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ORIGINAL ARTICLES.

I.—ON DR. STERRY HUNT'S GEOLOGICAL CHEMISTRY.

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IN considering the mutual relations of the sciences of Geology and Chemistry, the student must always bear in mind which of these two sciences is to form the basis or starting point for his inquiry, for this cannot fail to exercise an important influence on his reasonings and deductions.

In what Dr. Sterry Hunt calls my Chemical Geology,¹ I have taken Geology as my starting point, and then endeavoured to apply chemistry, especially experimental chemistry, to the explanation of known geological phenomena. On the other hand, Dr. Hunt, in what may be termed his Geological Chemistry, starts from data purely chemical, and then looks around for geological instances to which they may be applied.

Thus, for example, starting from the chemical fact, that a solution of carbonate of soda will throw down carbonate of lime from a solution of the chloride of calcium, he at once asserts that the whole of "the calcareous strata, the marbles and various limestones which we find on the earth's surface," have been precipitated from the sea by a solution of carbonate of soda.

And again, Dr. Hunt observing in the laboratory that the reaction of the compounds of magnesia with carbonic acid in a dense atmosphere of that acid could be turned to account in facilitating the separation of Dolomites and Gypsums, at once jumps at the conclusion "that all magnesian limestones and gypseous strata from the most ancient up to the Tertiary periods were formed in a dense atmosphere of carbonic acid." Now in face of these assumptions, I contend and I feel confident the Geological world will bear me out, that

¹ Here it should be explained that Dr. Hunt, from having some time back published both in England and France an outline of his principles of Chemical Geology, has thereby fairly laid himself open to having his views both criticised and disputed; whilst, on the contrary, Dr. Hunt's knowledge of my views on this subject could be only derived from the allusions to my opinions scattered through the two papers relating to this controversy in the GEOLOGICAL MAGAZINE of October 1 and the *Chemical News* of October 4 of last year. Although his virulent criticism might therefore be considered as hardly fair; still, so far from objecting to it, I feel truly thankful to Dr. Hunt for thus enabling me to strengthen the weak points, and inspiring me with more confidence than before in the *resumé* of the views on Chemical Geology put forth in a lecture to the Chemical Society, now in the press.

no Geologist whosoever could in applying the study of Chemistry to the explanation of the phenomena of his science ever by any possibility have arrived at such sweeping generalizations.

When the safety of Rome was endangered by the victories of Hannibal, the advice of Scipio to the Romans was to save Rome by attacking Carthage; and the papers of Dr. Hunt in the *Chemical News* of Jan. 17, and the *GEOLOGICAL MAGAZINE* of Feb. 1st, evidently prove that he is determined to pursue a similar course; yet I confidently trust with a different result, since in this case I believe the forces at command are fully adequate, both for offence as well as for defence.

In this discussion, however, much more trouble is likely to be caused to me by the method in which Dr. Hunt carries on his scientific warfare, and which seems to partake of the character of the country in which he resides, where the Indian system used to be, to worry out the enemy by skirmishing, but never to attack strong points; and the history both of scientific discussion as well as of nations has shown how very effective such a plan of operations may prove, even in the defence of a very weak cause.

For this reason, therefore, I have considered it wise to keep the main points under consideration as prominently in view as possible, and if possible not to allow the discussion to become so diffuse as to risk losing sight of them, which I fear the readers of Dr. Hunt's long communication may be likely to do. Acting upon this determination, therefore, I have in my reply to Dr. Hunt's paper in the *Chemical News* of Jan. 17, which also appeared in the *GEOLOGICAL MAGAZINE* of Feb. 1, given a plain and concise statement of the points, numbered 1 to 9, in which I have presumed to differ from Dr. Hunt's views; and as I now find nothing in his subsequent communication to the *GEOLOGICAL MAGAZINE* of February 1 which could in any way tend to shake my conviction of the unsoundness of these points, I must be content to wait until Dr. Hunt may condescend to bring forward new evidence in their defence.

