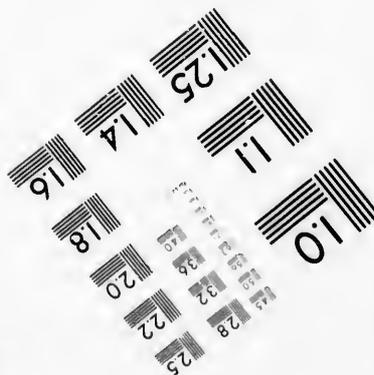
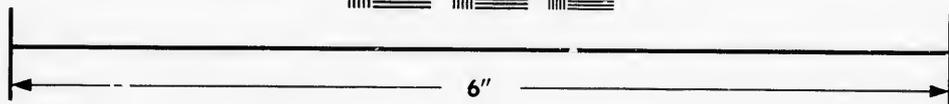
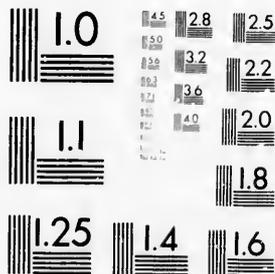


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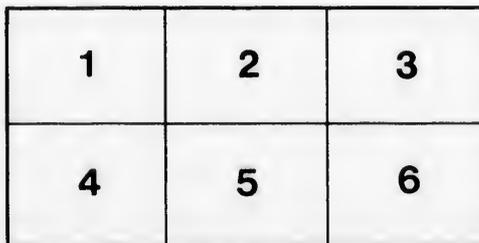
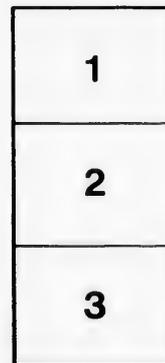
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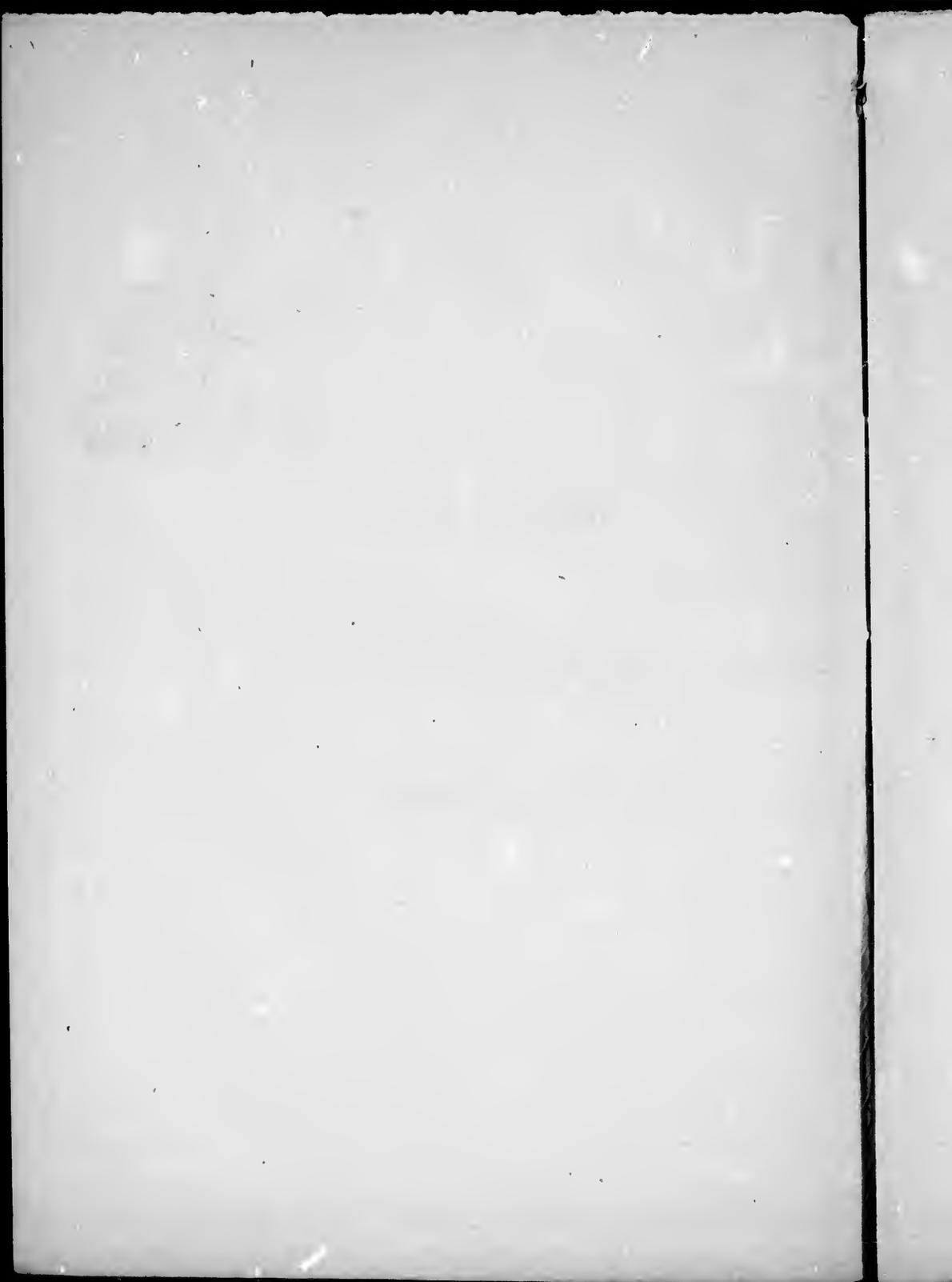
CHEMICAL GEOLOGY.

MR. T. STERRY HUNT'S

REPLIES TO THE CRITICISMS

OF

MR. DAVID FORBES.



A NOTICE OF THE CHEMICAL GEOLOGY OF MR. D. FORBES.

BY T. STERRY HUNT, F. R. S.

The *Geological Magazine* for October last contains a criticism by Mr. David Forbes of certain views put forward by me in a lecture delivered before the Royal Institution of Great Britain on the 31st of May, 1867. Of this lecture a short-hand report appears in this Magazine for August, besides which a condensed report, revised by myself, is published in the proceedings of the Institution, in the *Chemical News* for June 21st, and in three French translations in the *Revue des Cours Scientifiques*, *Les Mond.* and *Cosmos*. The *Chemical News* for October 4th contains a criticism of my lecture by Mr. Forbes, to which I have replied in a communication recently addressed to that Journal.

In the lecture in question, I endeavored to bring together the results of modern investigations in physics, chemistry, mathematics and astronomy, and to construct from them a scheme which should explain the development of our globe from a supposed intensely heated vaporous condition down to the present order of things. I could not pretend to discuss from their various stand-points, all the conclusions arrived at by different investigators, inasmuch as, even had my attainments permitted, the limits of an hour's lecture would have proved far too short.

In regard to the structure of the earth I alluded to two views, one of which supposes a liquid globe covered with a thin crust of solidified rock, generally estimated at from twenty to thirty miles in thickness, while the other regards the earth, if not solid to the centre, as having a crust at least several hundred miles in thickness, and of such solidity and rigidity as to be, so far as superficial phenomena are concerned, inert as if in a solid state. To this latter view, I incline, and I cited in support of it the conclusions of Hopkins from the phenomena of precession and nutation, the investigations of Archdeacon Pratt on the crushing effect of immense mountain masses like the Himmalayah, and the deductions of Sir Wm. Thompson from the phenomena of the tides, showing the great rigidity of the earth, as so many concurrent evidences that our planet, if not actually solid to the centre, has a crust far thicker than can be accounted for by the theory of a liquid globe covered only with a crust resulting from superficial cooling. This latter view, which was deduced from the increase of temperature observed in descending into the earth, is in conflict with the various mathematical and physical considerations above noticed, and it becomes necessary to revise the older notions of the conditions of a cooling globe.

