

Tyrolese Alps, and has even attempted to introduce into a small plot of ground in the mountains surrounding Innsbruck, a number of plants indigenous to the lowlands of the Tyrol. In this enterprise, however, he met with no very encouraging success; "the greater number of the plants which I brought to those heights with inexpressible toil, succumbed to the uncongenial Alpine climate; and in the remaining small portion, I have noticed at present only very unimportant changes." His conclusion from these experiments is "that changed conditions of life can kill the species, or they can reduce it to a starved existence, but can in no case produce a *direct* change into a new permanent species adapted to its altered conditions." Such change can only take place by the slow process of natural selection among slightly varying offspring from the parent species. The writer notices a number of interesting features that characterise the Alpine flora with which he is familiar, as contrasted with those found under other climatal conditions. One of these is the very small number of annual plants, which bear to perennials the proportion of 4 to 96, as contrasted with that of 42 to 58 in the Mediterranean district, and of 56 to 44 in that of south-eastern Europe; a result of the very short period of summer warmth, varying from $1\frac{1}{2}$ to $3\frac{1}{2}$ months, which does not allow time for the seeds to ripen. The same cause produces also the appearance in many Alpine species of the flower-buds at the close of the summer, ready to burst into blossom during the first days of returning warmth in the spring. The remarkably large proportion of Alpine plants with evergreen rosettes of fleshy or succulent leaves, *Primulas*, *Gentianas*, *Androsaces*, *Saxifragas*, *Drabas*, &c., he attributes to the advantages of some contrivance for obviating the effects of the intense heat of the sun during the long days in their short summers, and also to the necessity that the plant should possess leaves at the very commencement of the warm season, in order to afford it a store of nourishment, and thus economise the whole of the brief period of vegetation. With this peculiarity he contrasts the poverty of the Alpine flora in plants possessing stores of *underground* nourishment in the form of bulbs, a class so abundant and prominent in the south of Europe. The necessity for great caution in deriving general conclusions from a small array of facts, is shown by the mention by M. Kerner, among the plants well adapted by their constitution to withstand the great alternations of an Alpine climate, of *Dryas octopetala*, a species which flourishes equally well in the remarkably uniform climate of the west coasts of Ireland and Scotland. The want of any considerable number of large shrubs and forest trees is obviously due to the rigours of the climate; and the almost entire absence of climbing and creeping plants indicates that protection from the sun is not one of the first conditions of existence, as it is in tropical forests. The large proportion of plants with flowers of intense hues, and the deficiency of spiny and stinging species, are not so easy to account for, though the author attributes the latter to the comparative absence of destructive animals; and the former may possibly have some connection with the advantage derived from the speedy attraction of insects, after the flowers expand, to assist in their fertilisation. We can conceive no greater service to biological science than a series of observations on the floras of limited areas, both with respect to what they possess and to what they are deficient in, carried out with the care of those recorded in the work before us.

A. W. B.

The fourth volume of the *Atti della R. Accademia delle Scienze di Torino*, contains several important papers on various departments of science. We may notice especially Prof. Salvadori's memoir on some birds from Costa Rica, and the same author's monograph of the genus *Ceyx*; a memoir by M. F. Giordano, on the orography and geological constitution of the Gran Cervino; and mineralogical papers by Prof. Strüver and Dr. Cossa.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Evidence concerning Heterogeny

THE question of the truth or falsity of "heterogeny," as it is called, is perpetually recurring both in your columns* and elsewhere, in connection with several of the most important scientific controversies of the day. It is a subject which has engaged my attention for several years, and I am anxious to be permitted to lay before your readers as concisely as possible a statement of what appears to me to be its present position.

I think the impression which most recent references to it are likely to leave on the minds of readers is this—viz., that while amongst the most advanced thinkers there is a gradually strengthening conviction that the weight of theoretical considerations is in favour of the actual existence of heterogeny as a real mode of origin of living beings, yet that the authority of M. Pasteur's famous researches inclines the balance of experimental evidence heavily the other way.

I am perfectly willing to admit the principle of authority in matters scientific as far as any reasonable person can admit it—that is to say, I believe it is natural and right that when a scientific man of so deservedly high reputation as M. Pasteur publishes a long series of carefully conducted researches, and announces the conclusions to which they lead him, and when no evident flaw can be shown in his processes, either of experimentation or of reasoning, his conclusions should be accepted as against those of another comparatively unknown experimenter. But in the present case neither of these conditions is fulfilled. In the first place, instead of merely an unknown experimenter we have in this case a consensus of at least eight experimenters—some of them by no means unknown—who have given their best attention to the same investigation in different parts of the world and under widely differing conditions, and who agree in disputing M. Pasteur's results.†

