But in any case, even though these specimens show that *all* the Bude rocks are not without distinct evidence of metamorphic action, it is still true that the effect produced is not in anything like the proportion we might expect, from the stresses endured by these beds.

I am not able to follow some of the reflections which Major-General McMahon bases on the supposed total absence of alteration at Bude.

Hallock's experiments (as quoted), and still more Hallock's conclusion from them, seem to be beside the mark. It is not generally supposed that pressure is able to liquefy rocks,—quite the reverse in fact,—and there does not seem to be any justification for saying that "consequently" no chemical or mineralogical changes are to be expected.

Again, Spring's experiments are admitted to have proved that pressure can produce chemical combinations and re-arrangements; and nothing that was done by "Professor Spring's pestle and mortar" would be lacking in the intermixture of minute particles of minerals in the fine silt of which these Bude rocks and similar strata are largely composed. There is no call here for rocks to be "crushed and ground to pieces by irresistible geological disturbances." All the crushing and grinding has been done in the gentlest and quietest way, and the resulting material has but to lie and await the pressure.

Whether pressure, with or without movement, is in itself sufficient to intensely metamorphose sedimentary rocks, is another question.

And, if it *is* sufficient, there is still much room for inquiry and speculation as to why it acts so comparatively feebly at one place and so very intensely a few miles away, when, so far as can be judged from the rocks, the feebler metamorphism has by no means corresponded to feebler stresses.

NEWCASTLE-ON-TYNE,
March 10th, 1890.

W. MAYNARD HUTCHINGS.

CONTORTION AND METAMORPHISM.

SIR,—General McMahon's "Notes on the Culm-measures at Bude" in the March Number of this MAGAZINE (p. 106) form a welcome contribution to the petrology of the district, and have a particular interest as indicating the probable derivation of the strata in question from the destruction of granitic rocks. The fact that the Culm-measures are much contorted without having experienced any appreciable mineralogical changes seems, however, to have only a limited bearing on the general question of metamorphism by pressure.

Adopting the familiar treatment employed by Thomson and Tait, we may usefully resolve any system of strains into (i) a uniform voluminal compression and (ii) certain shears. The term shear is here used in its strict sense, viz. deformation apart from change of volume, and it is evident that the varying amounts of shearing from point to point within the mass express themselves completely in the contortion of the rocks affected, faulting being regarded for this

purpose as a particular case of contortion. In like manner the correlated stresses resolve into (i) a uniform pressure and (ii) certain shearing stresses. The energy set free consists of two parts; (i) that due to compression, measured by the product of the uniform pressure into the relative compression, and (ii) that due to shearing, measured by the products of the shearing stresses into the amounts of the corresponding shears. The total energy thus set free, except in so far as it is lost by conduction of heat, must be absorbed in the production of mineralogical changes. Rocks are known to be very bad conductors of heat, but the amount of energy lost in this way must vary with circumstances, time being an important factor.

Again, viewing the strains and stresses in a rock-mass with reference to the external forces that produce them, it is essential to notice that the voluminal compression and uniform pressure depend upon the *sum* of the forces acting in different directions (*e.g.* vertically and horizontally) while the shears and shearing stresses depend upon the *differences* of those forces. We may, for example, picture a mass of rocks subjected to a lateral thrust and to the weight of overlying rocks. If the mass be situated at no great depth, the latter force may be very much less than the former, and considerable shearing may be produced if the material be not a very rigid one, or if the thrust be of long duration; for shearing is, within limits, proportional to the time. The pressure and the total energy set free may or may not be very great, and under a comparatively small cover of rocks much of the energy must be lost by conduction. It is thus easy to imagine conditions under which any amount of contortion may be produced without any metamorphism of the rocks so affected.

If the same lateral thrust operate upon a rock-mass at a greater depth beneath the surface, it will be more nearly balanced by the weight of the cover, and so the compression and pressure will be greater, but the shears and shearing stresses less. The total energy set free will be greater, and there will be less loss by conduction. We may thus have metamorphism produced with or without contortion.

In the case of rocks at a depth, too, the time-element must be important. The rigidity of the mass being there materially diminished —this, at least, is generally admitted—there must be a tendency to propagate pressure uniformly, as in a liquid. If this property hold good to any extent, shearing stresses cannot be set up unless the disturbing forces increase comparatively suddenly. However this may be, it appears that the contortion of rocks cannot afford an accurate measure of the forces which have produced it, and that contortion and dynamo-metamorphism, though due to the same ultimate cause, are by no means necessarily associated in the same place. One or the other phenomenon may occur alone, or both together, in accordance with complex conditions, such as the depth of the cover, the rigidity of the rocks affected, and the slowness or rapidity of development of the disturbing forces.

General McMahon apparently calls in question the experimental

researches of M. Spring and others on the physical and chemical changes produced by the action of high pressures. It seems rather rather late in the day to take this position, but the subject is too wide to be discussed here. The Belgian physicist, too, is well able to defend himself: witness his reply to the American critic cited by General McMahon.

ALFRED HARKER.

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COCCOSTEUS DECIPIENS.

SIR,—In a very important paper on the structure of *Coccosteus decipiens*, Ag., Dr. Traquair has recently remarked (Ann. & Mag. Nat. Hist. [6] vol. v. p. 125) that he suspects I have mistaken the lateral margin of the interlateral plate for a pectoral spine in my description of *Coccosteus*, and he feels justified in asserting that, if such a pectoral swimming organ does really exist in *C. Bickensis*, that species cannot be referred to *Coccosteus*, in which no such appendage is present.

In reply, I must repeat that there occurs a hollow, triangular, bony spine, filled with calc spar, quite distinct from the other plates. Apart from this spine, *C. Bickensis* agrees so well with undoubted species of *Coccosteus*, that I am inclined to regard Dr. Traquair's statement cited above as not yet beyond question; and although a similar pectoral organ has not yet been recognized in Scottish specimens, it is quite likely it may still be found. I am all the more confirmed in this opinion since, according to Dr. Traquair, the sclerotic ring appears to exist only in *one* specimen from Gamrie in the Edinburgh Museum, while it is rather common in my German specimens. The pectoral spine is much more rarely seen in my fossils than the sclerotic ring, and I am thus not astonished that it should hitherto have escaped observation in the Scottish examples of *Coccosteus*. Finally, I would add that the spine in *C. Bickensis* attained a length of 55mm. (fig. 12 of my paper on Placoderms), but the end is wanting, the impression of it being retained on the rock. It is therefore not shorter, but much longer than in the restoration of *Brachydeirus inflatus*.

I may add that my specimens are exposed in the Royal Geological Museum here at Göttingen, and may be examined by any one interested in the subject.

A. VON KOENEN.

GÖTTINGEN, *March 12th*, 1890.

TIDAL ACTION.

SIR,—As tidal action has been called in of late in your pages to assist if possible in solving the riddle of the Triassic sandstones and conglomerates, it may be well to point out one line of evidence which seems to have been overlooked by the supporters of the tidal theory, *i.e. the zoological.*

Mr. Mellard Reade writes as follows in the Philosophical Magazine, vol. xxv. p. 342:—"Although it is on the littoral margins and the shallow seas opening into the oceans that the resistless force of the tides is most obvious," etc., etc.[1]

[1] See Mr. Mellard-Reade's Article in this Number, *supra*, p. 157.—ED. GEOL. MAG.