The investigations of Charles Deville and of Delesse, as well as the earlier ones of Bischof, show that the density of fused rocks is very much less than that of the crystalline minerals of which they are composed. From this we may naturally conclude that the crystalline compounds which would separate by slow cooling from a bath of molten rock would gravitate toward the centre, as Saemann has already justly observed, (Bull. Soc. Geol. de Fr. Feb. 4. 1861.) In opposition to this view, Mr. Forbes appeals to the results seen in a small scale in the cooling of melted metals, etc.,—where a crust forms over the surface. It must, however, be considered that the conditions presented by a small vessel full of a liquid congealing in an atmosphere greatly below its own temperature, and having a crust growing out from and supported by the sides of the vessel, are widely different from those of a liquid globe slowly cooling beneath a very dense and intensely heated atmosphere. In such a case, with a bath of materials similar to those forming our present rock-crust, the crystalline minerals of which have been shown by Deville to be from $\frac{1}{6}$ to $\frac{1}{4}$ heavier than the liquid mass, these, as they separated, would sink as naturally as the crystals which form at the surface of an evaporating basin of brine. The analogy holds good, since the denser crystals formed

at the surface, whether by evaporation or by cooling, obey the inevitable laws of gravity.

Mr. Forbes next proceeds to some considerations drawn from the mean density of the earth, which being about 5.3, is twice that of the average specific gravity of the solid materials known at the surface. Admitting that a solid crust of specific gravity 2.65 were to form at the surface of a liquid of density 2.3, and in obedience to natural laws, to sink therein, our critic conceives that, in its descent, it would meet with a denser liquid stratum. He supposes a liquid globe "becoming rapidly denser in descending, as the pressure increased by the superincumbent column of liquid matter," and he tells us, in a note, that we may admit a density of "nearly 10.7 for the middle zone and about 18.8 for the centre." (page 435). Two pages farther on he has completely changed his mind, for he tells us that "experimental research tends 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," and concludes that there are strong reasons for believing that the central parts of the earth "must consist of much denser bodies, such as metals and their metallic compounds," which he farther on explains may mean "dense sulphids."

To which of these two views does Mr. Forbes mean to hold, that of a rapidly and constantly increasing density from pressure, or that in which, limiting the condensing effect of pressure, he seeks to explain the density of the earth by a nucleus of heavy metallic compounds? The latter is seemingly an after-thought of the critic, suggested by some notion of the principle involved in the augmentation by pressure, of the fusing point of bodies which expand in melting. As was shown by James Thompson, the effect of pressure upon ice (and naturally upon such metals and metallic alloys as, like it, contract in melting,) would be to reduce its melting point, a fact which has been experimentally established for ice. Reasoning from the same principle, Sir Wm. Thompson deduced the conclusion that a reverse effect should result from pressure for all such solids as expand in melting, that is to say, that their points of fusion would be raised, a conclusion verified by the experiments of Bunsen and by those of Fairbairn and Hopkins. From some apparent irregularities in these results, and from the fact that certain of the substances submitted to experiment were bodies of the carbon series, which Mr. Forbes calls "organic," he argues against the conclusions which depend upon a well defined physical law. In the case of the fusible alloys tried

by Mr. Hopkins, it is to be remarked that most of these bodies, like ice, expand in cooling, and consequently *should not have their melting points raised by pressure*. For the memoirs of James and William Thompson, see Trans. Royal Soc. Edin. XVI, part 5, and L. E. D. Philos. Mag. [3] XXXVII, 125. A simple and popular exposition of the principle, and of Mr. Hopkins's argument therefrom, for the solidity of the globe, will be found in the fourth of Tyndal's lectures on *Heat as a Mode of Motion*. See also Sorby's Bakerian lecture for 1863, cited farther on. Mr. Forbes must consider that just so far as he admits the condensing power of the pressure of the superincumbent mass, he increases the difficulty of maintaining that rocky mass in a liquid state.

The condensing effect of pressure was by Dr. Young estimated to be sufficient to reduce a mass of granite at the earth's centre to one-eighth its bulk at the surface, which would give to the earth a mean density equal to twelve or thirteen times that of water. This consideration has led a recent writer in the *London Athenæum* to conclude with Herbert Spencer, that our earth and the other planets may be only shells of varying thicknesses, enclosing a central cavity filled with vaporous matter, by which hypothesis we may explain the apparently feeble density. See Mr. Spencer's essay on the Nebular Hypothesis in the *Westminster Review* for July 1858. It may be observed that his view which supposes condensation to have resulted in the formation of a solid shell around a gaseous nuclent, is not incompatible with my scheme, which is simply opposed to a liquid interior. See also the note of Mr. Barker in this Magazine for September last, page 426. Leaving Mr. Forbes to settle these vexed questions, we may remark that in case we suppose condensation of the gaseous globe to have commenced either at the centre or around a gaseous nucleus, it is probable that solidification from pressure must have taken place long before the liquefaction of earthy matters was complete.

But if we adopt Mr. Forbes's second hypothesis that pressure would not materially augment the density nor raise the melting point of the fused mass, what grounds has he for assuming, as he does, that there occurred a separation of the liquid into zones of different densities? That metallic sulphids could be formed at an elevated temperature, by condensation from an atmosphere containing an excess of oxygen, is contrary to all that we know of chemical affinities; sulphurous acid and metallic oxyds would be the results so soon as the temperature fell below that of dissociation. As for the noble metals, whose compounds with oxygen are decomposed at

elevated temperatures, their great volatility, as compared with earthy and metallic oxyds, would keep them in the gaseous form till the last stage of precipitation of earthy oxydized matters, when by far the greater part of the globe was probably solidified. Hence we now find them in the earth's superficial crust, instead of being, as Mr. Forbes would suppose, carried to the centre of the planet.

Judging from what we know of chemical affinities, and of the proportions of the elements now existing in the superficial parts of the globe, we cannot conceive anything else than the production of a homogeneous oxydized silicated mass, upon which, at a late period, would be precipitated the noble metals. From this mass, while yet liquid, there might take a separation of various crystalline compounds, by a process analogous to that by which pure lead separates from the bath of the argentiferous alloy in Pattinson's process, as Fournet has already suggested, (*Geol. Lyonnaise*, 1862, page 398). The last congealed and lighter portion of our globe, with which alone we have to do, was, probably, a sort of mother-liquor from which, during its slow cooling, compounds of various constitution and density may well have crystallized. In furnace operations, it is true, we may obtain, besides silicated slags, a dense stratum of reguline metals, sulphids or arsenids on the one hand, and a lighter one of saline sulphates or chlorids on the other. But neither of these classes of compounds was possible in the cooling globe, the reguline matters for reasons just given, and the saline compounds, for reasons yet to be explained.

I have in my lecture set forth that the earth's superficial crust must have been composed of silicates of the metallic, earthy and alkaline bases, surrounded by a dense acid atmosphere of hydrochloric, sulphurous and carbonic acids, besides watery vapor, nitrogen and oxygen. These chemical combinations are such as would naturally result from the affinities brought into play at the elevated temperatures then prevailing, in virtue of which all those elements capable of forming fixed and stable compounds with oxygen would be precipitated as oxyds. In these conditions, as already said, no metallic sulphids would be formed, and the whole of the sulphur would be found as sulphurous acid. In like manner the production of alkaline chlorids under such conditions, is inconceivable, since in the conjoined presence of oxygen, hydrogen, and silicon or silica, an alkaline silicate and hydrochloric acid would result. Even, if, as Mr. Forbes supposes, chlorid of sodium were to be formed in the heated atmosphere, it would be precipitated into a bath of fused silicates, covered by an intensely heated at-

mosphere containing water, or mingled oxygen and hydrogen gases, and would immediately undergo the same decomposition that takes place when the vapors of common salt are diffused through a potter's kiln, or, as in Mr. Gossage's new soda-process, are passed with steam over red-hot flints. In both cases silicates of soda are formed with separation of hydrochloric acid.

