recent of the electrical inventions, Mr. Munro has tried to accomplish a well nigh impossible task. That he should have been perfectly successful in his endeavour is hardly to be expected; nevertheless he has produced a book which a person unacquainted with electrical science may read with pleasure, and from which such a person may learn what wonders are accomplished by the aid of electricity, and in a general way how this powerful and subtle agent does its work.

The first thirty-one pages, in which the author gives so much of the theory of electricity as may be necessary to enable any one to understand his descriptions of the inventions which follow, form without doubt the weak part of this book. In describing the effect of grouping thermopiles and elements in series, the author seems to have confused electromotive force with current strength, for he says that with such a combination a powerful current equal to the sum of the elementary ones will circulate in the connecting wire. The short chapter on induction is likely to cause in the mind of a person unacquainted with electrical science some confusion between statical and current induction.

With the fourth chapter a description of the inventions begins, and here it may be said that the book proper begins. The chapters on the telegraph and telephone and all the inventions which depend on the telephone are excellent, the general principles being so clearly given as to be readily understood. The theory of the dynamo is too difficult for an essentially popular book, and here, as in one or two other places, the author has wisely refrained from leading his readers into a sea of complexity from which they could with difficulty have escaped, but has carried them over with an agility worthy of a conjurer. The remaining chapters, which deal with lighting, transmission of power, heating, and plating, are written in a popular style, and will no doubt be read with interest.

C. V. B.

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.]

Professor Henrici's Address at Southport

MR. J. J. WALKER, in his letter printed in your last number (p. 515), draws attention to the works of Chasles, and I am glad that he has thus given me an opportunity of saying a few words about the relation in which the writings of this great geometer stand to those of the German geometers mentioned in my address.

I am fully aware that his works are well known in England, and so I believe are those of Poncelet and others. But the study of the works of Chasles does not give as complete a view of the variety of methods invented and results obtained on the Continent as might be expected from the author of the "Aperçu Historique" (1837). In that brilliant work he regrets himself that he does not understand German, and does not therefore give an account of what was done in Germany by "Steiner, Plücker, Möbius, &c.," and it seems that he always remained ignorant of it. At all events he did not fill up those gaps in his important "Rapport" of 1870. In much of his own work contained in his "Aperçu" he had been anticipated by Möbius and Steiner, and his "Géométrie Supérieure," which appeared twenty years later than Steiner's "Entwickelungen," and twenty-six years later than the "Bary-centrische Calcul," by Möbius, and which is, I believe, in England considered the chief book from which to learn modern Continental geometry, must not be taken as a standard of what at the time of its publication was known in Germany.

With regard to the arithmetic, I beg to point out that I only judge from my experience as an examiner. The methods of

abbreviating calculations with decimals must have been known long before De Morgan, I fancy, but that is a very different thing from having them introduced as an important part of the teaching of arithmetic in schools.

The manner in which a large number of candidates worked examples at the London University Matriculation Examination startled me considerably, especially as I noticed that the process of working decimals described in my address was used by candidates who otherwise gave evidence of really sound knowledge and good teaching.

O. HENRICI

The New Comet

THE new comet (Pons) was seen here last night in the 6-inch equatorial, its place closely corresponding to that of the ephemeris contained in the recent *Dun Echt Circular*.

In the comet eyepiece it was large, round, and faint, with no tail and but little trace of central condensation. It might, in sweeping have been taken for a nebula, having very much that look. I could not see it in the 2-inch finder, and though fairly visible in the comet eyepiece (power 35), a very little mist that came up rapidly obscured it.

J. RAND CAPRON

Guildown Observatory, Guildford, October 2

The Genus "Simotes" of Snakes

IN the report of the Committee appointed by the British Association for the investigation of Timor Laut, given in at Southport during the recent meeting, I find that among the snakes discovered by me one has been described as a new species of *Simotes*, and is noted as being of "special interest, as no species of the genus had hitherto been previously known to occur eastward of Java." In the *Proceedings of the Zoological Society* for the year 1864, p. 180, a species of *Simotes*, *S. australis*, was described by Gerard Krefft, from Port Curtis, Australia, as being "the first *Simotes* discovered in Australia."

