

full references to the works in which each genus has been first described or best illustrated, with similar references to the authorities for synonyms, add further to the value of the work as a guide to the student of systematic botany.

The descriptive characters of the genera have been throughout verified or established after the previous examination of numerous specimens, and as a rule it may be said that for the purpose of this work the whole of the vast collections in the Royal Herbarium at Kew were passed in review, and especial attention given to the aberrant forms presented by many large genera. In the comparatively few cases where the authors were unable to refer to and examine specimens of a genus enumerated, they are careful to cite the author on whose authority it has been admitted. Genera that appear to the authors to have been founded on insufficient characters, or on an erroneous view of the structural facts, are in some cases reduced to the rank of subgenera or sections of the typical genus, in others simply recorded as synonyms at the conclusion of the description of the genus to which they are referred. There remains a further category of generic names given by authors who, either from ignorance of the science or incomplete materials, have failed to make it possible to identify them at the present day. These are enumerated as *Genera dubia* at the end of the synoptic table of the genera of each family. In short, it may be truly said that the authors have neglected nothing that could make their work useful and practical, as well as a complete storehouse of the present condition of our knowledge of this branch of natural science.

Of the many different points of view in which this great work may be regarded, the most interesting, perhaps, to the scientific naturalist is the consideration that we have here the results of a complete reconsideration of the whole subject of the classification of the flowering plants by two men of remarkable intellectual power, possessing an extent of knowledge and a command of materials far surpassing anything possible to the authors of preceding works of similar scope. In one or two brief sentences of a note already cited, Mr. Bentham has assigned the amply sufficient reasons which induced the authors to maintain in its main features the arrangement of the natural orders established by the elder De Candolle. Every attempt to set forth in a linear series the complex relations which connect together as in a network the various groups of the vegetable kingdom is necessarily incomplete and defective. It is a fortunate circumstance that the authors of the "Genera" have added the weight of their authority to the judgment of those botanists who hold that no one of the various arrangements which have been proposed during the last half century, and more or less extensively adopted in some parts of Europe, possesses advantages which can compensate the serious practical inconvenience of having systematic works of reference arranged after a variety of discordant systems. The Candollean arrangement has therefore been deliberately maintained in this work, with a few not unimportant modifications; but in the arrangement and grouping of the genera into tribes and subtribes there has been ample space for the exercise of the highest faculties of the philosophical naturalist. It is evident throughout the work that every question as it has arisen

has received fresh consideration, and in many important families the classification adopted is altogether new. It is remarkable that, even in regard to families previously elaborated by Mr. Bentham, he has not hesitated to introduce important changes suggested by further consideration and study. It is of course impossible to say that the final results of future discovery and research may not lead to further modifications in botanical classification; but for the present generation this will remain as the best result of the comprehensive survey of the whole field of our knowledge.

The number of genera described in the present work, taking into account the addenda, is 7565, while the number described by Endlicher in his "Genera Plantarum," with the supplements, is 7202. These figures give some measure of the progress of botanical discovery during the last thirty years, and at the same time some indications of the amount of labour involved in the collection and examination of the materials scattered throughout the numerous general works and monographs published during that period, and especially throughout hundreds of volumes of scientific periodicals which are now annually produced in every part of the world. The increase in the number of known genera is in truth much greater than the figures above cited would indicate, inasmuch as the tendency of Bentham and Hooker is to unite under the same generic designation plants which do not appear to present sufficient differences of structure, and they have not hesitated to suppress numerous genera that have been admitted in preceding systematic works of authority. Those who may not be disposed to acquiesce in these conclusions may easily continue to regard as genera the subgenera and sections whose distinctive characters are throughout the work subjoined to the descriptions of the respective genera.

