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ORIGINAL ARTICLES:

I.—ON THE CHEMISTRY OF THE PRIMEVAL EARTH.¹

By DAVID FORBES, F.R.S., etc.

IT will, no doubt, be admitted by all, that it is highly conducive to the advancement of any science, that some one of its votaries, more courageous than his colleagues, and endowed with a more generalizing turn of mind, shall, by the comprehensive and exact study of accumulating observations, endeavour to elevate himself above the level of the more plodding but invaluable collectors of facts, by attempting the arrangement and organization of such data, and the deduction therefrom of the laws which regulate their existence and govern their movements.

Without such generalization, science itself would be but a mere accumulation, or, rather, chaos of unconnected observations.

All honour is therefore due to such leaders in science, whose success should be appreciated in proportion to the difficulties which had to be overcome before it had ultimately been attained. It must not be forgotten, however, that in this century of rapid progress, science, in the pursuit of truth, becomes each day more and more severe in insisting upon exactitude in research, and in demanding that no hypothesis or theoretical deduction be accepted as correct before it has run the gauntlet of stringent scientific scrutiny, in order fully to test its soundness.

In this present communication it is intended to take into consideration the general views in chemical geology, recently expounded by the well-known chemist, Dr. Sterry Hunt, during his late visit to this country, and which have already appeared at length in this MAGAZINE, in a report of a Lecture on the Chemistry of the Primeval Earth, delivered by that gentleman in the theatre of the Royal Institution.²

Although but few of the opinions expressed in this lecture are original, the manner in which Dr. Hunt has combined them as a whole, in his argument, is sufficiently at variance with what has

¹ In order not to extend this communication to too great a length, and to avoid going deeper into the more purely chemical points, these latter have been considered in greater detail by the author in a paper "On some points in Geological Chemistry," in the *Chemical News* of October 4th, 1867, to which the attention of the readers of the GEOLOGICAL MAGAZINE is herewith directed.

² GEOL. MAG., Vol. IV. pp. 357–369. See also pp. 432 and 477.—EDIT.

hitherto been generally received in Europe, as to lead to the inference that Dr. Hunt thus gallantly throws down his glove in order to invite discussion upon the subject.

After taking the opinion of several well-known geologists and chemists upon the lecture in question, the author of these remarks concludes that in this case, at least, silence does not imply consent, since, without exception, all referred to were at variance with more than one of the views therein put forth. He would therefore have gladly seen the challenge of Dr. Hunt accepted by some one more able than himself; and, consequently, before entering the lists thinks it proper to explain that he has been induced to do so by Dr. Sterry Hunt's special invitation to "have a friendly fight," which he must confess he was not unwilling to accept,¹ especially as in a late discussion that gentleman's opinions, when quoted by one of his admirers (in the pages of this MAGAZINE) in opposition to his views, were put forward with a show of authority which opinions as yet neither generally accepted nor confirmed can be entitled to.²

In his introduction Dr. Hunt adopts the nebulous hypothesis of the earth's origin, assuming the chemical elements to have all been originally present as intensely heated gases, uncombined, and in a state of "indifference" to one another, which he accounts for by referring to Deville's experiments on the dissociation of certain gaseous compounds at intensely high temperatures. A subsequent lowering of the temperature is then supposed to have brought about the combination of the elements, and afterwards the condensation of their compounds into the shape of an igneous fluid globe.