In the next place, there is a step in M. Pasteur's experimentation which has been pointed out as a flaw, and of which it is in the power of any one of your readers to judge for himself by a process of simple inspection as to whether it really be a flaw or not. I refer to the subject of the microscopic power used by M. Pasteur in his investigation. The organisms which I found during my own experiments on this subject appeared often but little larger than a full point in the type used in your columns, even when seen with a power of 1700 diameters. In the woodcuts attached to my paper in the Proceedings of the Royal Society (April, 1865), they may be seen as they were kindly drawn for me by my friend Dr. Beale, who, though an uncompromising opponent of the doctrine of heterogeny in all its forms, is a very high authority in microscopy; there can therefore be no question as to their size, nor as to their actual presence in the experimental vessels. Now under the power used by M. Pasteur (350 diam.) these organisms would be about $\frac{1}{5}$ part of the size which they appear in the drawings. Yet it is upon the authority of observations made with such a power that M. Pasteur has pronounced, not upon the *presence* of these objects, but upon their *absence*. It is now nearly five years since my observations were made public. Since that time several critics have noted them as requiring an answer, but, so far as I am aware, no answer has been made to them; and meanwhile naturalists have gone on complacently quoting M. Pasteur's experiments as having settled the question against heterogeny, even though they have not failed to acknowledge the weight of the theoretical considerations which tell in its favour.

The theoretical aspect of the question I have fully discussed elsewhere,‡ and I will only here state my entire agreement with the belief expressed by Dr. Charlton Bastian in your issue of February 24, viz. "that the time is not far distant when the doctrine of the evolution of living things will be as much an

* NATURE, Feb. 1, p. 351; Feb. 24, p. 424.

† 1. Pouchet, "Nouvelles Experiences" passim. 2. and 3. Joly and Musset, *Comptes Rendus*, 1860. 4. Schaafhausen, of Bonn, *Comptes Rendus*, 1860 and 1862. 5. Mantegazza, of Pavia, see *Cosmos*, 1863, p. 630. 6. Wyman, Harvard, U.S., *American Journal of Arts*, &c., vol. xxxiv., July 1862, vol. xlv., September 1867. 7. Proceedings of Royal Society. G. W. Child, Oxford, June 1864 and April 1865. 8. Hughes Bennett, Edinburgh, *Edinburgh Medical Journal*, March 1868.

‡ "Essays on Physiological Subjects," 2nd edition, pp. 137—154.

accredited dictum of science as are the other doctrines of the correlation of the physical forces and of the correlation of the vital and physical forces which have been its necessary predecessors."

GILBERT W. CHILD

Elmhurst, Great Missenden, Bucks.

Prismatic Ice—Sandstone Boulder in Granite.

THE "two phenomena" observed on Dartmoor by Mr. C. Spence Bate and Mr. W. Morrison, and described by the former in *NATURE* of the 31st ult., have been previously noticed.

The late Rev. Dr. Scoresby, F.R.S., published a paper "On Columnar Crystallisation of Ground Ice," in the *Edinburgh New Philosophical Journal* for January 1850 (vol. xlviii.), and illustrated it with a plate containing eighteen figures. A presentation copy of this paper is now before me.

The so-called *sandstone boulders* in granite are by no means rare. They occur in various parts of Devon and Cornwall. I first noticed them at Shapton, near Bovey Tracey, in Devonshire, and have subsequently seen them in several other localities, but nowhere in such abundance as at Sennen Cove, near the Land's End, in Cornwall. There are several good specimens in my private collection. The following description of them occurs in a paper on "The Age of the Dartmoor Granites," which I read to the British Association at Manchester, and to the Royal Geological Society of Cornwall in 1861, as well as to the Devonshire Association in 1862. "Nodules, apparently segregative, sometimes occurring in the substance of the ordinary granite, might, from the fineness of their grain, be almost mistaken for sandstone; indeed, I not long since heard them appealed to as proofs of the metamorphic origin of granite. 'Here,' said the appellant, 'are unaltered remnants of the old sandstone rocks, which, with these exceptions, metamorphism has converted into granite. I do not quote this for the purpose of endorsing it, but simply to show the general dissimilarity of the nodules to granite proper. Excepting their darker colour, they reminded me much of the granite veins which pass through the older granite of Goatfell, in the Isle of Arran; nevertheless, they are not veins but nodules, and capable of being extracted as such from the granitic mass containing them. . . . They consist of very fine grains of quartz and schorl in about equal quantities, or with the latter somewhat preponderating.'"

Irrespective of the origin of the nodules, it is no doubt "clear that when this granite was formed, the temperature of (the surface of) the earth must have cooled down to below the boiling point of water," for the granite, as has long been established, is of post-carboniferous age; or, in other words, was formed after the rich faunas and floras of the Silurian, Devonian, and Carboniferous periods had successively passed away, to say nothing of the pre-Silurian organic eras.

