.

M. Deville, in his recent observations on the vapours disengaged from Vesuvius since the eruption of May in last year (for the perusal of which I am indebted to the kindness of my friend Dr. Daubeny), arrived at the conclusion, to use his own words, that "water in the proportion occasionally of 999 per mille must have formed an integral part of the Vesuvian lava at the moment of its emission; and consequently, that in the interior of the incandescent lava there is such an arrangement of molecules, as to permit the gaseous and volatile matters to remain there imprisoned, until in the progress of cooling and consolidation, they evolve themselves."

Above all, M. Scheerer, of Christiania, the eminent Norwegian geologist, who is better acquainted perhaps than any other with the granites of that country, published in 1847 a theory, which, he says, his observations had suggested to him in 1833, on the production of granite, entirely identical with that which I had ventured to suggest in 1824–25. I take the following account of it from the paper read before the Geological Society of France in 1847, and published in the fourth volume of the Bulletin de la Soc. Géol., p. 468.

M. Scheerer attributes what he calls the "plasticity" of granite when protruded on or towards the surface of the earth (a condition evidenced by the veins it throws into the fissures of neighbouring rocks) to the combined action of *water* and heat. He describes the water as "intercalated between the solid atoms of the crystalline and other constituent minerals, endeavouring to escape by its tendency to vaporization, and consequent elasticity, but unable to do so owing to the pressure to which the enclosing mass is subject." He considers the water so contained in granite to be "primitive," that is, one of the original bases of the rock, and not the result of infiltration. He attributes to it the solution of the quartz, aided by the alkali, and the consequent moulding of this mineral on the felspar-crystals. He even goes the length of styling the condition of granite before its protrusion by the term "une bouillie aqueuse," *a granitic broth.*

These theoretical opinions of M. Scheerer appear to have received the assent of M. Elie de Beaumont and other French geologists*.

* See Bulletin de la Soc. Géol. France, new series, vol. iv. p. 1312.

Their exact comformity with those which were first developed in my treatise on Volcanos, published 1824–25, and repeated in the Preface to my volume on Central France in 1826–27, will be evident to any one who will take the trouble to refer to those works.

It is not, however, for the vain purpose of claiming a priority in these views, that I now ask the attention of the Society to them, but because the subject has not, I think, yet attained the consideration it deserves from the geologists of this country; and especially because of its leading, if followed out, to further inferences of considerable importance, which were likewise suggested by me in 1825, but have been hitherto only partially pursued to their legitimate consequences.

Laminated or schistose rocks, slaty cleavage, and folded rocks.—I refer to the mechanical changes in the texture and structure of the plutonic rocks which could not fail to have resulted from the mutual friction of the component crystalline particles attendant on their internal movements, whether caused by mere dilatation and re-compression in place, or by a shifting of the entire mass in any direction, under intense and opposite, but irregular pressures.

I was led to reflect on this by observation of the ribboned pitchstones of Ponza and Ischia, in which, while in a state of vitreous fusion, crystallites had formed (just like those of the Oldbury obsidian), and subsequently been broken up by the movement of the semi-liquid mass, and drawn out into long stripes, giving a ribboned appearance to the rock.

Further examination proved to me that the ribboned trachytes of Ponza and Ischia, and some ribboned clinkstones, owed that character to a similar elongation of the felspar crystals and felspathic particles which they previously contained, in the direction in which the semi-liquid mass flowed, or rather was forced to move, and in which the pores or cells, when there are any, are equally elongated. These observations suggested to my mind the reflection that the solid particles of any crystalline rock which is put in motion while in a state of imperfect solidity, and under the influence, of opposing pressures, must be subject to a great amount of mutual friction or disturbance, by which their final arrangement when wholly consolidated will be determined.

Thus suppose a mass of granite, of which A B (fig. 4) represents

Fig. 4.

