interjected a doubt, but next day I carefully examined a large number of specimens here with a member of my staff, and we totally failed to confirm Mr. Bateson's statement.[1]

W. T. THISELTON-DYER.

Royal Gardens, Kew, April 29.

The Unit of Heat.

MR. GRIFFITHS, in a recent communication to the Royal Society, has called attention to the indefiniteness attending our present knowledge of the heat unit. In this connection I would wish to suggest—what indeed has long been present in my mind—that a unit of heat other than the present calorie is desirable. The present thermal unit is highly arbitrary, as well as most difficult of verification. This is true, whether we take the temperature at which the calorie is to be measured as 4° C. or 15° C. or as the temperature of minimum specific heat of water. The calorie owes its perpetuation to the method of mixtures—a laborious and inaccurate method of calorimetry—and dates from a period when the variations in the specific heat of water were not held of account.

If we do adhere to a specific heat of water calorie, it will be necessary to proceed as in the determination of the standard metre; obtain the more or less inaccurate measure of the primary unit in terms of some more accessible quantity.

My suggestion is that we start with an accessible unit. I think the latent heat of steam at the standard pressure has first claim. One gramme of saturated steam at 760 m.m. might be assumed to give up the unit quantity of heat in becoming water, without change of temperature. This unit might be called a therm, in order to avoid confusion with the existing unit. The specific heat of water would then stand as about 1·8 milli-therms. The larger value of the new unit commends itself as being more applicable to the problems of applied science; which, indeed, may be inferred from the fact that engineers often understand by the term calorie the kilogramme-degree.

I am aware that the change proposed is a radical one; but an appreciable change is better than a vexatious correction, and we know now that revision and change are inevitable.

In the definition of the proposed unit we replace the unreliable thermometer by one of the most trustworthy of instruments—the barometer; and our quantities of heat may be determined by the chemical balance, and, at 760 m.m., read directly upon the weights. We are sure of the purity of the material.

Trinity College, Dublin. J. JOLY.

The Study of Earthquakes in the South-East of Europe.

IN two recent notes in NATURE (vol. li. pp. 180, 468) attention has been drawn to the foundation by the Ottoman Government of a geodynamic section of the Imperial Meteorological Observatory at Constantinople. The new department has been placed under the direction of Dr. G. Agamennone, who for several years held a similar office at Rome, and who is well known to seismologists for the valuable work performed by him in Italy.

Not content with the foundation of a seismological observatory, Dr. Agamennone has also undertaken the organisation of earthquake studies throughout the Ottoman Empire, and he is anxious to extend this very important branch of his work so as to include the entire district within and bordering the eastern end of the Mediterranean. As there must be many readers of NATURE who are able, either directly or indirectly, to aid him in this attempt, I should be grateful if you would allow me to recommend it to their attention and support. Dr. Agamennone's address is "Observatoire Impériale Météorologique, Constantinople (Pera)."

That one of the finest seismic regions of the globe should at last attract the organised study it deserves, and that the initiation of the requisite observations should have fallen into hands so experienced and capable, will be matters of gratification to those who are interested in the progress of seismology. No less desirable would it be that all the results of such observations should be contained in the pages of a single journal, and Dr. Agamennone's publication of a monthly seismic bulletin, of which the first two numbers have already been issued, is an additional reason for the concentration of records from the different countries concerned in the Turkish Office.

Birmingham, April 19. CHARLES DAVISON.

[1] This and the absence of variation from the feral form in the foliage of the cultivated Cineraria, are covered by the principles laid down by Darwin in "Animals and Plants under Domestication," vol. ii, pp. 217-220.

Uniformitarianism in Geology.

IN reference to Prof. Judd's excellent statement of the position of the uniformitarian, allow me to call attention to an argument which tends to show that, so far as earthquakes and volcanic eruptions are concerned, catastrophes may be of greater magnitude now than in earlier geologic times.

The violence of an explosion will depend largely on the amount of confinement and pressure to which the exploding compounds are subject, as well shown in the case of Kilauea—where there is a constantly open vent and no violent eruptions—as contrasted with the numerous catastrophic explosions of long dormant volcanoes whose vents had become sealed up with cores of solid lava. But it is admitted that the crust of the earth has been growing thicker during all geological time. It is therefore almost certain that, in the remoter epochs volcanic phenomena were more frequent but less violent than they have become now that the crust is thicker, and, in its lower portions, at all events, denser and more consolidated. The usual argument, that, because the interior of the earth was somewhat hotter in early times therefore volcanic phenomena were more violent, appears to me to be entirely fallacious. The liquid matter immediately below the crust would have been at the same temperature then as it is now; and if there were a more abundant supply of aqueous vapour and other gases, the thinner and more permeable crust would have allowed of their constant and comparatively easy escape.

I do not remember to have seen this consideration referred to in any discussion of the question, and I therefore submit the argument to the judgment of physical geologists.

ALFRED R. WALLACE.

Research in Education.

PROF. ARMSTRONG'S trenchant indictment of the present methods of teaching science, is a little too much akin to Carlyle's fulminations against things in general—destructive but not constructive. Probably all good teachers are agreed upon the pernicious futility of the text-book and lecture-room cram system, and are in thorough accord as to the educational value of practical work; and are waiting only to learn or discover the best system of employing it. To this end destructive criticism helps but little. What is wanted is some definite scheme of work constructed by masters of practical instruction. Prof. Armstrong does certainly advocate what may be termed the "research method"; but it does not elucidate the question much, for it is difficult to understand how far he would extend this method. Would he, for instance, never mention Dalton's laws to students until, by a series of analyses, they were in a position to discover them for themselves? Or in the case of specific heat, how much information should be given before the beginners are set to investigate the phenomena alone? There are two ways of learning practically physical and chemical truths, either by repeating methods which have been explained and demonstrated, and then verifying each step by actual contact with real objects, and so acquiring real knowledge of fact and the application of theory, or by struggling to the truth by a process of trial and error. That the latter process, when successful, is the more stimulating to the intellect may be admitted, but that it is practically possible must be doubted. In introducing any new subject to the mind, surely broad outlines should be given first, and details filled in afterwards; observation requires teaching as much as any other faculty. Tyndall tells this story of Faraday. As Tyndall was about to show the latter an experiment, Faraday laid his hand on his shoulder and said, "Wait a minute; what am I to look for?" The application is plain—even Faraday felt the advantage of having the observer fore-armed.

Beginners know not what to observe, and cannot fashion experiments for themselves, and therefore it seems more rational, that students should have the recognised methods of science explained and demonstrated to them, and then be caused to repeat the necessary operations practically, numerical details being varied as in mathematical exercises. When thus equipped with sound theoretical knowledge and fair manipulative dexterity, they will be in a position to embark upon "research"; for they will then have acquired some power of observation, accuracy, and the faculty of making inferences. The "research method" *ab initio* appears like an attempt to teach a child to read before he knows his letters. I am fully conscious of my audacity in venturing into the lists, and am not ignorant of the sort of folk who "madly rush where angels fear to