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ORIGINAL ARTICLES:

I.—ON THE CONTRACTION OF IGNEOUS ROCKS IN COOLING.

By DAVID FORBES, F.R.S., &c.

UPON commencing, in 1847, the special study of the igneous rocks, I, no doubt in common with many others, took for granted the conclusions arrived at experimentally by Professor Bischof, of Bonn, as to the amount of contraction which such rocks would undergo when passing from the state of hot liquidity to that of cold solidity; and I did so, not after any critical examination of the details of Bischof's inquiry itself (the most important of which are contained in a memoir published in Leonhard und Bronn's *Jahrbuch* for 1841), but merely in deference to authority, since these conclusions had apparently received unquestioned acceptance from geologists in general.

Living at that time in Norway, a country pre-eminently fitted for the study of eruptive rocks on the large scale, I soon found, however, that these conclusions did not seem to be at all in accordance with the facts which every day presented themselves in the field. I could nowhere find evidence to prove that any such immense contraction as 25 per cent., or one-fourth of its volume in granite, or 10 per cent., one-tenth of its volume in basalt, as assumed by Bischof, had ever taken place in nature. Even when great masses of granite were seen interposed between rigid rocks of other character, they were not found to present large fissures or rents of contraction, or to possess a well-marked porous, honeycombed, or cavernous structure, such as might be expected in case any such enormous contraction in volume had really taken place.

On the contrary, although some small amount of contraction had undoubtedly taken place, these rocks, as a rule, appeared to be so solid and compact in texture throughout, as to indicate that the space which had been originally filled up by molten rock differed but comparatively little in volume from that which the rock now occupies.

These discrepancies led me to consult the original memoirs of Bischof, and to repeat his experiments on a more extended, and, in some cases, a much larger scale, using as materials the basaltic melaphyre of Staffordshire, commonly called the Rowley Ragstone,

various furnace slags, both acid and basic, and still more acid silicates like glass.

The results of this inquiry fully satisfied me that only comparative and but roughly approximative data, as to the actual amount of contraction undergone, could be obtained by such experiments as those of Bischof and myself, since, when operating upon so small a scale, it was quite impossible to avoid various sources of error, or to obtain the silicate, after cooling, so free from vacuities, or, in other words, so compact, as when working on an infinitely larger scale, as is the case in nature. For these reasons, therefore, I did not, as Bischof appears to have done, attempt to deceive myself by reducing the actual values obtained in these experiments to a numerical expression of the percentage amount of the contraction; but, instead of putting any great confidence in such figures, was content to point out that the results of all these experiments were unanimous in indicating that the actual amount of contraction experienced must be greatly below that which, upon Bischof's authority, seems to have been very generally accepted.

A summary of my experiments has been published in the *Chemical News* for October 23, 1868, and I now find, in a recent paper "On the Elevation of Mountains by Lateral Pressure, its Cause, and the Amount of it, with a Speculation on the Origin of Volcanic Action," by the Rev. O. Fisher, M.A., F.G.S., published in the Transactions of the Cambridge Philosophical Society, vol. xi., part iii., that they have been commented upon as follows:—

"Mr. David Forbes, in a paper in which he combats the idea of the solidity of the earth, and is concerned to reduce as much as possible the amount of contraction of its materials, states that, in the manufacture of artificial stone from the Rowley Rag, the specific gravity of the Rag was 2.84, while that of the fused stone, after having been cooled slowly and devitrified in red-hot sand moulds, was the same (2.84). But the specific gravity of the glass before devitrification was only 2.67. The volume being necessarily in inverse proportion to the density, these numbers give a contraction of 0.060 in passing from the glassy to the stony state.

"Mr. Forbes himself made some experiments upon highly siliceous slags, which he cast in iron moulds of 360 cubic inches, and found the slags to contract to from 350 to 355 cubic inches. These numbers give a contraction of from 0.014 to 0.028. In the latter case a further contraction would probably accompany devitrification, which, from the analogy of the Rowley Rag, would raise the contraction to from 0.074 to 0.088.

"The dimensions of the iron moulds above given seem to have been taken when they were cold. If this were so, the above numbers would be too small."

As I have already given the reasons which led me to inquire experimentally into the ratio of the contraction of igneous rocks, and since the question, whether such contraction is as large as Bischof estimates it, or as small as I found it, cannot have any influence, *pro* or *contra*, on the views or arguments which I have ad-

vanced as to the solidity of the earth, I am at a loss to conceive why I should be here represented as "concerned to reduce as much as possible the amount of contraction of its materials." However, on the *tu quoque* principle, I shall endeavour to show whether it may not in reality be the Rev. O. Fisher who is most "concerned to reduce as much as possible" the effect of the deductions (obvious from my experimental results) upon his own theoretical views: for, in the first place, he supposes that the iron moulds had been measured when cold, or, in other words, represents the experimentalist as being probably so blind as to overlook so obvious a source of error; and, secondly, he takes it for granted that the highly silicious slags which are referred to were, after casting, in a vitreous, and not in the devitrified or stony condition.