These considerations lead to the conclusion that after all the more fixed elements were precipitated, the whole of the chlorine would finally remain in the partially cooled atmosphere as hydrochloric acid, and the whole of the sulphur as sulphurous acid, together with a large proportion of oxygen, since we find this element in the form of sulphate and not as sulphite in the sea-waters. Mr. Forbes does not, it seems, believe that an excess of oxygen could exist in an atmosphere highly charged with sulphurous acid, and elsewhere (in the *Chemical News*), he tells that it is, "if not impossible, at least, highly improbable that such a heated atmosphere containing sulphurous acid, hydrochloric acid, with oxygen and aqueous vapor could exist," the elements being in his *opinion*, incompatible. He is aware that at certain temperatures sulphurous acid and oxygen unite, in the presence of water, to form oil of vitriol, but he forgets that at a higher temperature this compound is again resolved into water, sulphurous acid and oxygen; and that one of the best processes for preparing the latter gas on a large scale, is by this decomposition of sulphuric acid, and the subsequent removal of the sulphurous acid from the cooled gaseous mixture. In the *opinion* of Mr. Forbes, as set forth in the *Chemical News*, the sulphurous and hydrochloric acids would decompose each other, in the presence of watery vapor (though every chemist's experience teaches him the contrary;) another reason for holding that my supposed atmosphere was impossible. Unfortunately for his *opinion*, however, it happens that large quantities of precisely such an atmosphere are disengaged from various volcanic vents. To cite one among many examples examined by Charles Deville and Leblanc (*Ann. de Ch. et Phys.* [3] LII. pp. 5-63) a *fumeroles* of Vesuvius yielded in June, 1856, a mixture of highly heated steam, hydrochloric acid, sulphurous acid and air containing 18.7 per cent of oxygen. The sulphurous acid was equal to 2.6 per cent. of the air, and the amount of hydrochloric acid was about five times as great. Traces of sulphuric acid were found in the water condensed from this steam, doubtless formed by the slow combination of the sulphurous acid and oxygen, and I may state for the information of Mr. Forbes, that it was doubtless by a similar reaction that the sul.

phurous acid became eliminated from the primeval atmosphere. We have here, I may remark, an illustration of the fact upon which I have elsewhere insisted, that volcanoes reproduce, on a limited scale, the conditions of the primeval earth, not only in their solid but in their gaseous products.

Mr. Forbes next asserts that, according to me, "the hydrochloric acid in the primeval atmosphere was derived from the mutual reactions of sea-salt, silica and water," (page 438) and then charges the author with the folly of "supposing the pre-existence of compound bodies in a case where he had previously informed us that there were only dissociated elements engaged." Mr. Forbes knows better than this, or at least did know better when he wrote his criticism on my lecture in the *Chemical News* of Oct. 4, for he here quotes my own words, when, in describing the cooling globe, the conditions through which it must have passed, and the affinities brought into play, I say the products must have been "*just what would now result if the solid land, sea, and air, were made to react upon each other under the influence of intense heat.*" It is so difficult to characterize properly such a wilful perversion of an author's words, that I must leave the task to my readers. What follows in Mr. Forbes's paper as to chlorids, etc., I have already discussed and disposed of. The theory of the constitution of the solid globe next put forward by Mr. Forbes, borrowed from Phillips, Durocher and Von Waltershausen, is also, as I conceive, met by the argument in the previous pages. When, however, he comes to the atmosphere surrounding his primitive globe, Mr. Forbes puts forward a scheme which is strikingly original. He supposes around the "solidified crust" a dense vapor consisting chiefly of chlorid of sodium, "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," (p. 439), probably, also, separated in the order of their densities. In explanation of this order he tells us in a note that the zone of carbonic acid gas would be heavier than that of steam, and should therefore come below it, but he forgot that oxygen and nitrogen (or atmospheric air) are also both heavier than steam, and should consequently be placed *below* the zone of watery vapor. The specific gravities of carbonic acid and steam are respectively 1.525, and 0.624, air being 1.000. But, apart from this absurd mistake, what shall be said to a man who ignores completely the laws of the diffusion of gases? Will Mr. Forbes kindly explain why, in our present atmosphere, the same elements, namely, oxygen, nitrogen, carbonic acid gas and watery vapor, are com-

mingled, instead of being, as he would have them, arranged in separate zones ?

I have said in my lecture that the first ocean waters would hold in solution salts of alumina and the heavy metals, all of which would be precipitated before the separation of carbonate of lime commenced. In such event, says Mr. Forbes "*geologists, though as yet unsuccessful in doing so, might still hope to find beds of alumina or of the metallic oxyds or carbonates alluded to, in the older strata. As no beds of such character are known to occur in nature,*" he regards my view with distrust. *Known* to Mr. Forbes ! Has he never heard of beds of emery, which are chiefly crystalline alumina, and which occur in the crystalline limestones of Asia Minor, and in the old crystalline schists of New England ? Is he ignorant that the beds of bauxite, so abundant in the Mediterranean basin, and used in the manufacture of aluminium, consist chiefly of hydrated alumina ? To console Mr. Forbes however, I will say that I believe these beds of emery and of bauxite to have been formed by secondary and subsequent reactions, and that we have nowhere exposed to view the first, deposited beds, which are everywhere destroyed or buried under more recent strata. When he remembers that the oldest known series of rocks, the Laurentian, consists of quartzites, limestones, and gneiss, evidently of sedimentary origin, and derived from still older sedimentary rocks, he will understand why he cannot hope to discover the first deposits of alumina or metallic oxyds. These, however, in most cases, have doubtless, by mechanical subdivision, or by solution, been subsequently diffused, and enter into the composition of later rocks.

In a note to this paragraph, Mr. Forbes inquires what became of the sulphurous acid of the early atmosphere ; as I have already told him, it doubtless became changed into sulphuric acid and passed into the sea. He then says "it may safely be asserted that there is fully as much (if not more) sulphur than chlorine" in nature, and that according to my hypothesis, the sea would become a solution of sulphate of soda. *Very safely asserted* indeed, since Mr. Forbes takes care to tell us that the sulphur in the form of dense metallic sulphids went to the centre of the earth, which I have shown, I think, good reasons for not believing. As it is, we have only to consider the quantities of sulphids and sulphates in the rocks and waters to see the absurdity of his remarks.

He next proceeds to discuss the theory of the origin of carbonate of lime. I have said that with the exception of that derived from the subærial decomposition of primitive calcareous silicates,

all of the carbonate of lime of the earth's surface has been formed from the decomposition of the soluble lime-salts of the sea, by carbonate of soda (and other soluble carbonates.) I, moreover, lay down the proposition that "animals can only appropriate the carbonate of lime already formed." In the face of these quotations, cited by Mr. Forbes, he says, as if charging me with holding the view, that if limestones were "formed by precipitation, they would have, from the moment of their deposition, a decided crystalline structure," while "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*;" this will probably be surprising news for Mr. Sorby, and a decisive blow for those who question the organic nature of *Eozoon*. I am prepared to go as far as any reasonable man in asserting the organic origin of limestones, and have, as every one must see, implied the intervention of organic life, when I say, "animals appropriate the carbonate of lime, etc." The question is, however, whence comes the carbonate of lime to supply the wants of these animals? Mr. Forbes declares that "zoologists believe that marine animals can utilize the other salts of lime existing in the ocean," evidently the sulphate or the chlorid of calcium once so abundant there. Will Mr. Forbes or the zoologists explain what has become of the acids once combined with the lime which has built up the thousands of feet of limestone, chiefly fossiliferous, which are found in the earth's crust? The only plausible chemical explanation is that which I have given, namely: that the chlorid of calcium has been decomposed by carbonate of soda derived from decaying feldspathic rocks, giving rise thereby to common salt and to the carbonate of lime which has supplied the marine animals.