Aberdeen, October 1

HENRY O. FORBES

Floating Pumice

REFERRING to a note in the last number of *NATURE* (p. 532), giving an account of a steamer's having encountered vast quantities of pumice in the Indian Ocean, it may be of interest to record that after passing, in the R.M.S. *Quetta*, the Straits of Sunda on July 9 last (having sailed close under the then active Krakatoa), we traversed a continuous field, unbroken as far as the eye could reach, of pumice, every day till the evening of the 12th, when our position must have been six hours (± 60 miles) to the west of our noon position, $93^{\circ} 54'$ E. long. and $5^{\circ} 53'$ S. lat. Capt. Templeton assured me that there was, singularly enough a current against us all the way from the Straits of one-third of a mile per hour. There can be no doubt that this pumice came from Krakatoa, and possibly also that mentioned by the steamer in your note last week. The pumice knobs were all water-worn, and a few had barnacles of about one inch in length growing on them. It will be recollected that the eruption first broke out on May 22 and 23.

Aberdeen, October 1

HENRY O. FORBES

"Elevation and Subsidence"

IN the number of *NATURE* for September 20, Mr. W. F. Stanley (p. 488) requests references to where it has been considered that the sinking on the coast of Greenland is due to the weight of inland accumulation of ice. If Mr. Stanley has only so lately as the present year advocated this opinion, though doubtless the idea has arisen independently with him, he certainly has no right to consider himself the originator of it, which he seems disposed to do. So far as I am aware the priority is due to Mr. T. F. Jamieson (*Quart. Journ. Geol. Soc.* vol. xxi. 1865), who attributed the subsidence, which is universally conceded to have occurred during the Glacial period, to the enormous weight of snow resting on the land, considering that if the interior of the earth on which the crust rests is in a state of fusion, a depression might take place from a cause of this kind; and then the melting of the ice would account for the rising of the land which seems to have followed upon the retreat of the glaciers.

Unaware of this proposition, in 1871 (President's Address, *Proc. Liverpool Geol. Soc.* 1871-72; *Geol. Mag.* vol. ix. 1872) I in the same way ascribed the subsidence of the land during the Glacial period to the combined weight of snow and the boulders

clay, that is of the mud, &c., which then issued in streams from beneath the glaciers, and contains the pebbles derived from distant localities which had drifted in icebergs and coast-ice, and been dropped into it, the land being raised to a considerable extent when, upon the return of a more genial climate, it was relieved of its load of ice and snow.

Prof. N. S. Shaler, of Harvard College, U.S.A., in 1874 (*Mem. Boston Soc. Nat. Hist.* vol. ii.), considering that by the hypothesis of Adhemar the conditions which would have resulted were not fulfilled during the last Glacial period, concluded that we may more reasonably look to the weight of ice then accumulated on the continents for the depression of the land areas it occupied.

In a paper on the "Cause of the Glacial Period," read before Section C at the meeting of the British Association at Bristol, 1875 (*Report*, p. 79; also *Geol. Mag.*, Decade II., vol. ii.) I adduced evidence tending to prove that such a subsidence of the Isthmus of Panama has taken place as would allow a diversion of the equatorial waters of the Atlantic into the Pacific; as a consequence of which similar effects to those which occurred during the Glacial period might have been produced. The formation of the Canal ought to afford to competent observers absolute proof whether such has been the case or not. In the course of the essay I ascribed not only the subsidence during the Glacial period, but also that now rapidly progressing in Greenland, to the weight of the greatly increased accumulation of snow; and that the rise of the land in Norway is dependent on the removal of pressure by the melting or diminution of the glaciers. It does not appear unlikely that to a great extent the rising of the Andes may be due to the dissolution of the snow which once covered these mountains in a greatly increased degree, it may have been contemporaneously with the Glacial period in Europe and North America; and in part to the transfer of pressure, by the materials derived from its flanks and brought down by the Amazon and its tributaries forming at its delta the "measures" in this great coal-field of South America now in process of formation.

Mr. J. Starkie Gardner, at a much later period (*Geol. Mag.*, June, 1881) stated that great accumulations of ice in the Glacial period seem to have been accompanied by subsidence, and even Greenland at the present day may be sinking under its ice-cap. In the same year the Rev. O. Fisher, in "Physics of the Earth's Crust" (p. 223), accounted for the raised shell beds found in Scandinavia at an altitude of 700 feet, by the country having been formerly depressed owing to its being loaded with heavy ice fields, and that its gradual subsequent rise may have been caused by the ice having melted off. He remarks that similar movements have occurred and are now going on in Greenland; and that the subsidence of 6 or 8 feet in a century may possibly be accounted for by the snow-fall being at present greater than is carried off by the glaciers and evaporation.

During the present year Mr. W. F. Stanley (*NATURE*, vol. xxvii. p. 523) held that the cause of the coast of Greenland sinking is the weight of the present accumulation of ice upon that continent. Quite recently Mr. Searles V. Wood (*Geol. Mag.*, July, 1883, footnote) thinks that the overwhelming of reindeer pastures by the ice during the centuries of Danish occupation, and the indications of subsidence afforded by the position of ancient dwellings, may show that the ice is now augmenting, and the land sinking under its weight.

