III.—CONTRACTION OF ROCKS IN COOLING. REPLY TO D. FORBES, ESQ., F.R.S.

By REV. O. FISHER, M.A., F.G.S.

IN replying to Mr. Forbes' strictures upon my paper at Cambridge upon "The elevation of Mountains, etc.," I may be permitted to say, what would otherwise be of no consequence. The paper was read in April 1868. I received it back from the referees, accompanied by some valuable suggestions, in September. I was at that time much occupied, and did not work out those suggestions until January 1869. After having done so I wrote to Mr. Forbes, received his reply on the 29th, and a week later sent the paper back to the referees.

Although, therefore, as Mr. Forbes infers, the paper was altered subsequently to its being read, yet those alterations were not made without the sanction of the officials of the Society.

It seems to me that the more rocks contract in cooling, the more probable it is that the earth is solid. And I conceive that this must be a question of degree, unless we consider melted rock to be a perfect fluid.

I understood the same view to pervade Mr. Forbes's paper "On some points in Chemical Geology." Hence in quoting from his paper, in which he inclines to support the opinion of the earth's fluidity, I felt that I was not using data furnished by an advocate of the side of the question favourable to my theory; for that was based on the contraction of the earth in cooling. It was this, and nothing more, which prompted the expression, that Mr. Forbes was "concerned to reduce, as much as possible, the contraction of the materials of the earth." If the words *be* capable of a sinister meaning, I should have been ashamed to have used them in it.

The reply which I received from Mr. Forbes to my inquiry, whether he had allowed for the expansion of the iron moulds, was as follows:—"The expansion of the moulds was allowed for in many cases by measuring the *hot* moulds, which was easily done, since the moulds were in all cases cubes or rectangular. Other experiments were made both in stone and sand moulds. The results obtained by me are not put forward as representing the *true* amount of contraction, but as showing," in short, that Bischoff's results were considerably too large.

I did not alter what I had written because the qualifying words, "in many cases," led me to think (wrongly it appears), though hot moulds were in many cases used, yet that the dimensions given in the *Chemical News* were probably those of the moulds when cold.¹ This idea was confirmed by the disclaimer of exactness for the results. I am

¹ "The slags were allowed to flow into iron moulds, ten inches long by six deep and wide, also containing 360 cubic inches, a heavy iron top-plate being dropped upon them when overflowing, so as to ensure their being filled completely and evenly."—*Chemical News*, vol. xviii., p. 193.

now sorry that I said anything about the condition of the moulds ; for the sentence might have been just as well entirely left out. It was quite immaterial to my argument.

As Mr. Forbes did not state in his paper in the *Chemical News* that the blocks of slag were devitrified as cast, it was surely pardonable in me to suppose the contrary, having been told of the slow process of cooling needful to devitrify the Rowley rag. And I do not even now feel convinced that a slag cooled thus quickly enters into the normal stony condition of an igneous rock.

Mr. Forbes does not find fault with the number 0.060, as expressing the final contraction of the Rowley rag. If that number be used in the place of 0.09, which I have employed, the only part of my paper that will be affected by it will be the rough estimate of the height of mountain chains, caused by a contraction supposed equal to the latter number. These will have to be lowered, in the ratio of 0.64 to 1, or nearly in that of two to three ; and thus the results will be made to accord with nature better. But I must repeat the caution, that no attempt is made here to arrive at a definite result, because we have no right to assume that an upper shell of the earth, of the thickness I have supposed, has contracted to the same amount as the Rowley rag did, in passing from a fused to a devitrified state. Its contraction may have been less or it may have been greater. Yet I think we may assume that it has been of the same *order* of magnitude, which is all that I intend to assert. All that my general argument requires is the admission that heated rocks contract on cooling. It is not affected by the question whether the earth be *now* solid, or fluid, or semifluid, internally. If it be fluid, that condition will more easily explain volcanic action, though it seems possible the solid condition (to which I incline) may be compatible with my speculation as to its cause.

The somewhat lengthy investigation as to the condition of pressure in the interior, on the supposition that the earth is solid, is intended not to leave that case unprovided for.

Finally, a word for the mathematicians, of whom I make no pretension to be one. So long as certain questions cannot be touched without their aid, their vocation will be a needful and an honourable one. The present feud seems a re-enactment of the fable of the belly and the members. Mathematics has been likened to a mill. I will extend the simile, and call the mathematician, not the miller, but the millwright. The experimentalist is the grower of the corn. If the corn is good so will be the flour. But though bad corn be once put in, and bad flour come out, the mill is as good as ever ; and so, if a false numerical result be introduced into a problem, the mere substitution of a correct one will bring a correct result. Few men can be accomplished on all sides, and the shortcomings of the mathematician, as an experimentalist, will best be met by kindly suggestions from his more accomplished fellow workers in other portions of the vast fields of science.