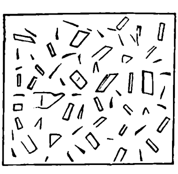
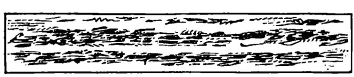


Fig. 5.



the section, consisting of crystals of felspar and mica irregularly disposed in a basis of more or less liquefied or gelatinous silex, exposed to movement in the direction A B, while under vast pressure both from above and below, that is in the opposite directions C and D. Whether the surface, C, or D, or both, remained fixed, or merely moved, owing to resistances, at a slower rate than the other parts, the crystals in the latter would be turned round by internal friction, and rearranged and drawn out in stripes or planes in the direction of the motion, while the proportionate dimensions of the mass would be equally varied so as to produce a section something like E, F, G, H (fig. 5), in fact, a rock which, if no further change occurred in it except consolidation in place, would have all the characteristics of *gneiss*. The same movement, if still further continued, might, it appeared to me, be expected to disintegrate the angular crystals of felspar altogether, so as to cause them to disappear, perhaps to force their elementary molecules to melt into the intensely heated silicate, to which they would impart their alkalies. And the resulting rock, supposing the laminæ of the mica-crystals to slide readily past each other, when lubricated by the silicate, and not therefore to be so far disintegrated as those of felspar (as from their peculiar form might be expected), would put on a lamellar structure, and very much resemble *mica-schist*,—especially since the great flexibility of the mica would render its laminæ extremely liable to yield to the irregularities of pressure pervading the mass, in a variety of directions, and consequently to take such wavings and contortions as are often exemplified in that rock. Whoever will examine the tortuous way in which the plates of mica envelope and bend round nodules of half-melted quartz or crystals of garnet in mica-schist, will be convinced, I think, that the whole mass has been subjected to great internal movement and consequent friction in the direction of the layers of mica, while under intense pressure, and in a comparatively softened state, the mica being lubricated, as it were, by a vehicle of liquid or gelatinous quartz. Whatever fissures or cracks were formed during this movement in the semi-solid rock, or subsequently, so long as the silicate remained unconsolidated, would be necessarily filled by it, and ultimately appear in the shape of the quartz-veins so frequent in this class of rocks.

Under this supposition gneiss and mica-schist would bear the same relation to granite as the ribboned trachytes and schistose lavas (clinkstone) to ordinary crystallized or granular trachyte; and the quartz-rocks associated with granite, represent the quartzose trachytes of Hungary, Ponza, and the Andes.

These views, developed by me in 1825, I cannot but think, deserve the attention of geologists engaged in investigating the origin of the so-called "plutonic" and "metamorphic" rocks. It seems to me more probable that some process of this kind may have metamorphosed granite into the laminated rocks of plutonic origin, gneiss, and mica-schist, than that these rocks should have been formed by the mere fusion and reconsolidation or crystallization in place of sedimentary strata *already laminated*, according to the usual "metamorphic" doctrine. I can understand the clay-slates and other fine-grained schists to have been formed through the mechanical disintegration of mica-schist, but not mica-schist by the baking or melting and cooling of the clay-slates in place, in the manner suggested by Sir C. Lyell.

In the formation of the clay-slates, perhaps, the action of heat was not concerned (except as engendering the pressure to which they have evidently been subjected), but that of water or an aqueous silicate only. Still in their case also internal movements and mutual friction of the component particles under extreme and irregular opposing pressures have, I am convinced, had a primary influence in occasioning that parallel arrangement of the scaly and flaky micaceous particles to which their slaty cleavage is due. This, at least, was the conviction forced upon my mind by a close examination of the fissile clinkstone of the Mont Dor and Mezen, which is used for roofing-slate, and is in its lamination and cleavage undistinguishable from many clay-slates. And that opinion I recorded at the time in my 'Considerations on Volcanos *.'

I have since found this view of the origin of slaty cleavage supported by Mr. Darwin in his work on 'Volcanic Islands,' and by Mr. Sorby in his paper on slaty cleavage in the Edinburgh Philosophical Journal for 1853. I need not say that such support affords strong confirmation of its correctness.

Of course we are led to connect the movements under extreme pressure, to which this peculiar texture of the laminated rocks is here attributed, with the action of those same forces by which their beds have been so generally bent and contorted into a series of folds or wrinkles, more or less at right angles to the general strike.

If we seek to discover under what circumstances these flexures were brought about, we can hardly be wrong in ascribing them to the same violent process by which they have been elevated, usually on the flanks of some protruded ridge or enormous dyke of crystalline rock, which is seen to form the axis of the mountain-range to which they belong.

Now what may we suppose to have been the character of this elevatory process?