If now, however, after a perusal of Dr. Hunt's paper in the February number of the *GEOLOGICAL MAGAZINE*, it is compared with his preceding communication in the *Chemical News*, it will be perceived, as the Editor of the *GEOLOGICAL MAGAZINE* has already observed, to be to a great extent the same, and in many parts even verbatim; and remembering Dr. Hunt's puerile accusation, that I, "for some unknown reason, withheld from the readers of the *Chemical News*" matter which I published in the pages of the *GEOLOGICAL MAGAZINE*, it is amusing to observe that Dr. Hunt has in like manner reserved for the readers of the *GEOLOGICAL MAGAZINE* several interesting observations which probably he may have considered (and with some reason) as beyond the capacity of the chemists who patronize the *Chemical News*,—among others, for example, the following: "As for the noble metals, whose compounds with oxygen are decomposed at elevated temperatures, their great volatility, as compared with earthy and metallic oxides, would keep them in the gaseous form till the last stage of precipitation of

earthly oxidised matters, when by far the greater part of the globe was probably solidified. Hence we now find them in the earth's superficial crust." And a little further, "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."

Chemists will not require any comments upon the above, for they are accustomed to regard Platinum, for example, as one of the most refractory bodies known, which, of course, cannot be the case now that Dr. Hunt has made this interesting discovery of its great volatility at a point at which silicates solidify; and further, they were not aware that the extreme refractory nature of the other metallic oxides had been so completely demonstrated, since some of them, at least, as Lead, Bismuth, Antimony, Molybdenum, etc., are not remarkable for that property; whilst geologists will not feel convinced from Dr. Hunt's mere assertion that the noble metals have from the beginning been in the earth's superficial crust, precipitated on to it from the skies like Jupiter's golden rain, but may also be inclined to believe that they may have been carried up from below.

The only important point which Dr. Hunt now advances is the courteous request for Mr. Forbes to explain "the intervention of water in all igneous rocks, which, as he declares, are outbursts from the still fluid interior of our globe." The above words do not exactly express my views, since I advance that igneous rocks have their sources in some "*reservoir or reservoirs*" of still fluid matter in the interior of the earth; and I would add that, by the actions of capillarity and heat, I see no difficulty in explaining the infiltration of the requisite amount of water for the supply of such a source. As, however, I could not even think of accusing Dr. Hunt of "unfamiliarity with geological literature," to use his own words, I could not suppose him ignorant of the writings of Daubrée, who, in Europe, at least, is regarded as somewhat of an authority on these subjects; Dr. Hunt will find this question fully answered by that gentleman, whose words are: "En résumé, sans exclure l'eau originaire, et en quelque sorte de constitution initiale, que l'on suppose généralement incorporée aux masses intérieures et fondues, M. Daubrée est porté à conclure de l'expérience ci-dessus relatée, que l'eau de la surface pourrait, sous l'action combinée de la capillarité et de la chaleur, descendre jusque dans les parties profondes du globe."

Always preferring, when possible, a reference to fact or experiment than to authority, I would advise Dr. Hunt, in order to form a conception of such strange action, to examine a common Gifford or other injector used to supply feed-water to a high pressure boiler, and he will soon perceive that the very forces which otherwise would prevent the entrance of the water into the boiler are the very means of forcing it in.

Dr. Hunt also asks me to remember "that the oldest known series of rocks, the Laurentian, consists of quartzites, limestones, and gneiss evidently of sedimentary origin and derived from still older sedimen-

tary rocks." When I was in Canada, what little I saw of the Laurentian rocks did not at all prove to me that they had been derived from *still older sedimentary rocks*, but, on the contrary, whilst believing that the Laurentian gneiss, quartzites, &c., were of metamorphic sedimentary origin, I inclined to the conclusion that the materials of which they had been reconstructed had most probably been the *debris* of eruptive igneous rocks, and this view I have maintained since 1854 with regard to some of the analagous Norwegian rocks which Dr. Hunt claims to be Laurentian. To refresh my memory, however, I have read over the description of the mineral characters of the Laurentian rocks in the Report of the Geological Survey of Canada, pp. 22-49, but can find no evidence whatsoever to the contrary—and, therefore, without disputing the correctness of Dr. Hunt's assertions on points where he ought at least to be confident, I would ask whether this statement is founded on facts or on hypothesis.

Dr. Hunt devotes a whole page to what appears to be an inquiry, as to who first showed that water played a part in igneous action, a subject which may be of personal or historical interest, but which is quite irrelevant to the questions under consideration; for all geologists will persist, notwithstanding whatever Dr. Hunt opines to the contrary, in regarding igneous action as volcanic action and volcanic action as igneous action, nor can they suppose for a moment that any person, except one who never had seen a volcano in eruption, could be blind to the evidence of his senses and deny the co-association of vapours and gases with volcanic action;—that the results of Mr. Scrope's admirable researches should have been discredited and ridiculed and declared unchemical, should be a warning in future to chemists not to hazard such opinions without having studied the subject in the field as well as in the laboratory.