It follows from the preceding remarks that for practical use in classing large botanical collections the present work is an indispensable guide. The present writer, who has enjoyed the advantage of daily, almost hourly, reference to its pages, feels that he is merely discharging a debt of gratitude in endeavouring to express his sense of the scientific value of a work which has become a classic from the day of its publication. A work which, at a given period, summarises the entire field of knowledge in one department of science, marks an epoch in its progress, and becomes the starting-point for further advance towards wider knowledge. Such is the work to which Mr. Bentham and Sir Joseph Hooker have devoted a full quarter of a century, and as such, notwithstanding the importance of their other works, it must remain their chief title to enduring fame.

ERN. COSSON

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Red Spot upon Jupiter

THE red spot on Jupiter has really disappeared. I have observed the planet again after conjunction. The region in which

the red spot formerly was is now very white; it passed over the central meridian of the planet this morning at 4h. 36m. (M.T. at Palermo), which gives for this place the Jovicentric longitude 63° , plainly corresponding to the longitude that Mr. Marth assigned to the red spot at present, if visible. This proves that the neighbourhood of the red spot had followed the particular motion of the spot itself. This place is well characterised by the permanent depression in the great reddish band of the planet.

A. RICCO

Royal Observatory, Palermo, September 10

"Elevation and Subsidence"

MR. O. FISHER has been so good as to offer a reply to my "remark with a query," his answer being (allowing for an obvious printer's error) that it is "an open question whether the melting temperature of rocky matter is, or is not, raised by pressure."

I cannot for a moment pretend to the same familiarity with the results either of experiment or of calculation as is doubtless possessed by Mr. Fisher. I only claim to speak as representing the class whose knowledge on these subjects is essentially second-hand; but, speaking as such, I think that Mr. Fisher's reply will not generally be regarded as satisfactory. I should, therefore, like to repeat my question with a little extension:—

1. Do not the "rigidity" calculations incontestably show that the earth is extremely rigid, *i.e.* solid? Are not, therefore, all theories which disregard this result (such as that the nucleus may be above its own critical temperature) put out of count?

2. Are not the phenomena of metamorphic and hypogene rocks on too large a scale to be accounted for by heat of merely local origin, whether produced by chemical or mechanical action, such as has been suggested in connection with volcanoes?

3. Do not all reasonable views of the origin of the earth, *i.e.* any form of the nebular hypothesis, point to the same conclusion as (2), viz. that the earth's heat is the residuum of a much greater amount formerly possessed, and not yet entirely lost by radiation?

4. Does not (3), taken in connection with the known laws of conduction, involve a continuous increase of temperature, whether rapid or slow, as we descend below the surface?

5. Although we may have no *direct* evidence as to the "temperature at depths bearing considerable ratios to the radius," is there not ample evidence that at comparatively insignificant depths the temperature is such as would melt not only "rocky matter," but far more refractory substances, if there were no counteracting influence? Even allowing a very slow increase, provided the increase is always positive, as 4 points out, should we not sooner or later almost certainly reach the melting temperature of the most refractory substances with which we are acquainted?

6. Can we then escape the conclusion, either that the nucleus consists of matter of a totally different kind from anything with which we are familiar, or that pressure raises its melting temperature? But does not every fact bearing on the question discredit the former hypothesis?

7. Should we not then accept the view that pressure does raise the melting-point of nucleus stuff, at least as a working hypothesis, only to be overthrown by direct evidence to the contrary, if direct evidence on the subject is ever forthcoming?

Trinity College, Cambridge

F. YOUNG

IN a paper I read before a full meeting of the Geological Association on March 2 last, of which a brief notice is given in *NATURE*, vol. xxvii. p. 523, I discussed the probability of subsidence of land, in certain cases, being due to *loading* by local accumulations of terrestrial matter acting upon a deflectible crust supported upon a viscous interior. The greatest effects, I imagined, from this cause, were due to local accumulations of ice past and present, particularly about the poles of the earth; but that secondary and important effects were due to the weight of accumulations of solid mineral matter from denudations being carried by oceanic currents and winds, from coral deposition, and the reaction of volcanic outflows. One illustration I proposed was that the sinking of the coast of Greenland was probably due to the weight of inland accumulation of ice, which proposition I thought was original, but Mr. Gardner (*NATURE*, vol. xxviii. p. 324) says—"It has often been supposed that the sinking of the coast of Greenland is similarly due to its icecap." I should

feel obliged if Mr. Gardner would point out references where this has been proposed, as I thought I had read the literature of the subject, and I fear that this part of my paper is less original than I assumed.