So far, Dr. Hunt arrives at the conclusion advanced in the last century by Hutton, the propounder of the plutonic theory of the world's origin, which assumed the world to have been at one time a sphere of molten matter solidified by refrigeration. In considering the results attendant upon the cooling of such a molten sphere, writers, however, differ greatly in opinion; the majority have supposed the formation of a solid crust of more or less thickness, enclosing a still liquid igneous kernel; more lately some have advocated the existence of a solid central kernel along with a similar external crust, whilst the intermediate space contains still liquid igneous matter; and, lastly, evidence has been recently brought forward indicating the possibility of a solid, or nearly solid globe. Dr. Hunt insists upon the earth being a globe solid to the core, which had solidified from the centre outwards to the exterior, and further represents the first of these views, hitherto the most generally received and maintained by many of the most able geologists and

¹ In the belief that fair discussion advances science, by developing energetically both sides of the question; such discussions should however only be indulged in by those who can give and take with equal good grace, without losing temper or deviating from the subject at issue by indulging in recriminations or personalities, such as Dr. Hunt at the close of his lecture alludes to, as having disturbed the social relations of the geologists of the last century, and which unfortunately sometimes creep into discussion even in this enlightened age.

² GEOL. MAG., Vol. IV. p. 287. The author still agrees with "most geologists" in opposition to the opinions referred to.

mathematicians, to have been "supported to a great extent by fallacious reasoning."

That such observers, however, could, as Dr. Hunt imagines, have been led astray by the exceptional case of the crystallization of water, is sufficiently refuted by the intimate knowledge of the behaviour of melted lavas, slags, metals, etc., everywhere displayed in their researches; so that other explanations of the "fallacious reasoning" alluded to must be sought. Dr. Hunt therefore informs us that the solidification could not have commenced at the surface, because the congealed crust is heavier than the molten fluid from which it had cooled—i.e., ought in other words to sink into the same.

Most of the readers of these remarks, have doubtless seen large open castings produced at foundries, and the question may therefore be put to them, whether they ever have observed in such cases, no matter how large the casting might be, that the pellicle or crust, formed upon the surface of the molten iron, sank down into the metal below it (unless purposely broken up by force); yet the cast iron, when solid, is well known to be heavier than when in the fluid state. On the contrary, it supports itself in the same manner as the external surface of an igneous sphere would do, if exposed to a cooling action, operating simultaneously upon the whole of its external area.

But, admitting even that it would sink, and supposing with Dr. Hunt that the mean density of the earth is 5.3, and of the solid exterior crust at one-half this, or 2.65, and also that the specific gravity of this latter, when in a fluid state, would be considerably lighter, say 2.3: then surely Dr. Hunt will not expect it to be believed that this solid crust of sp. gr. 2.65, can sink deep down into the fluid mass of a globe possessing a mean density of 5.3.¹

If broken up by force, the fragments of such a crust might be imagined to sink a short way, say a few miles, through the upper or lighter stratum; which at the surface was of a density of 2.3 (becoming rapidly denser in descending, as the pressure increased by the heightened superposed column of liquid matter) until it came to rest in a liquid stratum of its own density, where it would float (in a solid state, if Dr. Hunt is to be believed in asserting that under the extra pressure at this depth, its fusing point would be elevated, and so prevent its absorption into the main mass).

Such an action would, on the contrary, tend to bring about the very formation of a superficial crust, like that which Dr. Hunt denies, for this action once commenced, would go on solidifying at the exterior, sinking to a certain depth, then resting there, superposed on that which had previously congealed and descended, and so on, until a solid crust was eventually formed, extending from this depth to the surface.

¹ If we suppose the mean density of the earth to be 5.3, and that of the surface crust to be 2.65, and further imagine the earth to be composed of three consecutive layers of equal thickness, and of density increasing in arithmetical progression, we should have 2.65 for the density of the outermost zone, nearly 10.7 for that of the middle one, and about 18.8 for the centre.

Dr. Hunt then tells us, that the cooling of the liquid globe would be "like the cooling of a great bath of metal or sulphur," and that it "would commence at the centre and extend outwards toward the surface." This may fairly be questioned. Any one who ever cast a bullet knows how long the centre remains fluid after the exterior shell is solid; the founder knows how difficult it is to get his castings perfect, with their centres solid; and the mode of causing metals, as bismuth, lead, etc., etc., to crystallize, by allowing as large a mass of the fluid metal as possible to solidify exteriorly, and then pricking a hole through the crust, so as to let out the still fluid central metal, is familiar to all, as well as, probably, the fact that this same proceeding is commonly employed to obtain crystals of sulphur.

