being that "On the Schists of the Lizard District," April, 1890, perhaps the one he likes least.

As to the points in his letter under his figures 1, 2, 3 and 4, I have no doubt but that Prof. Bonney will in good time demonstrate these assertions; but in the meanwhile they are only assertions. I will freely and gladly admit the errors, both in my observations and inductions, when proofs are forthcoming. I was much amused by General McMahon's letter. I am well aware (perhaps before the General was) of the apparent sequence of the various rocks laid down by the masterly mind of De la Beche, and also (perhaps) I have seen more of the true dykes in the Lizard District than has fallen under the observations of General McMahon. There are dykes, however, that I regard as of contemporaneous or segregation origin.

Independent of the sequence of the rocks referred to, I think them the product of eruptions of one geological period, that intermittent action is noticeable, and that there is a decided passage of the main masses into each other, and that the same magma, cooling under different conditions, has given rise to many varieties of rock. My communications were intended to lead up to this point.

As to my theory of the origin of the "banded structure," let it with the others "sink or swim." I care not which survives.

As to the close of General McMahon's letter, I much regret having to say, that I think it is quite uncalled for.

Torquay, 9th *December*, 1890. Alexr. Somervail.

REPORT OF THE INTERNATIONAL GEOLOGICAL CONGRESS.

Sir,—I am periodically asked by friends who joined the last Geological Congress how it is that the promised report to which each member was said to be entitled has not yet appeared, although some of us paid an additional subscription to expedite its production.

Ought not the eminent geologists whose names appeared on the circular inviting support to that Meeting to be asked to furnish some explanation for this unaccountable delay? (B. V)^2.

ON DYNAMO-METAMORPHISM.

Sir,—I certainly had no thought of "rolling back the development of chemical theory a few decades at least," when I wrote of energy taking "the molecular forms of heat and chemical action." Dr. Irving in his criticism of this expression leaves out my reference to heat. I conclude therefore that he has no objection to that part of the statement. As to the assertion that part of the energy, which previously existed in the molar form, was converted into the "molecular form of chemical action," I was unable to know whether Dr. Irving's stricture expressed the generally received views upon the subject, owing to my imperfect acquaintance with chemistry. I have, therefore, consulted the highest authority on such questions to whom I could apply and on whose opinion I can place reliance. With respect to Dr. Irving's apparently general statement, that "chemical combination must generate heat," he replies, that, "when carbon is heated in carbonic acid gas, C O is formed with a disappearance of heat; and, when nitrogen and oxygen are sufficiently heated together, an oxide of nitrogen is formed with a disappearance of heat; and, that in these cases the heat which has disappeared has become chemical energy in the molecules of C O or N O. Whether it be *atomic* energy or not is not at present known, but as the molecule includes the atoms, it is certainly "molecular" as distinguished from ordinary mechanical, or molar energy. Since many chemical changes, which only take place at very high temperatures, appear to be attended with a disappearance of heat, it is at least not improbable that some of the changes, by which minerals are formed in the interior of the earth, may also be attended with a storage of energy."

"Perhaps Dr. Irving takes exception to the supposition that mechanical energy may be directly transformed into chemical energy. If so, you may reply that the known effects of pressure upon chemical changes, when those changes are attended by a change of volume, afford support to the supposition. Recent observations on the influence of surface tension on chemical change by Liebreich, J. J. Thomson, and others, lead in the same direction, so that it cannot be said that the supposition is unreasonable, even in the light of recent advances in physical chemistry."

Finally I am told that the assertion that "chemical combination must generate heat" is certainly incorrect, and that the examples CO and NO to the contrary are "only two out of an immense number."

HARLTON, CAMBRIDGE, 13 *Dec.* O. FISHER.

DYNAMOMETAMORPHISM.

SIR,—I must apologize to Dr. Irving for having overlooked the observations to which he refers. Unfortunately I had not read the work in question at the time when I wrote my letter.

As regards the main subject of his letter in your December number, I would offer only a few words. In assuming that the whole of the work done in the compression, deformation, and friction of rock-masses passes into heat, Dr. Irving misses the idea which underlay the whole of my remarks, and was more explicitly stated in Mr. Fisher's article. The *direct* correlation of mechanical and chemical energy was, I believe, first mooted by Dr. Sorby in 1863; but the practical verification of it rests on such experiments as those of Cailletet, Pfaff, and Spring. To take an example: Spring subjects a mixture of sulphur and copper filings to a pressure of 5000 atmospheres, and finds it converted into crystallised copper sulphide. The operation is conducted slowly, and the temperature of the apparatus kept constant. In other words, 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.

ST. JOHN'S COLLEGE, CAMBRIDGE. ALFRED HARKER.

https://doi.org/10.1017/S0016756800176824 Published online by Cambridge University Press