As regards the question of the origin of dolomites, which Mr. Forbes next proceeds to notice, he will do well to consult my paper on the subject in the *American Journal of Science* for July, 1866, ([2] XLII, 49). In this, at §112, he will see that, apart from the formation of stratified sedimentary dolomites, I insist upon the frequent occurrence of dolomite as a mineral of secondary deposition, lining drusy cavities, filling veins, and even the moulds of fossil shells. To such cases the observations of Sorby may possibly refer. I can find no other account of his researches than the brief note in the Proc. of the Brit. Assoc. for 1856, cited by Mr. Forbes. Although I have a great respect for Mr. Sorby as an investigator, I have very little for the old theory of dolomitization of sedimentary limestones. No one who has carefully studied, as I have done for

years, the distribution and association of the great beds of dolomite which occur in the Lower Silurian rocks of Canada and New England, can for a moment admit that these are the products of subsequent alteration. Repeated alternation of pure blue limestones with reddish ferruginous dolomites, interrupted beds and patches of these enclosed in the former, the line of demarcation sharply drawn, and finally conglomerates in which pebbles of pure limestone are enclosed in beds of dolomite, are incontrovertible evidences against the theory of the dolomitization of limestones, and in favor of the deposition of dolomites as magnesian sediments. (Geology of Canada, 1863, page 612).

Mr. Forbes, in a note, insinuates that I am unaware of the various speculations and theories which have been put forward to explain the supposed origin of dolomite by alteration. Although the stratigraphical relations of dolomite, as described above, completely contradict this hypothesis of its origin, at least in the great majority of cases, Mr. Forbes will find that the observations and speculations of Haidinger, Von Morlot, Marignac, and others, on this subject have been fully discussed and made the subject of multiplied experiments by me in a memoir published in 1859, (*Amer. Jour. Science*, [2] XXVIII. 170, 365,) and later in the paper quoted above, and that I have shown by many experiments that the action of sulphate of magnesia on carbonate of lime, alluded to by Haidinger and Von Morlot before Harkness or Regnault, does not give rise to dolomite, but to carbonate of magnesia, which remains mechanically intermingled with sulphate of lime and any excess of carbonate of lime.

Some of the results of my prolonged study of certain of the salts of lime and magnesia, which are, for the most part, set forth in the papers just referred to, were, says Mr. Forbes, by me considered worthy of being presented to the French Academy (*Comptes Rendus*, April 22, 1867), although he declares the reactions therein described, to have been for more than twenty years in general application, on a large scale in Great Britain for the manufacture of magnesia salts. Here it becomes difficult to admit the plea of ignorance which suggests itself for most of Mr. Forbes's previous errors and mis-statements. I have, in the note to the French Academy, above referred to, pointed out the following facts, discovered by my investigations of the salts of lime and magnesia:—
1st. That bi-carbonate of lime, at ordinary temperatures, decomposes solutions of sulphate of soda and sulphate of magnesia, with formation of sulphate of lime and bi-carbonates.

2nd. That from mingled solutions of sulphate of magnesia and bi-carbonate of lime, there separates by evaporation, crystalline gypsum, and, subsequently, a hydrous carbonate of magnesia; the bi-carbonate of this base being, as is well known, very much more soluble than either the sulphate or the bi-carbonate of lime. 3rd. That this separation of gypsum is favored and rendered more complete by an atmosphere impregnated with carbonic acid gas; and 4th, that mixtures, in due proportions, of precipitated carbonate of lime and hydrous carbonate of magnesia, when gently heated under pressure, and in the presence of water, unite to form the anhydrous double carbonate, dolomite. These are the reactions which I described to the French Academy as *new*, and as forming the basis of a reasonable theory of the origin of gypsums and of dolomites. I now demand Mr. Forbes to make good his bold assertions to the contrary, or to show that any one of them has been employed for the last twenty years in the manufacture of magnesian salts.

Mr. Forbes then proceeds to inform us 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." Is Mr. Forbes aware that a large proportion of the 4,750 feet of limestone measured by Sir William Logan in Canada, and constituting the three great limestone formations of the old Laurentian system, is magnesian, and often, through great thicknesses, a pure dolomite, that a large part of the Lower Silurian system, and nearly the whole of the Upper Silurian, from the St. Lawrence to the Mississippi, consists of dolomite, and embraces great gypsum beds; and, finally, that immense gypsum deposits found, at intervals, from Nova Scotia to the Ohio, lie at the base of the Carboniferous system, in which latter only are found the *first remains* of air-breathing vertebrates? It is dangerous to generalize from the geology of the British Islands, or of a small part of Europe. Moreover, will Mr. Forbes attempt to demonstrate that at the time when Tertiary gypsums were deposited in the Paris basin, there did not yet remain sufficient carbonic acid in the air to modify its chemical action on solutions of bi-carbonate of magnesia, and give rise to the associated dolomites, which I was the first to discover in that position?

We now, in the language of Mr. Forbes, approach the question "of the igneous origin of eruptive rocks and of granite in particu-

lar." I asserted in my lecture the non-igneous origin of granite, maintaining that "the composition of the fused crust would have excluded free silica," and "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." With regard to the first of these statements, it is to be observed, that the primitive crust, holding, as we have seen, in the form of silicates, all the soda, lime, and magnesia which now appear in other combinations, must have been a highly basic rock; moreover, even, were it much richer in silica than we can suppose it to have been, there is no reason to believe that free silica, in the form of quartz, ever did or ever could crystallize from a fused slag, such as this primitive rock must have been. Quartz, in the shape of rock-crystal, or flint, when fused, or even when long exposed to a heat much below its melting point, is changed into an isomeric modification of silica, analogous to, if not identical with opal, and distinguished from quartz by a much less specific gravity, (about 2.2 instead of 2.65), the absence of crystalline structure, and a much greater solubility in alkalis and hydrofluoric acid. Silica crystallized in the form of quartz, has, it is true, been repeatedly obtained by different reactions, but never hitherto, except in the presence of heated water or of watery vapor. Heinrich Rose, to whom we are indebted for a careful study of this subject records, that Gustaf Rose, having submitted to partial fusion a granite, rich in quartz, obtained a glass or obsidian, in which were enclosed unmelted portions of the quartz, converted, however, into the less dense and more soluble opal-like form. These facts in the history of silica were regarded by H. Rose, as decisive against the notion of the igneous origin of granite, which he concludes to be incompatible with the actual state of our chemical knowledge. His paper on this subject, which should be read by every geologist, appeared in Poggen-dorf's *Annalen* for September, 1859, and a careful abstract of it will be found in the *L. E. & D. Philos. Mag.* for Jan. 1860. ([4] XIX. 32.) The new view of the origin of granite, which is there cited, as maintained "particularly by Mr. Sterry Hunt," is, that all granite rocks are derived from the alteration of sediments, containing besides feldspathic or argillaceous elements, quartz, derived, as I have explained, by the action of acid solutions at high temperatures on the primitive crust of silicates.