But Dr. Hunt will doubtless object, that the above experiments were not made under pressure, and, to demonstrate that solidification must in such case have commenced from the centre, refers to the experiments of Hopkins and Fairbairn, as proving that the fusing points of bodies are elevated by pressure.

Fully aware that the fusing points of certain bodies are actually raised when under pressure, and prepared also to believe that silicates may be amongst such, still the author, after a careful analysis of both Hopkins' experiments,¹ and those of Bunsen,² cannot find such conclusive evidence in the same, as Dr. Hunt in his lecture would lead his audience to infer.

The results of these experiments condensed into a few words are as follows:—Hopkins and Bunsen both find by experiment that certain *organic* compounds (spermaceti, wax, stearine, and paraffine) actually have their melting and freezing points elevated (but in a very irregular manner), when exposed to pressures of from 1 to 793 atmospheres (15 to 11,880 lbs. per square inch)³; further, Hopkins shows that sulphur has likewise its fusing point elevated by a pressure from 1 to 520 atmospheres (15 to 7,790 lbs. per square inch), but that after this up to 793 atmospheres (11,880 lbs. per square inch), the highest pressure tried, the ratio of increase of temperature to pressure diminishes greatly, which might be supposed to indicate that at still higher pressures the fusing point might again diminish and even become depressed instead of elevated. No allusion is made to Mr. Hopkins' statement that in the case of such metallic alloys as he had tried, "*that he had not detected any elevation of fusing temperature acquired by increasing the pressure.*"

Even allowing however that the fusing points of bodies in general are elevated by pressure, it does not necessarily follow that the centre of the earth must have solidified before or even at the same time with the crust, unless it is also taken for granted that the earth's mass is perfectly homogeneous throughout, or at least com-

¹ Brit. Association Report, 1854, p. 57.

² Poggendorf. Ann., vol. 81, p. 562. 1850.

³ The nature of such organic substances being totally different from any to be met with under the circumstances here under consideration, much dependence could not be placed upon the similarity of behaviour of inorganic compounds.

posed of substances all of the same degree of fusibility, neither of which views are likely to meet with general acceptance.

The density of the exterior of the earth with which geologists are acquainted, is known to be only about one-half of the mean density of the earth's mass as a whole, and at first this was accounted for upon the supposition that its components became more and more dense in depth, owing to the pressure of the superincumbent mass; experimental research tends, however, to show that a limit is soon reached, beyond which the compression or increase of density becomes less and less in relation to the force employed;¹ and there are consequently strong reasons for believing that the central parts of the globe must consist of much denser bodies, such as metals and their metallic compounds.

As we well know that such metallic bodies are infinitely more easily fusible than the silicious rocks of the superficial crust, it may be fairly advanced, that the difference between the fusing points of these bodies would more than counterbalance the influence of pressure in causing solidification at the centre of the globe, by the elevation of the temperature at which the central mass could remain in fusion.²

The above reasons make the author come to the conclusion that Dr. Hunt has not produced sufficient evidence to prove that the earth is really entirely solid; and he still adheres to the opinion that the earth does enclose a vast reservoir or reservoirs of still fluid igneous matter in its interior.

The chemical composition of the cooling globe, and specially of its external crust, next demands consideration. Dr. Hunt believes the primitive crust to have been composed of the alkalis, alkaline earths, earths and metallic bases in combination with silica, and surrounded by a dense acid atmosphere, consisting of hydrochloric, sulphurous, and carbonic acids, along with steam, nitrogen, and probably the excess of oxygen. The author would protest against such an atmosphere, and for reasons about to be explained, does not believe that it ever did or could have existed.

We are told in the first place, that "all the sulphur would be diffused in the atmosphere as sulphurous acid." The author, on the contrary, believes that the sulphur would have combined with the heavy metals, forming dense sulphides, which would at once sink below the lighter external crust and there be protected from oxidation. Nor does he consider it probable that at such a moment of general combination an "excess of oxygen" could possibly be present in an atmosphere highly charged with sulphurous acid.