But the great question is not to whom belongs the priority of attributing the depression of the land during the Glacial period, and at present in Greenland, to the weight of accumulated snow, and the relevation to its removal; even this explanation of the phenomena under consideration, important though it may be, is but an item in the still greater one, namely, whether the depression which has taken place and is still in progress at the mouths of great rivers, in their deltas, in the estuaries and bays into which, emptying themselves, they carry mud, sand, pebbles, and other debris, is caused by the weight of these accumulated deposits pressing down the crust of the earth beneath them, thus permitting further accumulation to any extent; and also whether the subsidence, which by every one is conceded to have occurred during the deposition of all stratified rocks, from the earliest of which we can read the record in the "great stone book" to those now in progress, is due to the same cause—the weight of the materials of which they have been formed.

The converse has also to be inquired into, whether the elevation of the land and the formation of hills and mountains is the result of the abrasion of the land and the transfer of the disintegrated materials to a distance by rain and rivers; thus relieving

by so much the locality from which they have been removed of the weight pressing on the crust of the earth. The highest hills in a district are those from which the greatest amount has been removed by denudation, their summits not unfrequently consisting of the lowest rocks in the geological series of the neighbourhood.

Birkenhead, September 22

CHARLES RICKETTS

I QUITE agree with Mr. Mackie in believing that "the connection between sedimentation and subsidence on the one hand, and between denudation and elevation on the other," are "simply concomitant effects of the same cause;" that, in fact, depressions in the earth's crust are the cause of sedimentary deposits, and not the deposits the cause of the depressions, and, further, that the elevations and depressions are caused by lateral pressure developed by the shrinking of the earth's crust; but is it necessary that certain parts of a depressed area should be "strengthened by volcanic outbursts, &c."? I do not think so.

If a magazine or book with a paper cover be held close, and pressed from back to front, the mass of the leaves is thrown into anticlinal and synclinal curves though the book is at no point stronger than at any other; the pressure is brought to bear upon the book, and as it cannot "telescope," it is necessarily bent upwards and downwards. Is this not something like what happens to the rock?

Take, for example, the Old Red Sandstone between the base of the Grampians and the Carboniferous rocks of Fife. This is a plain partly composed of sandstones, partly of sandstone with interbedded volcanic rocks, and partly of solid masses of volcanic ejecta. The plain has been bent into two anticlinal and two synclinal curves.

In such a varied area, if anywhere, one would expect to find evidence of the influence of the relative strength of the rocks in modifying their curvature.

The syncline nearest to the Grampians is mainly composed of sandstone and conglomerates; as these rocks bend up towards the anticlinal axis to the south, the Sidlaw Hills (composed of hard sandstones and interbedded porphyrite, &c.) present a very striking example of strengthening of the beds; still they are neither on the anticlinal nor the synclinal axis, for though near to and towering high above the former, they lie on the slope of the beds dipping towards the north. The rocks of the second syncline are sandstones with intrusive and interbedded lavas, the volcanic rocks greatly increasing in proportion to the sedimentary towards the synclinal axis near the estuary of the Tay, towards which the rocks are gently bent up, while across the estuary, which occupies the position of the denuded arch of the anticline, the rocks are almost entirely sheets of lava, with volcanic breccias, &c.

Thus we have a synclinal and anticlinal curve, both of sandstone, while the hard and thickly-bedded volcanic rocks form part of the slope between them, and again we have a syncline partly composed of interbedded lavas and sandstones, while the almost entirely volcanic rocks are bent up into an anticline.

It would therefore seem that the quality and thickness of the rock masses have very little influence upon the form of the curves into which they are bent.

JAS. DURHAM

Newport, Fife

Photography and Still Life

I HAVE been assured, by a gentleman to whose opinion all dabblers in science photography must bow, that the following method of photographing objects of still life was unknown to him, and that its publication might prove useful to others.

Having some years ago to photograph a series of implements to illustrate a paper on the Borness Cave, I was met at the outset by the difficulty of avoiding cast shadows and such accessories as were needful for posing the objects to be copied. It occurred to me that a pane of glass, a white cloth, and some beeswax would meet the difficulty; as objects fixed to the glass by beeswax with a white cloth behind them would "come out" on a white ground free from the shadows and accessories I wished to avoid. Having been recently asked to photograph some important bones, teeth, and flint implements, necessity, "the mother of invention," has much improved on the original rough process, and I can confidently recommend the following cheap apparatus as extremely efficient, viz., a square pane of plate glass with a hole drilled in the centre (for fastening such