In reply, it may be stated, that the cast-iron moulds made use of were constructed in parts, so as to be adjusted in capacity with ease, either when cold or hot; that they were measured whilst in the hot or expanded state; and lastly, that curiously enough, as it may seem at first sight, the results obtained from casting in perfectly cold moulds, measured when cold, were as near as possible identical with those in the expanded hot moulds, measured when hot. This is easily explained from the fact that the slag, when poured into a perfectly cold mould, solidifies instantaneously on all sides in contact with the iron long before the thick cast-iron mould has even had time to expand, the slag thus forming a cube or rectangle of the exact size of the original cold mould. Next, the highly silicious slags which Mr. Fisher supposes would have contracted still further, when they came into the devitrified condition, could not do so; simply because they were already in that condition, as is very commonly the case with slags which contain a considerable percentage of lime, since, on cooling, such slags (as may easily be seen at the Staffordshire and other blast furnaces) pass, in most instances, at once from the molten to the stony or devitrified condition, and show but little of the glassy surface or interior so characteristic of other slags containing less lime. This fact is fully appreciated by the glass manufacturers, who take especial care not to introduce too much lime into their mixtures.

The introductory pages of Mr. Fisher's paper, in the state in which it now appears in the Transactions of the Cambridge Philosophical Society, have evidently been considerably altered from what they were when read at the Society (April 27th, 1868), since reference is made both to Mr. Delaunay's researches—a translation of which I communicated to the GEOLOGICAL MAGAZINE of November, 1868—as well as to my paper in the *Chemical News* of October 23rd, of that year. For this reason also I might have expected, when it eventually did appear in print, that I should not have been obliged to make the above remarks, particularly as Mr. Fisher, under the date of January 22, wrote me as follows:—"Will you allow me to ask you, with reference to your article in the *Chemical News* of October 23rd, whether, in estimating the contraction of the slags cast in iron moulds, the expansion of the iron was allowed for. It

seems to me that the amount of contraction would be greater than the mere difference between the dimensions of the moulds when cold and that of the cooled blocks? I ask this question because I am engaged in revising a paper of mine relating to the cooling of the earth, in which I am inclined to support the view of the solidity of the globe, though I feel strongly the inadequacy of our means of arriving at any certainty on the subject."

In reply to this I wrote Mr. Fisher to the effect that allowance had been made for the expansion of the moulds, amongst other means, by measuring the moulds whilst hot; but it appears now, from his published paper, that he has not modified the expressions made use of in his comments upon my experiments.

Mr. Fisher's paper is a purely mathematical one, and altogether beyond the scope of my present communication; yet what I have already stated is but another proof of the tendency which mathematicians apparently have to treat experimental data in their own way, overlooking the vital importance of thoroughly sifting their evidence, or premises on which they base their elaborate reasonings. The recent notorious case of the Pascal forgeries is but an instance in point; and a representative of the Press pleasantly remarked that nobody but a mathematician could have been deceived by such imperfect evidence.

II.—ON SOME SUPPOSED LITHODAMOUS PERFORATIONS IN LIMESTONE ROCKS.

By JOHN ROSE, F.G.S.

[PLATE I.]

WITHIN the last few months considerable interest has been revived on the question of the origin of certain small excavations or borings which have been found in limestone rocks in different localities in Sicily, France, England, and Wales. The first public notice of them in England was by Dr. Buckland, whose attention, when at a meeting of Geologists at Boulogne in 1839, was called to some borings which had been found in the under-surface of a ledge of Carboniferous Limestone rock in the Boulonnais. After this Dr. Buckland examined some similar borings which the Rev. N. Stapleton had observed near Tenby, and described both cases in a communication to the Geological Society in 1841. About the same time that Dr. Buckland was engaged with these excavations, M. Constant Prévost found some similar perforations on Monte Pellegrino, near Palermo. Mr. C. P. Jopling discovered them in Furness in 1843. M. Bouchard Chantreaux, who met with them in the Bois des Roches, in the Bas Boulonnais, has published a memoir on the subject, the result of many years observation, to which I shall have occasion again to refer. Mr. Pengelly met with them in many localities in the Torbay district. They were also found on Birkrigg Common by Miss E. Hodgson, of Ulverston, and were described by her in vol. vii. of *The Geologist*, p. 42. Mr. Darbyshire has discovered them in North