Dr. Hunt alleges that we should find "all chlorine in the form of hydrochloric acid," which is also contrary to the opinion of the

¹ And the author thinks it probable that the same would also be the case with the relations of fusing points to pressure.

² In opposition to this view, it might be said that the densest of all metals, Platinum, is also one of the most infusible. To this we answer, that many of the compounds of Platinum, say with zinc, tin, arsenic, etc., are so extremely fusible as to melt in the flame of a candle.

author, who considers that the chlorine would chiefly, if not entirely, be in combination with the metals of the alkalies and alkaline earths, as chloride of sodium, etc.

According to Dr. Hunt, the hydrochloric acid in this atmosphere was derived from the mutual reactions of sea-salt, silica, and water.

This, in the first place, is supposing the pre-existence of compound bodies, in a case where he had previously informed us that there were only dissociated elements engaged in the formation of this igneous sphere. For the sake of argument, however, let it be admitted that sea-salt, water, and quartz were present, then it is still contended that the reaction, described by Dr. Hunt, could not have taken place. All chemists know that quartz, water, and sea-salt, if heated together in a confined space, or if the vapor of water and salt be passed over highly heated quartz, that such a reaction would take place forming hydrochloric acid gas along with silicate of soda. This, however, could never occur in nature in the case under consideration, for long before the quartz had attained a heat sufficient to enable it to act upon the salt, all the water would have evaporated into space; and if the heat were continued, the vapour of the salt would follow, leaving the quartz behind.

As the greater part of the sodium is considered to have been at once combined with the chlorine, it follows, of course, that the silicate of soda could not have played so important a part in the formation of the primary crust as is ascribed to it by Dr. Hunt; and there would also be no necessity for the extraordinary theory that the saltiness of the sea is due to the rain of hydrochloric acid "flooding the half-cooled crust" with a highly heated acid deluge, which extracted the soda from its silicate, leaving the quartz behind;¹ and it is therefore conjectured that neither geologist nor chemist will be contented with this explanation of the salt in the sea.

Having thus opposed the views of Dr. Hunt, as set forth in his lecture, the author of this communication will in a few words sketch out the chemical reactions which he supposes to have been characteristic of this period of the earth's history.

The act of combination of the elements is regarded by him as having given rise to a molten sphere, surrounded by a gasiform atmosphere, both of which were composed of concentric layers or zones of different densities and chemical composition.

This sphere, it is imagined, would arrange itself into three grand zones, (each zone, probably, containing sub-zones), somewhat as follows:—An external zone, or crust of highly acid silicates, the bases being chiefly alumina and potash, with minor quantities of soda, lime, magnesia, oxide of iron, etc.; below this a second zone of

¹ The silica produced from such decomposition of silicates is of the specific gravity of 2.2, is soluble in alkaline solutions, and does not polarize light, which is not the case with the silica contained in any of the older rocks, which Dr. Hunt supposes to have been so formed. The chemical and physical properties of the silica of such rocks indicate them to have been derived from the breaking down of acid rocks analogous to granite.

silicates, more basic, and of greater density; the bases being lime, magnesia, alumina, oxide of iron, with soda, and but minor quantities of potash, etc.; and still deeper a far denser sphere, containing metallic bodies, more or less combined with sulphur, arsenic, etc.

On the other hand, the atmospheric zone, next the solidified crust of the earth, would be composed of a dense vapour of those compounds, volatile only at a high temperature, amongst which the chloride of sodium or salt would be probably the most prominent, above this a stratum of carbonic acid gas, and then of water, in the form of steam, whilst the oxygen and nitrogen would be elevated still higher.