The phænomena of active volcanos, and the protrusion of intumescent crystalline matter on so many points of the earth's surface, and at all periods of its history, may be admitted to prove the continued existence beneath a very large area of that surface—if not the whole—of a mass of intensely heated crystalline matter, having disseminated throughout its substance (in the manner already dwelt upon) some fluid or fluids, such as water, affording an imperfect liquidity to the mass, and, by its intense elastic force, communicating to it a powerful tendency to expansion. Now suppose any considerable diminution to occur locally in the amount of pressure confining this expansible mass beneath the crust of the globe,—such as might be brought about by any extraordinary concurrence of the ordinary barometric, tidal, oceanic, or excavating causes (not to suggest others),—or, on the other hand, any considerable increase of its expansive tendency, owing to a local increase of temperature, from some

* See pp. 103, 144, and 202.

unknown, but easily imagined, cause,—we should anticipate, as the necessary result, the violent fracture and elevation of the overlying crust of rocks, and the extrusion through some principal fissure, or line of fracture, of a ridge of the subterranean intumescent crystalline matter.

It seems very probable that under such circumstances the central axis of the protruded ridge may retain its irregularly crystalline grain and structure, but that the portions of crystalline matter that from either side would rush or be thrust up by pressure from behind (consisting partly of the weight of the overlying rocks on the semi-liquid matter below them) towards the opening should be subjected to so much internal friction of their crystalline particles, and so much pressure at right angles, or nearly so, to the direction of the movement, as must stretch and draw them out into parallel planes,—just as happened evidently to the striped and ribboned trachytes in the protruded dykes of Ponza and Palmarola. This friction and pressure would be extreme, of course, along the lateral parts of the protruded mass, that is, the selvages of the great dyke; which, if the original mass were granite, would thus appear composed of an axis of granite, passing on either side into gneiss (or squeezed granite) and further on into mica-schist.

But every irregularity, whether on the large or the small scale, obstructing more or less the even motion of the layers, must create a waving or contortion in them, especially in the planes of slippery mica-plates, such as is exemplified even in hand-specimens of the Ponza trachytes, and also on the largest scale in the same locality. And the extreme irregularities of motion, occasioned on the upper layers of the intumescent mass by the pressure and resistance of the overlying beds, may be expected to carry their wavings still further, and at the throat of the fissure where the squeeze and jam of the protruded matters must be at its maximum, to occasion those enormous and repeated zigzag foldings of the laminated beds, so frequently observed in mica- and chlorite-schists in such positions.

Meantime another influence would be similarly affecting the overlying stratified rocks above, or on the outer flanks of the elevated axis, namely their own specific gravity, urging them to slide or slip laterally when tilted up at (perhaps) a considerable angle on either side. The more compact and indurated strata would be partly fractured into cliffy masses, partly broken up into breccias and conglomerates by this movement; but the softer beds, especially those which were saturated with water (perhaps even yet under the sea), or which contained interstratified beds of silt, shale, or clay, permeated with water, would glide laterally away from the axis in extensive land-slips, and be wrinkled up into vast foldings under the intense pressure compounded of their own weight, and that perhaps of portions of the protruded matter thrust against them,—in a manner very similar to the contortions produced in the more crystalline laminated rocks by the violent squeeze which accompanied *their* protrusion. It may even be difficult to draw a line between the effects of these two replicating and fracturing forces. But, together, they seem to me sufficient to account for most of the phænomena of the kind observable in mountain-chains.

These were the ideas on this subject which I endeavoured to develope, though very imperfectly I am aware, in the more theoretic portion of my work on volcanos, so often referred to, and they were illustrated by a rude ideal section of an elevated mountain-chain in the frontispiece to the volume. I still think they will be found a not improbable solution of this the greatest problem in the dynamics of geology. It appears to me, that the results would be much the same, whether we suppose this elevatory action to have been *paroxysmal* and simultaneous, or gradual, taking place by minor and successive expansive throes or shocks, or even still more slowly in the manner of a *creep*, as Sir Charles Lyell would probably conceive it to have operated, and to be still continuing. On these last assumptions, the earthquake-shocks which certainly accompany at present every effort of elevation, and appear to be propagated in waves through the substance of the earth's crust, in directions usually at right angles to the principal axes of elevation, or fissures of crystalline protrusion, may indicate the force by which the extreme replications and slaty cleavage of the laminated beds are occasioned.