The geological evidences are multiplied that gneiss, which does not differ mineralogically from granite, and in its coarser varieties

is constantly confounded with it, is the result of the alteration *in situ* of sedimentary rocks consisting of quartz with feldspathic or argillaceous matters, the debris of pre-existing rocks. Moreover, it is demonstrable that these stratified sediments have been softened, and while in such a condition, displaced or extravasated, and have thus taken the form of exotic or eruptive rocks. Having by this or other means lost the mechanical evidences of their former stratified condition, they are called granites. The same view is, according to me, applicable to dolerites, diorites and trachytes. Modern lavas have no other origin, but take a different form, because they come to the surface and are rapidly cooled, instead of being slowly solidified under the pressure of superincumbent strata. The fact that every eruptive or exotic rock (with the exception of certain rapidly cooled lavas) has its mineralogical equivalent among indigenous crystalline rocks, that is to say among sedimentary strata of chemical or mechanical origin, is a powerful argument in support of the view here put forward. In connection with this, I have shown that a combination of chemical and mechanical agencies naturally and inevitably leads to the division of aqueous sediments into the two great types to which lithologists refer all eruptive rocks, namely, the acid, granitic or trachytic, and the basic, doleritic groups, which are supposed to form the two zones of igneous rock imagined by Phillips, and since insisted upon by Durocher, Bunsen and Forbes. As all of these crystalline rocks are, according to my hypothesis, ancient sediments, it follows that water has been present among them from their first deposition, and during all the subsequent processes of their heating, softening, crystallization and ejection — a view constantly insisted upon by me, and in accordance with the ideas maintained by Scheerer and subsequently by Sorby. This theory of igneous rocks, although suggested by Keferstein in 1834, and by Sir J. F. W. Herschel in 1837, has been elaborated by me in various papers for the past ten years.

See Theory of Igneous Rocks and Volcanos, *Canadian Journal*, March, 1858; some Points in Chemical Geology, *Quar. Jour. Geol. Soc.* Nov. 1859; Chemistry of the Earth, *Comptes Rendus*, June 9th, 1862; Chemistry of Metamorphic Rocks, *Dublin Quar. Jour.*, July, 1863; Contributions to Lithology, Part I, *American Jour. Science*, March, 1864.

In view, then, of my theory of the derived and sedimentary origin of all eruptive rocks, what does Mr. Forbes mean when he inquires whether I am aware of the immense masses "of volcanic

rocks (trachytes) scattered all over the face of the globe, which contain abundance of free quartz?" If he will refer to my Contributions to Lithology, just cited, he will find that I have insisted upon the presence of quartz in trachytes, and, also, upon the fact that such trachytes pass into granites, from which they differ only in structure (*Amer. Jour. Science* [2] XXXVII, 260). The obvious conclusion to be drawn from the presence of quartz in granites and trachytes, is that neither during nor subsequent to crystallization, have these rocks been subjected to a temperature sufficiently elevated to alter the quartz in the manner observed by Rose. In the paper last cited, I have devoted two pages to an analysis of the beautiful researches of Sorby on the microscopic structure of crystals, about which Mr. Forbes talks, though evidently without any conception of their geological bearing. Mr. Sorby, who makes of the cavities partially filled with watery solutions, which occur in many crystals, thermometers which registered the temperature at which these crystals were formed, concludes that the quartz, mica, feldspar, and tin-stone of the Cornish veins "were deposited from water holding various salts and acids, at temperatures varying from 200° centigrade to a low red heat," about 340°; while for some minerals from Vesuvius, which present, besides cavities holding liquids, others filled with stoney and glassy matters, he deduces a temperature of from 360° to 380°, and concludes them to have been formed "at a dull red heat, under a pressure of several thousand feet of rock, when water, containing a large quantity of alkaline salts in solution, was present, along with melted rock and various gases and vapors. I therefore think (he says) we must conclude provisionally, that at a great depth from the surface, at the foci of volcanic activity, liquid water is present along with the melted rocks, and that it produces results which would not otherwise occur." (*Quar. Jour. Geol. Soc.* XIV, 483.) One of these results, as is evident from the above citation, is the reducing of rocky matters to a *melted* condition, at a *dull red heat*, a point to be borne in mind when Mr. Sorby speaks in his paper of *igneous fusion* in this connection. A true igneous fusion of such matters, *without water*, would, as every one knows, require a vastly higher temperature; and I have elsewhere, after Scheerer, described this softening of mineral matters under the combined influences of water and heat, as an *igneo-aqueous fusion*.

Mr. Sorby has, moreover, calculated the temperature at which the quartz crystals in the trachyte of the Ponza Islands, cited by Forbes,

were formed, and finds it to be 360° , they being, in fact, generated under like conditions with those of the quartz of granite veins, from which Mr. Sorby rightly concludes to a similarity of origin between trachytes and granites. That both have crystallized at temperatures not above dull redness, under great pressure, and in the presence of water, is precisely what I have always maintained. When it is remembered that copper, gold, and silver require for fusion, from 1000° to 1400° , and quartz a temperature of 2800° , it may not be thought incorrect for me to designate 360° , the highest assigned by Mr. Sorby for the crystallization of quartz, as a "comparatively low temperature," to which expression, however, Mr. Forbes takes exception. Mr. Sorby further concludes from his investigations of crystalline metamorphic schists, that they must have crystallized at about the same temperature as the granites, affording, in his words, "a strong argument in favor of the supposition that the temperature concerned in the normal metamorphism of gneissoid rocks, was due to their having been at a sufficiently great depth under superincumbent strata."

The reader may now judge how far the views of Mr. Sorby, whom Mr. Forbes invokes, differ from my own on the subject of metamorphic rocks, of which I say in my lecture, as quoted by Mr. Forbes, that they have been formed from ordinary sedimentary strata, "depressed so that they come within the action of the earth's central heat," a proposition which our critic thinks "may be disputed." What theory he substitutes, he does not deign to inform us, but proceeds to ask how I explain the depression of strata on the surface of a globe with a solid centre. Both in my lecture and in the papers already cited, I have taken pains to explain that the deeply buried layers of sediment, together with the superficial and water-impregnated portions of the solid nucleus, constitute a *softened or plastic zone*, from which all plutonic and volcanic rocks proceed, and which allows of the movements observed in the solid crust. Is Mr. Forbes aware that geology affords many examples of depression of the earth's surface over great areas, permitting accumulations of sediments, to the extent of 40,000 feet or more, followed by elevation of the yielding crust and denudation to as great an amount?

I here take occasion to call attention to an important consideration in this connection deducible from Mr. Sorby's admirable Bakerian lecture before the Royal Society, in 1863, on the Direct Correlation of Mechanical and Chemical Forces, in which he shows how chemical action is produced by mechanical force. Stating

from a consideration of the results of Bunsen and Hopkins that those bodies which expand when fused have their point of fusion raised by mechanical pressure, and from the discovery of Sir Wm. Thompson that water, which contracts in melting, has, on the contrary, its melting point lowered by pressure, we may say that as the solution of a solid in a liquid is a kind of fusion, the same general law will hold good, and that, for all those salts which contract in dissolving (to which rule there are very few exceptions), the solubility should be increased by pressure. This was abundantly established by the experiments of Mr. Sorby. If now, we suppose that the mineral compounds of the crystalline rocks, like most salts, occupy a less bulk when dissolved than when in the solid state, we can understand the greatly increased solvent power of the water present in sediments submitted to a pressure equal to many thousand feet of rock. Moreover, as suggested to me by Sir William Logan, the diminution of solvent power of the liquid as the pressure is removed, will help to explain the deposition of mineral matters from watery solutions, which in their upward flow through fissures in the earth's crust, have given rise to mineral veins.