It is imagined that such an arrangement would, on cooling, first condense the lowest atmospheric zone¹ (vapour of salt and other chlorides, etc.), on to the already solidified crust of the earth; covering this with a layer of these substances, in a solid state²; upon a further reduction in temperature the steam in the atmosphere would now be condensed on to this layer, which it would, in great part, dissolve, forming the ocean, which consequently would be salt from the first moment of its appearance on the face of the globe. The atmosphere now surrounding the globe would contain less oxygen and all the carbon, in the form of carbonic acid, (excepting only the amount of that acid already absorbed by and carried down with the rain water), but otherwise it would probably not differ much in composition from what it is at present.

From this stage in the earth's history, the author believes that all the changes which have taken place in the globe, up to the present time, have been effected by agencies similar to those going on in it at this present day; rocks were formed from the wearing down and disintegrating action of the atmosphere or weathering of the primitive crust,³ and the subsequent stratification of the debris, so formed by the action of the sea; just as they are at present in the course of formation from the disintegration of pre-existing rocks. Eruptions of igneous matter from the still fluid interior⁴ from time to time disturbed and broke through the primitive crust and the rock strata above it, in course of formation from its debris, just as at the present day (though possibly on a somewhat smaller scale), similar outbursts are produced by volcanic action. The products of such older eruptions are almost identical, in chemical composition, with those of the newer period. Thus the result of chemical analysis of the most ancient granite often cannot be distinguished from that of an

¹ The zone of carbonic acid gas would be heavier than that of steam; 1 cubic foot of the latter weighing at 212° only 265·17 grains, whilst 1 cubic foot of carbonic gas would weigh 642·09 grains, at same temperature.

² A rough calculation shows that the layer of sea salt alone would be sufficient to clothe the entire sphere with a crust of salt some 10 feet in thickness.

³ This action would, no doubt, be much facilitated in the older geological epochs, by the amount of carbonic acid in the atmosphere being so much greater than at present.

⁴ The contraction, consequent upon the cooling of the original sphere, would, doubtless, greatly disturb the previously comparatively even surface of the crust, and produce cracks and fissures, the sides of which, from their unequal subsidence or elevation, would often be dislocated and form lines of faults.

ordinary volcanic trachyte, and the basaltic rocks from recent volcanoes resemble very closely those from far more ancient periods; in fact it is often the case that such rocks can only be distinguished from one another by a very careful study of their less prominent characters.

Taking them as a whole, the main distinction between the eruptions of the most ancient and most modern epochs is, that in the earliest period the acid rocks, or granites, predominated, whilst at the present day the acid volcanic rocks, or trachytes, are in less proportion, the more basic rocks predominating.

Several reasons to account for this circumstance have been put forward, and are well worthy of consideration.

To return, however, to Dr. Hunt; he states that after the energetic action of the acid deluge had ceased, a second similar but slower process of decomposition and solution of the crust commenced by the action of, in this case, carbonic acid with water, resulting in the formation of clays which remained behind, whilst solutions of the carbonates of soda, lime, and magnesia, poured down into the sea where they precipitated, first the alumina, and subsequently the heavy metals. In such events geologists, although as yet unsuccessful in so doing, might still hope to find beds of alumina or of the metallic oxides or carbonates alluded to, amongst the older strata. As no beds of such character are known to occur in nature, this hypothesis must, however, be received with some distrust.¹

The next assertion of Dr. Hunt, that the limestones have been formed by the precipitation of the lime in the sea by a solution of carbonate of soda, is so decidedly at variance with all the conclusions hitherto arrived at by geologists, zoologists, and microscopists, that it cannot but be disputed, and there is sufficient evidence now produceable to refute this hypothesis.

In making the assertion, that "the whole of the carbonates of lime which make up the calcareous strata—the marbles and various limestones which we find on the earth's surface are so formed," Dr. Hunt at the same time states that he is quite aware that geologists are of opinion "that these limestones are the result of organic action," but no doubt classes this opinion as another sample of the "fallacious reasoning" which he supposes them to indulge in, and will probably be surprised to learn that zoologists also will dispute his further assertion, that "animals can only appropriate the carbonate of lime which they find ready formed," and that they, in opposition to this assertion, believe that marine animals can utilise the other salts of lime, existing in abundance in the ocean.