I would ask of geologists to consider whether such a mode of protrusion of the laminated crystalline rocks and of the lateral replication of the more earthy schists and marine strata, as is here suggested, does not accord with the general facts known respecting their position? Let me take two descriptions of the general position of the crystalline rocks from two writers of experience, judgement, and wholly impartial character, as respects the theory here indicated. Mr. Evan Hopkins* gives as the results of his extensive mining experience in the Andes and elsewhere, "that the great base [of all mountain-chains] is below more or less granitic, strongly saturated with mineral waters, and that this passes upwards by insensible gradations from a crystalline homogeneous compound into a laminated rock, such as gneiss, and still higher up into schists in vertical planes; the peculiar varieties of the higher rocks depending on the mineral character of the 'parent rock' below; the schistose rocks forming, in short, the external terminations of the great universal crystalline base,"—that is to say (as I would phrase it), the squeezed out, and therefore laminated, upper and lateral portions of the inferior crystalline mass.

Mr. Ruskin, in his recently published volume, having closely examined the structure of the Alps with the eye of a geologist no less than of a painter, but certainly without any theory to support, declares that the central axes of "irregular crystallines" (as he calls the granitic rocks) uniformly graduate on either side into the foliated or "slaty crystallines," *i. e.* into gneiss and ultimately mica- and chlorite-schists.

One point observed in the structure of the Alps and many other

* Quart. Journ. Geol. Soc. vol. xi. p. 144.

mountain-chains I may notice before I conclude, namely the occasional dip of the elevated strata towards the central axis of extruded crystalline rock, producing a synclinal, instead of an anticlinal, ridge. Another section copied loosely in the frontispiece to my work on volcanos, from Von Buch's paper on the Tyrol, may show the mode in which I conceive this to have occurred through the injection of a mass of crystalline matter into a wedge-shaped fissure, opening downwards; such as must have frequently occurred among the fractures of the overlying strata—giving occasion in some cases to the further rise of the heated and intumescent matter into the hollow between the outer slopes of the synclinal valley. It would indeed accord with the theory suggested above, if such dykes or extravasations at synclinal axes were found to alternate frequently with the elevated anticlinal axes, for the cracks formed in indurated beds of overlying rock would very frequently open alternately upwards and downwards*.

Time will not allow of my dwelling now upon other points explanatory of geological problems, which are afforded by the theory of an expansive subterranean crystalline mass preserved by external pressure in a more or less solid condition beneath the crust of the globe, but always ready to expand and perhaps to intumesce upwards on any relaxation occurring in the overlying pressure. But I suggest it now, as I did thirty years since, as the solution most reconcileable with the known facts of the structure and relative position of the great elevated rock-formations of the globe, and as a theory founded, not upon mere guess-work, but on careful and extended observation of the phænomena of both active and extinct volcanos, and the disposition of volcanic products of all ages.

May 7, 1856.

The following communications were read:—

1. *On some* Footmarks *in the* Millstone Grit *of* Tintwistle, Cheshire. By E. W. Binney, Esq., F.G.S.

Some years since a series of strange impressions was found in one of the lower beds of Millstone Grit in a quarry belonging to James Rhodes, Esq., at Rhodes Wood, near Tintwistle, in Mottram-en-Longdendale, Cheshire. Mr. Rhodes was much struck with the impressions, from the fact of two of them bearing some resemblance to the mark of a human foot; and the workmen employed in the quarry, when they first showed him the impressions, remarked, "Master, somebody has been here before us." During several weeks the quarry was visited by many hundreds of people from Glossop and the surrounding neighbourhood. The common opinion was that the impressions were the footprints of some of Noah's

* See the diagram at p. 205 of 'Volcanos.'

SINCE the above paper was in print my attention has been called to a translation, in our Journal for February 1848, of the essay of Prof. C. F. Naumann, "On the probable eruptive origin of several kinds of gneiss, &c.," in which views very similar to those here entertained are given, in relation to the laminated and fissile structure of the crystalline plutonic rocks,—reference being there made to my observations on the *Trachytes* of Ponza and Palmarola, and the *Phonolites* generally, as illustrating the mode of formation of gneiss and mica-schist, and leading to the inference that these rocks owe their structural parallelism rather to pressure and friction accompanying their eruptive protrusion, than to the effect of metamorphic action upon sedimentary strata. I rejoice to find my early views on the subject supported by such high authority, and trust that other geologists of weight and influence may thereby be induced to give them their unbiassed consideration.