W. F. STANLEY

THAT there is a connection between sedimentation and subsidence on the one hand and between denudation and elevation on the other is a fact now admitted by most geologists. The real question to be answered, however, is:—Are these directly connected as cause and effect? or are they simply concomitant effects of the same cause? If the first be true, we should expect cause and effect to vary together, that is, that subsidence should keep an even pace with sedimentation. That this has not been exceptionally the case is proved by the sections of the carboniferous system in the central valley of Scotland, where the facts point to a continuous subsidence, accompanied by a very irregular sedimentation, with the result that now subsidence gained on sedimentation, now sedimentation on subsidence. Again, once the process commenced—and it is not very evident how on an originally even surface it could have commenced at all—we should expect it to be continuous. Sedimentation causes subsidence, subsidence gives rise to fresh sedimentation, and that again to renewed subsidence, and so on and on. Consequently we should expect that when once an area of sedimentation and subsidence was formed, it would continue an area of sedimentation and subsidence through all geological time.

It appears rather, I think, that the connection between them arises from their being concomitant effects of lateral pressure in the earth's crust (for notwithstanding the Rev. O. Fisher's masterly exposition of the inadequacy of this cause to produce the observed inequalities of the earth's surface, I still believe that, with the exception of the ocean basins, which must be otherwise accounted for, it is quite competent to account for the facts). We may suppose the action to take place so:—

A certain portion of the earth's crust is first thickened and strengthened by volcanic outburst or other accumulation on the surface. This part, when the tangential thrust comes, offers, by reason of its increased weight and thickness, a greater resistance to the elevating force than the parts around, and as a consequence these are raised around the thickened part, while it is at the same time depressed in a corresponding degree; in other words it becomes the centre of a syncline, while the strata around are raised into anticlines. Depression naturally leads to sedimentation, and this still more thickens the part, and enables it to offer greater resistance to the tangential thrust, with the result that it continues to be depressed as the strata around are elevated. The converse is also true. Denudation means the thinning and consequent weakening of the crust, and hence when the thrust comes the denuded part is the more likely to be elevated into the anticline.

This theory provides for the cessation of the phenomena, since the tension of the crust is after a time relieved. It also accounts for the fact that strata around volcanoes and volcanic necks, as also along the base of mountain chains, so frequently appear to dip below them. The rate of subsidence, too, would vary with the intensity of the exciting force, though the consequent sedimentation need not vary with it in the same absolute degree.

Perth, September 3

WILLIAM MACKIE

MY article on elevation and subsidence has provoked considerable and, on the whole, friendly criticism, a so far satisfactory result, though but few points have been raised requiring reply. Dr. Ricketts objects, and very properly, that I have not alluded to his many writings on the subject; and to this I can only plead want of space, that I have not entered at all into its already voluminous bibliography, and that my article was written and in type before his recent contributions to the *Geological Magazine* had appeared. Beyond this I had sufficiently indicated that there were many observers in the field, and every geologist must be aware that the subject has for a long while past excited attention not only in England but in France and America.

The fundamental error in my article is pointed out by the Rev. Mr. Fisher and by Mr. Young, and the assumption that inert pressure induces heat must be abandoned. As I had read the "Physics of the Earth's Crust," I expected that this would be challenged, but I let it stand, as the fallacy has been shared by a large number of geologists, comprising some of the most distinguished, and has even escaped the correction of physicists. But this rectification, while very important, by no means affects the results, and on the contrary facilitates an appreciation of the