Had the limestones been so formed by precipitation, whether hot

¹ It may here be remarked that Dr. Hunt, in his lecture, does not allude to what became of the sulphuric acid, which would be the ultimate product of "all the sulphur" burnt into sulphurous acid, and afterwards condensed from the atmosphere into the ocean; for since it may safely be asserted that there is fully as much (if not more) sulphur as chlorine, the sea formed, according to Dr. Hunt's hypothesis, would be as much a solution of sulphate of soda as of seasalt, and he can hardly suppose it to have been precipitated, for it is well known that no beds of sulphate of any importance whatsoever occur in the very oldest formations.

or cold, they would have, from the moment of their deposition, possessed a decided crystalline structure, visible when examined by the microscope; as in the case of stalactites, stalagmites, travertines, etc.; this, however, is not the case.

Sorby's microscopical researches prove satisfactorily that all limestones, from the most ancient up to the most recent, are solely formed of the debris of organisms,¹ and that they do not possess any crystalline structure whatsoever, unless when altered by subsequent infiltration, or other metamorphic action.

Dr. Hunt next proceeds to explain that the magnesian limestones, dolomites, and gypseous beds owe their origin to chemical "reactions hitherto unsuspected," and that his experimental researches have proved them to have been formed at a period when the surface of the earth was covered by a dense atmosphere of carbonic acid, and that this "theory is confirmed by climate, by vegetation, and by the singular series of reactions which hitherto have been a perplexity to chemical geologists."²

The microscopical and chemical investigations of Sorby have, however, eliminated the most conclusive evidence against the correctness of this theory of precipitation, and shown the magnesian limestones and dolomites alluded to by Dr. Hunt (whether of the Devonian, Carboniferous, or Permian period) to be mere mechanical aggregates, or true limestones of ordinary character subsequently altered by infiltration of magnesian matter.

This result had been long before arrived at by geologists, as the study of these rocks in the field showed that such magnesian limestones were frequently only portions of the ordinary limestone beds peculiar to the formation itself, altered at places, apparently by some then unexplainable chemical action. This was found to be the case even with limestones pertaining to the Devonian and Carboniferous formations, in which period it has long been advanced that an atmosphere rich in carbonic acid did exist.

As all geologists know that the grand development of magnesian limestones, dolomites, and gypseous beds really took place in an epoch when numerous air-breathing animals, both vertebrates and invertebrates, lived upon the face of the globe, it will surprise them to think that Dr. Hunt can imagine these animals living in an atmosphere of carbonic acid.

The next point in this lecture to which attention is directed is a very important one in its general bearings, although it is to be feared

¹ Even the chalk is entirely so composed, notwithstanding that its external appearance is so like that of a precipitated carbonate of lime.

² Dr. Hunt seems to be quite unaware that in the Brit. Assoc. Report, 1856, p. 77, Sorby has fully explained these reactions, that Harkness (Brit. Assoc. Report, 1857, p. 68) applied similar experimental investigations of Regnault, to explaining the dolomitization of the Carboniferous limestones near Cork; and, lastly, that the results of his researches on the artificial atmosphere of carbonic acid, which he has thought worthy of bringing before the French Academy (Compt. Rend., 1867, p. 815), so far from being new, have for the last twenty-two years at least, if not much longer, been employed on a large scale in the manufacture of magnesian compounds in both England and Ireland.

that the attractions of palæontological research have caused it to be of late, in a great measure, shelved by geologists in general.