As Dr. Hunt brings forward the question of the density of quartz, I may here state, what I omitted in my paper in the *Chemical News*, that all arguments based on this fact are completely invalidated by the fact that the specific gravity of crystallised quartz out of true volcanic lavas is 2.6, or the same as that of the quartz in granite; and, further, that Mr. Sorby's examination of the quartz out of these lavas completely proves that it was crystallized out of the melted rock, and not, as Dr. Hunt would have us infer, merely entangled from the *debris* of originally sedimentary strata.

Having long occupied myself with the application of the microscope in geology, and repeated most of Mr. Sorby's experiments relating to this subject, I consider it superfluous to contradict Dr. Hunt when he accuses me of not understanding Mr. Sorby's views, being quite content with that gentleman having expressed himself decidedly to the contrary. I would recommend Dr. Hunt also to commence with the study of microscopic geology, and can well imagine his being disconcerted when, on opening the last number of the GEOLOGICAL MAGAZINE, he found a few lines from Mr. Sorby, quite sufficient to annihilate all the deductions he had so elaborately arrived at from the study of that gentleman's memoirs, with the object of making them serve his own purposes.

All the other points have been noticed in my recent communication to the *Chemical News*, and I would merely state here that as regards Dr. Hunt's criticisms upon my views it is probable that many of them would not even have been advanced by Dr. Hunt had he waited until the outline of my views on Chemical Geology, now in the press, had appeared, instead of selecting scattered and disjointed sentences for attack, without giving the context. Thus, for example, when he accuses me of being ignorant of the laws of diffusion, he would have found my opinions expressed as follows:—

“Whilst, on the one hand, the zones formed in the earth are considered to have possessed a somewhat stable or permanent character, those formed in the atmosphere would, on the contrary, be the reverse, for no sooner had the gasiform products forming them, by in the first instance obeying the impulse of gravity so overcome the counter-acting tendency of the laws of diffusion of gases, than these latter would assert themselves, and, in process of time, entirely obliterate this arrangement.”

And again, “as before stated, this arrangement would be gradually obliterated by diffusion, but, as the element of time is of vital importance in considering the effects of diffusion, it is imagined that, before being obliterated, this arrangement may have had considerable influence in modifying the chemical re-actions which took place at this period in the earth's history.”

Dr. Hunt, whose knowledge of the laws of diffusion does not seem to include any appreciation of the importance of the element of time in their consideration, might just as well tell us that a lump of sugar could not reach the bottom of a tumbler of water because sugar will dissolve in water. As Dr. Hunt seems to have such respect for authorities on the subject, I will, with the greatest pleasure, submit the question, whether the above proposition is invalidated by the action of the laws of diffusion, to the decision of Mr. Graham, the great expounder of these laws, and abide by his verdict.¹

In the discussion of new views, more is required than mere quotations from old authorities. What is specially wanted are facts and experimental evidence. It must also be remembered that much depends upon the mode in which authorities are made use of in such discussions, since it is often an easy matter to select passages, or disjointed fragments, from the published works of authorities, which may appear to support almost any view which may be taken of a subject under consideration.

Dr. Hunt, whose paper consists, in greater part, of references to numerous authorities, from the time of Thomas à Kempis down to that of Sterry Hunt, seems to be quite aware of this fact, as an instance or two will testify.

Thus, when Dr. Hunt quotes Hopkins in support of his views as to the consolidation of the molten sphere, he takes care not to inform his readers that Hopkins distinctly declares his opinion that the exterior was not the last to solidify, but would have consolidated be-

¹ It must be remembered that these gases are supposed to be formed at an instant of general combination *in situ*, and not gradually gathered from the realms of space.

fore the interior had become entirely solid, a view which I have adopted on his authority, and which is diametrically opposed to Dr. Hunt's opinion that the surface of the earth immediately previous to its entire solidification was "a liquid bath of no great depth, surrounding the solid nucleus."

Again, although he finds it convenient to quote Forchammer in reference to some minor points quite beyond the limits of the present discussion, he seems to be quite unaware of the fact that the idea of the saline crust of chlorides, &c., which he ridicules my having adopted, was long before propounded by Forchammer, who made the calculation that the chloride of sodium in such a crust would have been fully sufficient to have clothed the entire sphere with a coating of salt some 10 feet in thickness.