But Mr. Forbes, after considering the conclusion of Sorby that water has played an essential part in the crystallization and softening of rocks, which have been effected at temperatures not above low redness, charges me with "sensation" writing in asserting that the plutonists claimed that igneous rocks were formed "entirely by fire," and accuses me of injustice to the memories of Hutton, Playfair, Hall, Humboldt and Von Buch, whose writings "show that they never overlooked the all-important influence of water." Now I mentioned none of these geologists in my lecture. As to Hutton, to whom belongs, I believe, the idea of the metamorphic origin of the crystalline schists, I have elsewhere written (*Dublin Quar. Jour.* July, 1863), "I accept in the widest sense the view of Hutton and Boué that all the crystalline stratified rocks have been produced by the alteration of mechanical and chemical sediments." The question before us is, however, neither the views of Hutton nor yet the origin of metamorphic rocks, but what both he and modern plutonists hold with regard to the origin of granite, whose derivation from metamorphosed sediments neither he nor they admit. With regard to this point Mr. Forbes elegantly says "*the idea of dry fusion could only have originated in the brains of their antagonists.*" Farther in a note to a paper on the Microscope in Geology, in the *Popular Science Review* for October, he says, with

equal good taste and truth, "the idea of a true dry fusion in nature exists only in the brains of the ultra-neptunist or the luke-warm hydrothermalist," and asserts that in igneous action, the agency of water was always recognized. He alludes to Poulett Scrope, who in 1824, put forth his views on the intervention of water in giving liquidity to lavas; but as Mr. Scrope himself tells us in his late paper (*Quar. Jour. Geol. Soc.* XII. 343,) his views were declared to be unchemical, discredited and ridiculed; nor was it till in 1847, when Scheerer published his remarkable essay on the origin of granites, (*Bul. Geol. Soc. Fr.* [2] IV. 468,) that lithologists began to admit that water had intervened in the generation of granite and other eruptive rocks. But our readers shall judge what value is to be attached to Mr. Forbes's assertions in this matter. After Scheerer's view of aquo-igneous liquidity had been made known to the Geological Society of France, Durocher, as the champion of the plutonists, maintained in opposition to it, the hypothesis already referred to of a separation of the yet liquid globe into two layers, the lower one heavier and basic, the upper lighter and acid, which by its solidification gave rise to granite. While he declared that "Scheerer's new theory has for its principle the introduction of water in the solidification of granitic rocks," Durocher conceived all the water found in eruptive rocks to have been subsequently absorbed by them, (*Bul. Soc. Geol.* [2] IV. 1029, 1032.) Rivi re, following a second communication by Durocher on the same subject, declares, "I think with Durocher, that water has played no part (*n'a jou  aucun r le*) in the formation of granite," and as to the rocks considered by Scheerer (granites etc.,) asserts "the geological position of these absolutely excludes the intervention" of water, (*Ibid.* [2] VII. 287.) I might further quote Fournet, who in his *G ologie Lyonnaise*, strongly maintains similar views to the above, and invokes in favor of his purely igneous theory, the results and the statements of Hutton and Hall. Lest, however, there should be any mistake, and that the advocates of dry fusion, Durocher, Rivi re and Fournet, be, after all, ultra-neptunists, I shall cite Elie de Beaumont, who in his classic essay on Volcanic Emanations, etc., (published in 1847,) *Bul. Geol. Soc. Fr.* [2] IV. 1249, has admirably discussed the question before us, giving the views of Fournet and Durocher, the former of whom explains the liquidity of granite by a surfusion of the quartz, which melts at 2800  centigrade, but remains viscid at much lower temperatures on cooling; while Durocher, on the contrary, imagines "a sort of fusible alloy" of the various elements, from which the feldspar and

mica crystallized. Rejecting these, which he designates as "*purely igneous surfusion*," he declares in favor of Scheerer's "*altogether novel idea*," of a condition of quasi-fluidity at a low red heat, due to the intervention of water, and asserts that "the hypothesis of a primitive state of *simple igneous fusion of granite*, notwithstanding the evidences brought forward in its favor, is no longer justified," (*loc cit.* p.p. 1305, 1311.) It is in the face of records like these, and despite the energetic protest of plutonists against the possibility of the intervention of water, and in favor of a dry fusion or a simply igneous fusion of the elements of granite, that Mr. Forbes has the hardihood to assert that the intervention of water in igneous agency "was always recognized by the plutonist."

But I have not done with Mr. Forbes until he shall have shown how, with his own theory of the earth, he explains the intervention of water in all igneous rocks, which, as he declares, are outbursts from the still fluid interior of our globe. How did the water find its way there, since, according to him, far above the already solidified crust, this element at first formed a vaporous layer, separated from the earth by a stratum of volatile chlorids and another of carbonic acid? In virtue of what law did this water, after its precipitation, diffuse itself throughout the various layers of the liquid mass which still fills the centre of the earth, so as to be present in every eruptive rock coming up from that great reservoir? For my part, I am inclined to say with Rivière, that the geological position of such matters must "absolutely exclude the intervention of water," and until Mr. Forbes, or some other plutonist, shall have given a plausible hypothesis to explain the fact, which he admits, of the universal diffusion of water in igneous rocks, I prefer my own theory of their origin, namely: that the anhydrous and incandescent nucleus of the globe is solid, and, except in its outer portions, takes no part in volcanic or plutonic phenomena, which have their origin entirely in the stratified sedimentary deposits, and in those superficial portions of the nucleus which were necessarily permeated, during their partial cooling and consequent contraction, by the superincumbent waters.

One word in conclusion; Mr. Forbes, who prides himself on his great opportunities of travel, and on his geological studies in various regions, discourteously taunts me with my own more limited field of investigation. Let me tell him in reply, that if the three papers which he published in October last, show any one thing more clearly than his unfamiliarity with geological literature, it is

his ignorance of the facts of geognosy, and that he involuntarily recalls to mind the wise saying of Thomas à Kempis, passed into a proverb among churchmen: "those who make many pilgrimages rarely become saints."

Montreal, December, 1867.



ON THE CHEMICAL GEOLOGY OF MR. DAVID FORBES:

BY T. STERRY HUNT, F.R.S.

In the *Chemical News* of October 4th, 1867, there appears a paper purporting to be a criticism of some views on the chemistry of the primeval earth, put forward by me in a lecture delivered before the Royal Institution of Great Britain, on the 31st of May last, and published in the proceedings of that Institution, as well as in the *Chemical News* of June 27th, and the *Revue des Cours Scientifiques*, of October 19th, as also in *Les Mondes* and *Cosmos*. The object of my present communication will be to notice briefly some of the criticisms of Mr. Forbes. The readers of my lecture are aware that I assumed as my starting point the hypothesis, now generally accepted, of the origin of our earth, and of all planetary and stellar worlds, by a process of condensation and cooling from a nebulous or a gaseous matter, so intensely heated as to be luminous, and to contain, at the same time, in a free or dissociated condition, the various chemical elements. The first objection of Mr. Forbes, is that I do not explain the origin of this intensely heated condition: a consideration entirely beyond the scope of my lecture, but established by the spectroscope, and to be accepted as an ultimate fact, the secret of which, like that of the origin of matter itself, rests with the great first cause.

In discussing the laws which presided over the cooling of our own globe, I gave several reasons which have led modern investigators to reject the old theory of a liquid centre covered by a thin crust of congealed rock. I alluded briefly to the mathematical deductions of the late Wm. Hopkins from the phenomena of precession and nutation,—those of Archdeacon Pratt on the feeble resistance which would be offered by a crust of the thickness generally admitted by the old school, to the crushing weight of masses like the Himmalyah Mountains,—and the conclusions of Thompson as to the rigidity of the earth, deduced from the theory of the tides, as so many concurrent arguments in favor of a crust at least many hundred miles in thickness, if not of a globe entirely solid. Proceeding, thence, to consider the conditions of cooling presented by the fused and oxydized mass of the globe, I asserted that the analogies offered by most of the bodies forming the earth's crust, which yield compounds considerably denser when solidified than when in their fused condition, lead us to conclude that the solidification of the globe must have begun from the centre. In fact, the numerous and detailed experiments of Charles Deville, (*Comptes Rendus*, XX. 1453) and those of Delesse, (*Bull. Soc. Geol. de Fr.* [2] IV. 1380.) not to mention the earlier ones of Bischof, unite to show that the density of fused rocks is much less than that of the crystalline products resulting from their slow cooling, so that, as Saemann has justly observed, we are forced to conclude that the crystalline stoney masses formed at the surface of a liquid globe must sink towards the centre (*Ibid.* Feb. 4. 1861). To this conclusion Mr. Forbes objects that, in the cooling of sulphur or metals from fusion, a crust forms at the surface before the interior is solidified; he should consider that the conditions in a small crucible, placed in a cold atmosphere, where cooling is rapid, and the crust is supported by adhesion at the edges, are vastly different from what would obtain in a world-wide bath, cooling with great slowness beneath an intensely heated atmosphere. In such a case, as the crystalline silicates known to us are, according to numerous experiments, from one-seventh to one-sixteenth denser than the same materials in a fused condition, it would require a suspension of the laws of gravity to counteract the inevitable tendency of the heavier solids formed at the surface to sink in the fused mass, in which they would subside as naturally as the crystals which form at the surface of an evaporating basin of brine. The analogy holds good, since the crystals formed at the surface, whether by evaporation or by cooling obey the laws of gravity.