It has lately been the fashion, especially amongst many of the younger votaries of the science, to "pooh-pooh" the igneous origin of eruptive rocks in general, and of granite in particular. A careful study of the literature of the subject shows, however, that this secession from opinions, previously all but universally adopted, has originated in the writings of one or two able but one-sided men of science, blindly followed, as is usual in such cases, by adherents who reason not for themselves, or who have either not sufficient leisure or inclination to examine into the true merits of the question. The author fully believes, however, that had anything like a careful study of what has already been published (*pro et contra*) upon this subject been made, that not only would an explanation or answer have been discovered to meet any and all of the arguments brought forward in opposition to the igneous origin of such rocks, but that such as are open to conviction would with the author of these remarks have come to the conclusion that nothing has as yet been advanced which can in any way tend to prove the eruptive rocks to have an origin differing from that of those rocks produced by volcanic action at the present day.

At present, however, only such arguments as are advanced in Dr. Hunt's lecture can be discussed, and these only in all brevity, since the space already occupied by this communication has extended beyond its proposed limits.

As evidence against the non-igneous origin of granite, Dr. Hunt asserts "that the composition of the primitive crust would have excluded free silica;" again, "that this very quartz, which is one of the constituent elements of granite, is only the result of a secondary process;" and yet again, in the report of his lecture contained in the "Chemical News," vol. xv., p. 317 (revised by himself previous to publication), "that granite is in every case a rock of sedimentary origin, as it includes in its composition quartz which, so far as we know, can only be generated by aqueous agencies, and at comparatively low temperatures."¹

In making such statements, it may be asked whether Dr. Hunt is aware of the immense masses of undoubted volcanic rocks scattered all over the surface of the globe which contain abundance of free quartz? Amongst others, the Ponza Islands, for example, under the very shadow of Vesuvius; the hundreds of miles of volcanic outbursts of quartz trachytes, from the still active volcanoes situated along the range of the Andes, in South America, as well as numerous examples which might be referred to in other parts of the globe, although, unfortunately, not in Canada. Does not Dr. Hunt know that the admirable memoir of Sorby, contained in the Quart. Journ.

¹ If we, with Dr. Hunt, believe that the temperature increases in proportion to the pressure, then, as Sorby has shown that the quartz of the granite of Aberdeen has solidified at a pressure equal to a column of seventy-eight thousand feet of rock, this alone would be quite sufficient to refute the statement of comparatively low temperatures.

Geol. Soc. of London, vol. xiv., shows how perfectly identical in structure this volcanic quartz is with the quartz of granites (both containing in common, fluid, vapour, gas, and stone cavities, and that this accurate observer has concluded that the modern volcanic trachytes and old granites have one common igneous origin, in which, as is the case in volcanoes, water has played some part.¹

That the metamorphic rocks have been formed from ordinary sedimentary strata, by their having been "depressed so that they come within the action of the earth's central heat" may be disputed; but before doing so it might be as well to learn from the author of this ingenious theory by what mechanical arrangement he supposes strata on the surface of the earth to be lowered down into a globe solid to the core. The further development of this theory, assuming a similar action to have produced the eruptive rocks emitted by the volcanoes of the present day, is at once strongly protested against; for how, may it be asked, are we, according to this theory, to account for the fact, that volcanic rocks taken from any quarter of the world, no matter how far distant from one another—from Iceland or Terra del Fuego, from the Islands of the West Indies or from those of Polynesia—that in all cases such rocks possess an absolute identity in chemical and mineralogical composition; in physical and in optical properties. Can any geologist be expected

