I do not lay claim to any great originality for my little sketch, but, in fact, I did not derive my ideas from Dr. Irving's paper.

3, Pump Court, Temple, E.C. Horace W. Monckton.

REPLY TO MR. A. SOMERVAIL.

Sir,—I owe an apology to Mr. Somervail for plucking a leaf from his coronet of laurels. It is the simple truth that the paper which he cites had not in any way impressed itself on my mind, and thus (as the index for the last volume was not then published) escaped recollection. While making this atonement, I will take the opportunity of explaining to him why I use that plainness of speech to which he evidently objects. If he is right in his principal hypothesis about the rocks of the Lizard, I am so hopelessly wrong that I must begin my petrological studies *de novo*. The one or the other of us, so to say, is ignorant of the very grammar of the language. Now, as it happens, I have given, for nearly twenty years, more attention to petrology than to any other branch of geology; twice or thrice every year I have visited districts which were known to be instructive, making often long journeys in order to study some critical question. I have examined many of the most interesting localities on the Continent of Europe, a few also in Canada. I have formed a very large collection of rock specimens and microscopic slides, to the study of which I have devoted such leisure as I can command. Now in Mr. Somervail's writings no evidence appears of either wide experience or knowledge of the microscope, both of which are necessary for theorizing on difficult problems in petrology; indeed, of the latter, not so long since, he admitted his ignorance. Of course I know that many of these problems are yet unsolved; I make no claim to infallibility; I am well aware that notwithstanding all my pains I have not escaped the fate of workers in a progressive science, and have to modify or even abandon conclusions which at one time seemed most accordant with facts, but some of Mr. Somervail's hypotheses appear to me irreconcilable with facts and inductions which, not only I, but also petrologists of greater repute, accept almost as axioms. To me he appears to occupy the position in which I should have placed myself had I signalized my entrance in the "fifth form" at school by publishing "adversaria" on a trilogy of Æschylus.

T. G. Bonney.

DYNAMO-METAMORPHISM.

Sir,—M. Spring's valuable experiments have had a very stimulating effect on many minds; so much so that his experiments are sometimes quoted in proof of positions very much in advance of those taken by M. Spring himself. Thus Mr. Harker in his letter on the subject of dynamo-metamorphism in your last issue, after remarking that "the practical verification" of "the *direct* correlation of mechanical and chemical energy" "rests on such experiments as those of Cailletet, Pfaff and Spring," refers to one of the experiments of the latter in the course of which sulphur and copper filings subjected to a pressure of 5000 atmospheres were converted into crystallized copper sulphide. Mr. Harker comments on this as follows:—"So much of the mechanically-developed energy as takes the form of heat is carefully removed; but chemical combination still takes place. It follows that the energy absorbed in this combination comes directly from the mechanical work done, without the intervention of heat."

I have no desire to interpose in the controversy between Mr. Harker and Dr. Irving, but, without doing this, I may point out that the explanation offered by Mr. Harker does not appear to be in accord with that offered by M. Spring himself.

In the American Journ. Science, xxxvi. (1888) pp. 286–289, Spring remarks regarding his "researches on the compression of powders," "To my mind pressure was not an active agent in the matter; but only the means to the end, and I looked for the effects to contact alone. . . . In another place [Bull. Soc. Chemique, 1884], I said with regard to chemical action produced by my experiments—'one must not lose sight of the fact that pressure is not a chemical agent to the same extent as heat or electricity.' But as I have always thought that *contact* was brought about by compression, I have often, for the sake of brevity, spoken of '*welding* due to pressure,' instead of always saying welding due to contact produced by compression. I now see that I was unwise in thus wishing to economise my time. Besides, as conclusive proof that it is always to contact that I assigned welding phenomena, chemical reactions, and also in part the diffusion of solids, there is the fact that I deemed it necessary to operate *in vacuo*, on account of the failures in preliminary experiments made under the ordinary conditions;" and M. Spring goes on to explain that when he did not operate *in vacuo* the presence of air between the particles hindered intimate *contact* between them and thus prevented chemical action, pressure notwithstanding.

In a previous communication to the American Journ. Science (vol. xxxv. 1888, p. 78) Mr. Spring wrote: "Since then [1880] new experiments, still in part unpublished, have made me recognize the importance of the part that a certain degree of temperature plays in these phenomena, so that for the solid state, as well as for the gaseous one, a *critical temperature* would be remarked, above or below which the changes of simple pressure would be no longer possible."

Spring, therefore, in the conclusions arrived at as recently as the close of 1888, attributes the chemical action set up in his experiments to *contact* plus a certain degree of *temperature*. Pressure is merely the matrimonial agent, so to speak, that brings the highly susceptible particles together; but it is to *contact* plus *heat* that, according to Spring, the chemical action is due.

C. A. McMahon.

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