The freezing over of the surface of such a mass would be as unnatural as the freezing of a lake of water from the bottom.

Mr. Forbes next comments upon my allusion to the experiments of Hopkins on the effect of pressure in elevating the melting points of such bodies as contract in cooling, and says that I appeal to these as conclusive proof that the melting points of bodies do become (*ad infinitum*) elevated in proportion to the pressure. In fact, I said nothing of the sort, but insisted that the researches of Hopkins "are to be considered in this connection." If Mr. Forbes had taken some pains to inquire into the question, he would learn that these experiments of Hopkins, and others (by W. Thompson on the effect of pressure in reducing the melting point of ice) were suggested by a remarkable essay by James Thompson (Trans. Roy. Soc. Edin. XVI, part 5). In this it was shown that the fusing point of ice, which contracts in melting, must necessarily be reduced by pressure; while, as Sir Wm. Thompson showed, the reverse effect was to be expected for all solids which expand in melting (L. E. and D. Phil. Mag. [3] xxxvii. 125). The results of Hopkins thus come under a general physical law. Mr. Forbes will find a simple and intelligible statement of the principle laid down by Thompson, and Hopkins's argument therefrom for the solidity of the interior of the globe, in the fourth of Dr. Tyndal's admirable lectures on Heat, delivered before the Royal Institution. See also Mr. Sorby's Bakerian lecture for 1863. As to Mr. Forbes's suggestion of denser matters towards the earth's centre, I have said the same thing in my lecture.

Mr. Forbes next proceeds, in his own words, to submit my views of the chemical changes which took place at the surface of the globe, to "careful scrutiny," in order to determine whether "they are sound and likely to meet with acceptance in the chemical world." Of the critic's fitness for his self-imposed task the reader shall judge. The first thing to be determined in the cooling of an intensely heated vaporous mass is the nature of the chemical compounds which would be formed among the dissociated elements. As I have stated in my lecture, the combinations stable at the elevated temperature then prevailing, would be first formed. The affinities of oxygen are such, that under such conditions, an excess of this element being present, instead of sulphids of the heavy metals, as imagined by Mr. Forbes, oxyds and sulphurous acid would be produced in virtue of affinities known to every chemist and metallurgist. So with regard to chlorine, the production of alkaline chlorids in such conditions is inconceivable, since in

the conjoined presence of oxygen, hydrogen and silica, an alkaline silicate and hydrochloric acid would necessarily result. Even if, as Mr. Forbes supposes, chlorid of sodium were to be formed in the heated atmosphere, it would be precipitated into an intensely heated bath of fused silicates, covered by an atmosphere charged with aqueous vapor, or with mingled hydrogen and oxygen, and would immediately undergo the same decomposition that takes place when the vapors of common salt are diffused through the heated atmosphere of a potter's kiln, or, as in Mr. Gossage's new soda-process, are passed with steam over red-hot flints. In both cases silicates of soda are formed, with separation of hydrochloric acid. These considerations lead to the conclusion that, after all the more fixed elements were precipitated, the whole of the chlorine would remain as hydrochloric acid, and the whole of the sulphur as sulphurous acid, together with a large proportion of oxygen, since we find sulphates and not sulphites in the sea-waters. To this constitution of the still intensely heated atmosphere, Mr. Forbes objects, and inquires whether it is "at all probable, even if possible, that an excess of oxygen could exist along with the vast amount of sulphurous acid." He farther adds that "the improbability of such an atmosphere containing a mixture of hydrochloric and sulphurous acids, may be inferred from Dumas's researches; that chemist having long ago shown that these two gases, when mixed together, react and mutually decompose each other, with the formation of water, chlorine and sulphur." Mr. Forbes thinks he has hit upon two objections to the existence of a heated atmosphere holding, as I have endeavored to show, besides nitrogen, oxygen and watery vapor, sulphurous and hydrochloric acids. He has evidently a vague notion that sulphurous acid and oxygen have an affinity for each other, and ought to form together sulphuric acid. So they do unite slowly at proper temperatures, in the presence of water, being converted into oil of vitriol, and it was doubtless in this way, as I have elsewhere shown, that the sulphur was eventually brought down from the atmosphere, and formed the sulphates of the sea. But every chemist is aware that at higher temperatures oil of vitriol is resolved into water, sulphurous acid and oxygen gases, and that this reaction is made use of as an economical process for the preparation of oxygen on a large scale, the sulphurous acid being removed by absorption from the cooled gases. As regards his second point, Mr. Forbes, who cites Dumas (*Traité* i. 146) has been misled by quoting at second-hand, apparently from the English edition of

Gmelin (ii. 321). Dumas states that in solution sulphurous acid and hydrochloric acid undergo no change; but, "in a dry state, on the contrary, they are rapidly decomposed, at least in operating over mercury." It may be true that as Gmelin states, water, chlorine and sulphur result, but such is not the assertion of Dumas. The point, however, is immaterial, since as Dumas and Gmelin state, and as every chemist knows, the two gases remain unaltered in the presence of water, even if in the form of vapor. Indeed, it happens, unfortunately for both of Mr. Forbes's objections, that large quantities of precisely such an atmosphere as he supposes to be impossible, are disengaged from numerous volcanic vents, as he will find by referring to the researches of Charles Deville and Leblanc. (Ann. de Ch. et Phys. [3] LII. pp. 5-63). Among other examples described by these chemists, a *fumerolle* of Vesuvius yielded in June, 1856, a mixture of highly heated steam, hydrochloric acid and air, the latter containing in 100 parts oxygen 18.7, sulphurous acid 2.6, the remainder being nitrogen; while the acids of the steam and air together yielded for one part of sulphurous acid about five parts of hydrochloric acid. Traces of sulphuric acid, due to the slow union of the sulphurous acid and oxygen, were found in the water condensed from this *fumerolle*. Volcanos, as I have elsewhere stated, reproduce, on a limited scale, the conditions of the primeval earth, not only in their solid but in their gaseous products.

Mr. Forbes proceeds to comment upon my illustration of the condition of the primitive globe from a supposed reaction of the present air, sea and land under the influence of intense heat. He suggests that the carbonaceous matters would convert into sulphids the mineral sulphates. Here, as before, he ignores the intervention of water and silicious matters, which would cause the sulphur to escape in the form of sulphuretted hydrogen, (which is doubtless evolved from modern volcanos by a similar reaction), and this at an elevated temperature, would at once be burned to sulphurous acid and water. He descends to trifling when he objects that by the effect of heat upon the present surface of the globe, the water of the sea would be first evaporated, and then the chlorid of sodium sublimed in its turn. It was made clear to every reader, that I never intended by this illustration to represent the process of nature; moreover, I said, "*if the elements were made to react upon each other,*" which would not be the case if they were successively removed by evaporation beyond the sphere of reactions.