And yet again when he refers to Sorby's experiments as proving many points in favour of his views, amongst others that quartz cannot be volcanic, *i.e.*, a product of igneous fusion in nature, his deductions are at once entirely put to rout by the few lines from Sorby himself, produced in my last communication to the *Chemical News*.

On the other hand, after a careful consideration of the various memoirs of Hopkins, Forchammer, and Sorby, along with a careful repetition of many of their experiments, I cannot discover any one single point inconsistent with the views I have advanced. I am also able to bring much evidence in their favour from the writings of Daubrée, Bunsen, Durocher, Phillips, and other men of eminence, whose opinions Dr. Hunt evidently considers of no importance.

To prove that it is better to stay at home in one's laboratory than to travel wide and far in order to study Nature's operations in the field (as recommended by Sir Charles Lyell and other eminent men), Dr. Hunt quotes Thomas à Kempis, to the effect that "those who make long pilgrimages rarely become saints." What we require, however, is geologists, not saints; and it is well known that a knowledge of the world acquired by travel is the best antidote to bigotry or one-sided opinions.

As I have previously explained, I was induced to enter into this controversy (which I am quite confident will do good to science, by ventilating some obscure points) by the special invitation, conveyed in writing, from Dr. Hunt "to have a friendly fight;" but I now find, if I may judge from the style of that gentleman's communications, both to the *GEOLOGICAL MAGAZINE* and *Chemical News*, that his idea of scientific discussion consists in an attempt to overwhelm his opponent with sneers and countless accusations of incompetency and ignorance,¹—ignorance of chemistry, of geology,

¹ Dr. Hunt does not merely content himself with mere accusations of ignorance, for when disputing my assertion that "reactions of the compounds of magnesia with carbonic acid in an artificially compressed atmosphere of that acid," had long been employed on a large scale, he uses the words "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 may merely add that, since the appearance of Dr. Hunt's communication in the *Chemical News* of January 17, I have received various communications from Chemists and others, connected, or acquainted, with this manufacture, not only offering to supply more facts in corroboration of the truth of my

of petrology, mineralogy, microscopy, literature of the subject, etc., etc.; whilst at the same time he has not omitted to herald in his own views as what might be termed the quintessence of the combined "results of modern investigations in physics, chemistry, mathematics, and astronomy."

Would it not have been more wise, as well as more becoming, to have left to our readers the task of forming their own judgment upon these points after having weighed the evidence brought before them on both sides, in the course of this discussion.

Having no pretensions, like Dr. Hunt, either to being a saint, or even to be versed in saintly lore, I cannot cite Thomas à Kempis, yet I can, nevertheless, follow his example, and even at the risk of appearing still more uncourteous, I really cannot resist the temptation to remind him of the old saying,—passed into a proverb among laymen—that "Curses, like chickens, come home to roost."

II.—ON THE IGNEOUS ROCKS OF CHARNWOOD FOREST AND ITS NEIGHBOURHOOD.

By the late Rev. BADEN POWELL, F.R.S., F.G.S., formerly Savilian Professor of Geometry in the University of Oxford.

[This paper, written in 1859, has been obligingly communicated to the Editor by WARINGTON W. SMYTH, Esq., F.R.S., President of the Geological Society of London.]

THE geology of Charnwood Forest appears to have been first systematically investigated by Professors Sedgwick, Whewell, and Airy in 1833. A very brief notice of their labours by C. Allsop, Esq., is appended to the history of Charnwood Forest by J. R. Potter, 1842, as is also a valuable and detailed memoir on the geology of the district, by J. B. Jukes, Esq.

More recently the labours of the Government Survey have furnished us with the geological colouring of the Ordnance Map, and with several sections; accompanied by a few notes by H. Howell, Esq.

Since these researches I am unable to learn that anything has been published on this interesting region, which is admitted by Mr. Jukes to present many problems for investigation. In the very elaborate classified index of Mr. C. W. Ormerod, F.G.S. (1858), not a single instance occurs of any paper illustrative of the geology of this district, having been published in the Quarterly Journal of the Geological Society of London.

Having enjoyed an opportunity of residing upwards of two months in this region, during the summer of 1859, I examined and collected specimens from nearly every locality of igneous action. The brief notices here given have no pretensions beyond that of being faithful records of a few facts which fell under my notice, which do not seem to have been previously attended to, but which appear to bear on the questions still open to discussion, as to some of the geological features of this remarkable district.

assertion, but also directing my attention to an expired patent, taken out many years ago (No. 9102, A.D. 1841) by the late Mr. Pattison, of Newcastle, in which these identical reactions are embodied.