to render the stratified appearance of the rock very marked at comparatively great distances from them.

There is in many cases a crack marking the junction of contiguous layers.

As an illustration of these "composite" dykes, I append a diagrammatic sketch representing a section of the coast about 200 or 300 yards south of the Cligga promontory, which is very difficult of approach.

A has all the appearance of a bed of sandstone, the strata curved, owing to the intrusion of the dyke *B* (granitic); *C* is an

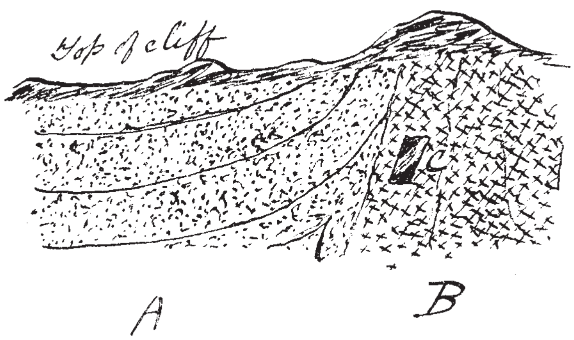

FIG. 3.

old tin burrow. As a matter of fact, each is a granitic dyke, *A* finer grained than *B*, and very like sandstone in all petrological features.

The remarkable fact is the apparent stratification of the beds *A*, which are really bands of several dykes—a continuation of those figured at p. 164 in De La Beche's book. He does not seem to have observed this instance, or at any rate does not mention it; his figure is from the cliff immediately in contact with the Cligga promontory, and north of that I have figured.

Further instances of this very interesting kind of composite dyke would hel in many cases to unravel the seeming complexity of such geological features as those I have touched upon in Cornwall. HENRY E. EDE.

45 Walker Terrace, Gateshead-on-Tyne, October 4.

Weismannism.

I NEVER answer reviews, save in so far as they may be misleading on matters of fact. As this is the case with "P. C. M.'s" notice of my "Examination of Weismannism" (NATURE, November 16), I should like to say a few words touching the more important of such matters.

It seems that in seeking to do justice to all sides in the heredity question, I have been too careless in expressing my own view. At all events, any one reading the review must gather from it that I am a Lamarckian engaged in fighting the theories of Prof. Weismann. In the book, however, it is stated that I have been an adherent of the theory of Stirp ever since it was published by Mr. Galton in 1875. It is also stated that this theory is, in my opinion, identical, as regards all main principles, with that of Germ-plasm in the present phase of its numerous metamorphoses. Therefore, far from fighting the Weismannian theory of heredity, I see in all its main features, as it now stands, a "re-publication" of the one which I have held for close upon twenty years.

It is further stated that the only points of much secondary importance wherein I can perceive the two theories to differ are, (*a*), that while Galton confined himself to publishing a theory of Heredity, Weismann proceeded to rear upon this basis (*i.e.*, the hypothesis of "continuity") a further and elaborate theory of organic evolution; and, (*b*), that Weismann has not gone so far as Galton did in expressly recognising the possibility of an occasional transmission of acquired characters, in faint though presumably accumulative degrees. As regards these two points of difference, I have endeavoured to show, (*a*), that Weismann has now himself withdrawn nearly all his previous generalisations with regard to organic evolution, while largely modifying his theory of heredity; and, (*b*), that he has only to expand certain hints which he has already given—and which, if expanded, would entail much less modification of his original system than those which he has now made in other parts thereof—in order as fully to recognise as Galton did the possibly occasional transmission of acquired characters.

Hence, such opposition as I have found any reason to express with regard to Weismann's system in the latest phase of its development arises, almost exclusively, against the inordinately speculative character of his method. The history of science furnishes no approach to such a disproportion between deduction and induction.

Thus it seems to me that any writer on Weismannism who aims at impartiality must fail in his aim, if he does not give due prominence to this the most distinctive feature of Weismann's method. And, unless the reviewer is prepared to defend such a method as scientific, he has no reason to quarrel with what he calls my "hard words," since they all have reference to it, and are statements, not of opinions, but of facts.

On the other hand, I have endeavoured by "soft words" to fully recognise the great merit of Weismann's work in constituting the heredity question one of world-wide interest. And any bias that I may have with regard to this question is assuredly on the side of "continuity," although I cannot hold that the subordinate question is closed—*i.e.*, as to whether such continuity can never, under any circumstances or in any degrees, be interrupted. GEORGE J. ROMANES.

Hyères, November 20.

Correlation of Solar and Magnetic Phenomena.

MR. ELLIS, in his letter (NATURE, November 9), has discussed the coincidence between Carrington's observation of a solar outburst in 1859 and the magnetic movements observed at Kew and Greenwich. He comes to the conclusion that the disturbance of the magnets corresponding to this outburst was small, and that, although many greater magnetic movements have occurred since, no corresponding manifestation has been seen, although the sun has been so closely watched.

He appears to have overlooked an observation made at Sherman, by Prof. Young, which shows a very striking *series* of coincidences, and which is described in his work, "The Sun" (p. 156), in the following words:—"On August 3, 1872, the chromosphere in the neighbourhood of a sun-spot, which was just coming into view around the edge of the sun, was greatly disturbed on several occasions during the forenoon. Jets of luminous matter of intense brilliance were projected, and the dark lines of the spectrum were reversed by hundreds for a few minutes at a time. There were three especially notable paroxysms at 8.45, 10.30, and 11.50 a.m., local time. At dinner the photographer of the party, who was making our magnetic observations, told me, before knowing anything about what I had been observing, that he had been obliged to give up work, his magnet having swung clear off the scale. Two days later the spot had come round the edge of the limb. On the morning of August 5, I began observations at 6.40, and for about an hour witnessed some of the most remarkable phenomena I have ever seen. The hydrogen lines, with many others, were brilliantly reversed in the spectrum of the nucleus, and at one point in the penumbra the C line sent out what looked like a blowpipe jet, projecting toward the upper end of the spectrum, and indicating a motion along the line of sight of about 120 miles per second. The motion would die out and be renewed again at intervals of a minute or two. . . . . The disturbance ceased before eight o'clock, and was not renewed that forenoon. On writing to England, I received from Greenwich and Stonyhurst, through the kindness of Sir G. B. Airy and Rev. S. J. Perry, copies of the photographic magnetic records for those two days. . . . . On August 3, which was a day of general magnetic disturbance, the paroxysms I noticed at Sherman were accompanied by peculiar twitches of the magnet in England. Again, August 5 was a quiet day, magnetically speaking, but just during that hour, when the sun-spot was active, the magnet shivered and trembled. So far as appears, too, the magnetic action of the sun was instantaneous. After making allowance for longitude, the magnetic disturbance in England was strictly simultaneous, so far as can be judged, with the spectroscopic disturbance seen on the Rocky Mountains."

These observations of Prof. Young's seem to invalidate Mr. Ellis's statement that "no second occurrence similar to that of 1859 has come to light," and that although there undoubtedly exists a relation between sun-spots and magnetism, "it has not yet been found possible to trace direct correspondence in details."

Cambridge, November 12. A. R. HINKS.