Here I cannot resist the temptation of giving my readers a choice specimen of Mr. Forbes's chemistry, which he has embodied, with many other surprising things, in a further criticism of my lecture, which appears in the *Geological Magazine* for October, but has, for some unknown reason, withheld from the readers of the *Chemical News*. Proceeding to give his own notions of the chemistry of the primitive globe, Mr. F. supposes that immediately above the "solidified crust," there existed a zone composed chiefly of chlorid of sodium; "above this, a stratum of carbonic acid, and then of water in the form of steam, whilst the oxygen and nitrogen would be elevated still higher;" and, probably, also, in Mr. F.'s imagination, separated according to their densities. In explanation of this order, he tells us, in a note, that the zone of carbonic acid gas would be heavier than that of steam, and should therefore come below it; he even gives their respective weights, but he forgets that oxygen and nitrogen are also both heavier than steam, and should be found below, and not above, this zone of watery vapor. In fact, as is well known, the specific gravity of oxygen being 1.109, and nitrogen, 0.970, that of atmospheric air is 1.000; while carbonic acid gas is 1.525, and that of watery vapor, 0.624. But, apart from this absurd mistake, what shall we say of the man who displays an utter ignorance of the laws which govern the diffusion of gases and vapors? Will Mr. Forbes explain why it happens that in our present atmosphere, these same elements, namely, oxygen, nitrogen, carbonic acid gas, and watery vapour, are commingled, instead of being, as he would have them, arranged in separate zones?

Mr. F.'s mode of explaining the saltness of the sea must fall to the ground, unless he succeeds in showing how, despite well known chemical affinities, the requisite amount of chlorid of sodium could be formed and preserved under the conditions which I have discussed above, so that, as he supposes, it was ready to be dissolved by the first waters precipitated on the surface. When he has satisfactorily established this part of his theory, he will, perhaps, tell us how sulphates found their way into the sea, if, as he asserts, all the sulphur was at first separated in the form of dense metallic sulphids, which sank at once, "and remained in the interior of the earth, protected from oxydizing action?" Mr. F. may have data unknown to the world, for estimating the total amount of sulphur in the globe; but when he tells us that it would be sufficient to convert all the soda of the sea to sulphate, he reasons as if the amount of bases in nature were limited, for-

getting that the earth's crust contained more than enough of alkalis, lime, and magnesia, to saturate the acids of the primeval atmosphere, and, moreover, that the whole of the sulphur, sulphates, and sulphids of the earth's crust, have, to judge from all analogy, been derived from the soluble sulphates of the ocean.

Mr. F. next proceeds to inquire why the sea contains so much sodium, and so little potassium? If he will study the question, as he may do in my *Contributions to the Chemistry of Natural Waters* (Amer. Jour. Sci. [2] xxxix, 176; xl, 43, 193.), he will learn that at an early period the salts of calcium and magnesium greatly predominated over those of the alkalis in the ocean waters, precisely as they must have done in the crust of the primitive earth. It is by subsequent subærial decomposition that have been liberated the alkalis, which, in the form of carbonates, have decomposed the salts of the primitive sea, and substituted sodium for calcium, for it is well known that natural alkaline waters convey to the sea chiefly soda, and comparatively little potash, which is retained by argillaceous sediments. Moreover, the potash which does find its way to the sea, is constantly withdrawn in the form of glauconite, and also by the agency of fucoids, which, as Forchammer has shown, fix great amounts of potash, and, subsequently, by their decay in the ooze, restore it to the earth.

Mr. Forbes next expresses surprise that I find the origin of all carbonate of lime (except that from the subærial decomposition of primitive calcareous silicates) in the reaction of carbonate of soda on the lime-salts of sea-water, since, according to him, the results of the careful study of limestone rocks by geologists, paleontologists and microscopists have shown these rocks to be "*the result of organic action.*" And, moreover, that neither chemists nor zoologists will accept my assertion that *animals can only appropriate the carbonate of lime which they find ready formed*, but "will consider these animals capable of utilizing the other lime-salts in the sea." If we admit the power of the lower animals to decompose chlorid of calcium or sulphate of lime, as would appear from the acid liquid said to be found in some of them, will Mr. Forbes tell us what becomes of this at the death of these animals, and how the acid is to be disposed of? If the thousands of feet of limestone strata, consisting in large part of organic remains, have been derived from the decomposition of the sulphate or chlorid of calcium of the sea by any other process than by that which I have indicated, namely, the intervention of alkaline carbonates, will

Mr. Forbes kindly inform us what has become of the vast amount of hydrochloric acid equivalent to all this carbonate of lime ?

As to the origin of dolomites, Mr. Forbes will do well to read my paper in the *Amer. Jour. Science*, for July, 1866 ([2] XLII, 49). In this, at § 112, he will see that apart from the formation of stratified sedimentary dolomites, I insist upon the frequent occurrence of dolomite as a mineral of secondary deposition, lining drusy cavities, filling veins, and even the moulds of fossil shells. To such cases, the observations of Sorby and of Harkness may probably be referred; the microscopical investigations of the former, as given by him in the British Association Report for 1856, are like all the other works of that excellent observer, doubtless entitled to the highest credit. No one, however, who has carefully studied, as I have done, the distribution and association of the great beds of dolomite which occur in the Lower Silurian rocks of Canada and New England, can, for a moment, admit that they are the products of subsequent alteration. Repeated alternations of pure blue limestones with reddish ferruginous dolomites, interrupted beds and patches of these enclosed in the former, the line of demarcation sharply drawn, and finally conglomerates in which pure limestone pebbles are enclosed in beds of dolomite, all of which may be studied near Quebec, are evidences incontrovertible against the theory of dolomitization of pure limestones, and in favor of the deposition of dolomites as magnesian sediments.

Mr. Forbes insinuates that I am unaware of the various speculations and theories which have been put forward to explain the supposed origin of dolomites by alteration. Although the stratigraphical relations of dolomites, as described above, set aside entirely this hypothesis of its formation, at least in the great majority of cases, Mr. Forbes will find that the observations and speculations of Haidinger, Von Morlot, Marignac, and others on this subject have been discussed and made the subject of multiplied experiments by me in a memoir published in 1859, (*Amer. Jour. Sci.* [2] XXVIII. 170, 365) and farther in the paper quoted above; and that I have shown that the reaction of the sulphate of magnesia on carbonate of lime, to which he refers, does not give rise to dolomite, but to an admixture of the carbonates of lime and magnesia.

Some of the results of my prolonged study of certain salts of lime and magnesia, which are for the most part set forth in the papers just referred to, were, says Mr. Forbes, by me considered worthy of being presented to the French Academy of Sciences (*Comptes Rendus*, April 22, 1867,) although he declares the reactions there described

to have been for twenty-five years in general application on a large scale in Great Britain, for the manufacture of magnesian salts. Here it becomes difficult to admit the plea of ignorance which suggests itself for most of Mr. Forbes's previous statements. I have in the note to the French Academy above referred to, pointed out the following as facts discovered by my investigations of the salts of lime and magnesia : 1st. That bi-carbonate of lime, at ordinary temperatures, decomposes solutions of sulphate of soda and sulphate of magnesia, with formation of sulphate of lime and bi-carbonates. 2nd. That from mingled solutions of sulphate of magnesia and bi-carbonate of lime, there separates, by evaporation, crystalline gypsum, and subsequently a hydrous carbonate of magnesia; the bi-carbonate of this base being, as is well known, very much more soluble than the sulphate or the bi-carbonate of lime. 3rd. That this separation of gypsum is favored and rendered complete by an atmosphere impregnated with carbonic acid gas, and 4th. That mixtures, in due proportions, of precipitated carbonate of lime and hydrous carbonate of magnesia, when gently heated under pressure, and in the presence of water, unite to form the anhydrous double carbonate, dolomite. These are the reactions which I described to the French Academy as *new*, and I demand Mr. Forbes to make good his assertion to the contrary, or to show that any one of them has been employed for the last twenty-five years in the manufacture of magnesian salts.

Montreal, December, 1867.