¹ An argument has been brought forward against the igneous origin of granite, from the fact that the specific gravity of the quartz in granite is 2.6, whilst the density of silica artificially fused before the oxyhydrogen blowpipe is only 2.2. If this style of argument is admitted in philosophical reasoning, then the silica of the carapaces of infusoria ought also to have been formed by fusion, since its specific gravity is only 2.2, as is also the silica deposited from its gaseous compounds with fluorine, etc. Sorby's before-mentioned researches have shown that the quartz in granite has solidified under enormous pressure. It might therefore reasonably be expected to possess a higher density than such as has been fused artificially, without having been subjected to pressure at all. Another argument is found in the fact that some of the more fusible minerals in granite have often solidified and crystallized before less fusible ones; in reply, it may be stated that this is also the case in modern lavas; in those of Vesuvius, it is common to find that the refractory Leucite has crystallized before the easily fusible Augite, and to be superposed on crystals of this latter mineral. It has further been argued, that rocks like granite occasionally enclosing minerals containing water, could not have been formed by igneous fusion; independently of Sorby's discovery that the quartz of volcanic rocks and the felspar, nepheline, idocrase, etc., ejected from Vesuvius, do contain water: specimens taken out of the lava current from Etna, whilst still flowing in March, 1865, contained fine crystals of Stilbite (with 16 per cent. of water). Bunsen's researches (Taylor's Scient. Memoirs, Nov., 1852) have long ago experimentally proved that hydrated silicates, analogous to those occurring in eruptive rocks, might be formed at high temperatures and retain their water at such temperatures as long as enclosed in the matrix; if extracted from this, however, the water could be expelled by the application of a very gentle heat. Laurent has also showed that borate of Potash, fused at temperatures above the melting point of Silver, retained water which, singularly enough, might be expelled in bubbles by reheating the vitrified mass over a spirit lamp so as hardly to soften it.—Whilst correcting the proofs of this paper for the press, the author has had his attention directed to a communication made by Professor Ansted to the British Association, "On the Passage of Schists into Granite in the Island of Corsica," in which (if the report in the "Dundee Advertiser," Sept. 10, be correct) the learned Professor cites, in support of his views, the results of Mr. Sorby's researches in a manner apparently quite at variance with the conclusions arrived at in that gentleman's memoir.

to believe that such rocks have been formed by the melting up of a mere mechanical aggregate of rock-debris, possessing no analogy whatsoever, and whose chemical composition, etc., is known to vary to the widest imaginable extremes.

In conclusion the author cannot but express his feeling that it is doing an injustice to the memory of such noble minds as Hutton, Playfair, Hall, Humboldt, Von Buch, and others, to bring against them the narrow-minded charge of their wishing to create the earth "entirely by fire."¹ Their writings abound in evidence proving that they never overlooked the all important agency of water in nature's operations, and when claiming for igneous action its true share they based their plutonic theory upon the study of such agency as is exemplified in volcanos, in which, from the first, the co-operation of water (although in some, at that time, incomprehensible manner) was acknowledged; and not upon any idea of "dry fusion," which could only have originated in the brains of their antagonists.²

II.—ON THE GORGE OF THE AVON, AT CLIFTON.

By J. BEETE JUKES, F.R.S.

I OBTAINED, the other day, on my way into South Devon, another peep at the Bristol Channel, and the gorge of the Avon, at Clifton, which I had long been wishing for. It showed me, as I anticipated, that the hypothesis of atmospheric erosion, which I was compelled to adopt, a year or two ago, to explain the formation of the river valleys of the South of Ireland, is applicable to the Clifton gorge as to all other similar places.

With the existing form of the ground traversed by the Avon, above Bristol, it would, of course, be quite impossible for the river to cut a channel across the Clifton Downs. Fill up the gorge of the Avon with the mass of Carboniferous Limestone and Old Red Sandstone, that once occupied it, or even half fill it, and the waters of the Avon, after forming a lake, would, long before they overtopped that dam, run into the sea by Nailsea, as pointed out by Sir H. T. De la Beche.

This, however, only shows that the surface over which the Avon originally ran into the Severn, was not the present surface. All the rivers originally ran over a surface considerably above the present one, and they have continued to run in the same courses during all the wasting of the rock, by which the old surface has been transformed into the present one.

The rivers have been, at once, the channels by which the eroded matter was removed, and the motive power of the eroding machinery.

Colonel George Greenwood's phrase of "Rain and Rivers," gives us the whole secret in three words.

¹ Apparently an application of the "sensation" principle to geology.

² It is nearly half a century ago since Scrope not only pointed out the important part played by water in volcanic action, but further expatiated upon the difference between volcanic fusion and ordinary melting.