WM. PENGELLY

Torquay, April 2

The Transits of Venus in 1874 and 1882

IN the paper on this subject by P. L. S., there occurs a remark which is calculated to convey a mistaken impression. He states that "an Antarctic station is only required for the transit of 1882, and there is ample time to make a preparatory Antarctic expedition to ascertain" whether a suitable station can be found. The reverse is the case. No Antarctic expedition can be of any service in 1882, so that in a preparatory expedition the lives of our seamen and men of science would be uselessly risked. On the contrary, there are several Antarctic stations suitable for observing the transit of 1874; and I have shown that the comparison of observations made at such stations with observations made in Siberia would give the most effective means of determining the sun's distance available before the 21st century.

I may remark here, that the choice of stations for observing the transits of 1874 has been founded on calculations admittedly inexact, and it would be to the credit of English astronomy that the whole matter should be re-examined while there is yet time for a change to be made. In saying this, I am not by any means insisting upon the views put forward in my own papers on the subject; though the only error pointed out by the Astronomer Royal in my charts and calculations consists in the fact that they aim at an unnecessary exactness. But the utilisation of the

coming transits is a matter too important to be endangered for any personal considerations whatever. If errors have been made it behoves men of science to see that those errors shall not be suffered to prejudice the cause of scientific progress.

RICHARD A. PROCTOR

Euclid as a Text-book

I REGRET that Mr. Wormell has imported so much of a personal nature into his reply to my former letter. Personality and unintentional misrepresentation appear to me to be its predominant features. Unintentional, I say, for I know little of the writer beyond the fact of his being the author of two or more admirable text-books, and that he is a distinguished member of the London University.

Though I feel that the columns of *NATURE* ought hardly to be taken up with such matter, yet, in self-defence, I am compelled to say a few words. As I have neither time nor inclination for controversy, I hope that the discussion, if continued, will be entirely *ad rem*, and not diverge into personalities. Owing all my geometrical ability (*quod sentio quam sit exiguum*) to a twenty-three years' acquaintance with Euclid, and having had, as a teacher, to use that author for the last fourteen years, it would not be strange if I were a favourer of the old system, which I am not to the extent Mr. Wormell seems to think.

My plan of teaching geometry under the old system was to overcome Euclid's deficiencies by *viva voce* explanation, and, offering slight assistance, to get my classes to work a number of geometrical exercises. With my sixth class I have generally got well through three or four hundred such exercises as are given in Todhunter's edition of the Elements.

This is not the same as sending out boys who have merely "committed Euclid to memory," and certainly my pupils have found no great difficulty in the matriculation papers. Pupils thus prepared have taken first, second, third, and other high places in the examination, which places, I think, were in a measure due to their "flooring" the geometrical papers—with the exception, perhaps, of a "rider;" also, during the time I have held my present post, my pupils have carried off the Andrews Entrance Exhibition at University College each year, with one exception, when the finest geometer I have had was beaten. This is not the place for chronicling successes in other examinations.

I did not state that it was advisable for students to read Euclid only; what I did say was to the effect that I had heard of boys who were doing this with the idea that such a course would "pay" best. Mr. Wormell charges me with using an "infelicitous and ungenerous expression." That I willingly retract, as it has struck myself as being uncalled for; but Mr. Wormell must have read my purposely concise letter hastily, for I nowhere say that I desire a change in the syllabus; the syllabus is excellent, and I quite agree with him in the remarks he adds about the "unflinching courage in the reform of English methods of education" as far as regards the matter under discussion. But what is possible is, that the examiners, being chosen from the older universities, may overlook this distinction; until now, I have had to regard the papers from the old point of view, in which light they have suited me exactly; Mr. Wormell has viewed them from the modern stand-point, and bears the like testimony; this being so, it must be admitted that the examiners have well carried out the syllabus. That I should "impeach the integrity" of such men as the present examiner, from whom I have always experienced the greatest kindness, or the late examiner, one of the most successful teachers of my own university, would be absurd, were it not that it pains me to have it supposed. To return, I do not want quite such a change as Mr. Wormell thinks; the difficulty in my case has not yet arisen, for I have not yet sent in pupils whose training has been wholly confined to the new Geometry, and I wished to have the change made, if any were necessary, before sending them in. The difficulty will not be so great when we have obtained a thoroughly good modern text-book; ours is a very good one, but there are blemishes which will doubtless be removed in a new edition, and to adapt it to the matriculation scheme more propositions than at present must be proved, as I think, independent of proportion. I applied the term "Euclidean type" to the recent examination paper, because the questions are given in the exact words of Euclid; I would have this changed; they follow in the order assigned in the Elements, and perhaps my experience of

* See *Geologist*, 1863, p. 15; Trans. Roy. Geol. Soc. Com., vol. vii., p. 425; or Trans. Dev. Assoc., 1862, p. 50.