enough to show that the book ought to be read by every one, and therefore we shall now conclude by drawing more prominent attention to sundry opinions and suggestions, which, as Englishmen, we should desire to see our Government consider and act upon.

First, as regards the promotion of science :—

"The extraordinarily favourable climate and position of Peradenia especially fit it for more extensive use from a scientific point of view as a botanical station. In the same way as our young zoologists find the recently established zoological stations on the sea coast (at Naples, Roscoff, Brighton, Trieste, &c.) of inestimable value for their deeper scientific studies and experiments, a year's residence in such a botanical station as Peradenia would give a young botanist more experience and work than he could obtain in ten years under the various unfavourable conditions at home. Hitherto, less has been done in the tropical zones than elsewhere for such establishments for study and experiment, though they would be exceptionally beneficial. If the English Government would establish and maintain such a station for botany at Peradenia, and one for zoology at Galle—in the charming bungalow, for instance, belonging to Capt. Bayley, which is admirably suited to such a purpose [and would be sold by the owner to effect it]—they would be doing signal service to science, as they have already done by the *Challenger* Expedition and other great undertakings—and once more put to shame the great Continental States of Europe, who spend their money chiefly on breechloaders and big guns."

In reading this passage all true Englishmen should feel regret that their Government is not deserving of the meed of praise which the courtesy of the writer bestows. Seeing that we are the great maritime and colonising power, it is nothing short of a public disgrace that we are without a zoological station upon any of our thousands of miles of coast, and that hitherto there is no prospect of our escaping from the sarcasm (whether conscious or unconscious) wherewith the national seat of "deeper scientific studies and experiments" in marine biology is here specified as *Brighton*. Is it too much to hope that the Fisheries Exhibition may at length help to open the eyes of a Liberal Ministry to the importance of doing something in this direction?

Only in one particular does the English rule in India fall under censure, and this has reference to the atrocious treatment of the stage-coach horses. The scenes described are certainly monstrous beyond imagination—flogging by the whole village, dragging by the nostrils, wringing by the ears, and burning with torches. Truly, as Haeckel observes: "It is difficult to conceive how the English Government, which is generally so strict in its arrangements and discipline, has not long ago put an end to this brutality to animals, and more particularly extended its protection to the wretched horses that serve the 'Royal Mail Coach.'" Here is surely something for the anti-vivisectionists to memorialise upon with benefit.

We cannot take leave of this delightful book without congratulating the translator on the beautiful English into which she has rendered it.

GEORGE J. ROMANES

OUR BOOK SHELF

Elements of Histology. By E. Klein, M.D., F.R.S., &c. (London: Cassell and Co., 1883.)

THIS, which is the first of Cassell's "Manuals for Students of Medicine," contains 342 closely-printed

pages, with 168 well-executed woodcuts, mainly reproduced from Klein and Noble Smith's "Atlas of Histology," or the "Handbook for the Physiological Laboratory," intercalated in the text. It is not too much praise to say that the information in this little volume is generally very complete, quite up to date, and written in a concise, though, at the same time, thoroughly clear style.

Dr. Klein wisely omits all reference to the titles of works and papers, introducing where necessary simply the name of the discoverer of, or observer most intimately associated with, the structure referred to. Where different opinions exist, this is obviously convenient, and the right thing to do; but why on page 7 the names of fifteen histologists, followed by the words "*and many others*," should be given, it is difficult to understand, especially as they are quoted with reference to the indirect division of nuclei or Karyokinesis, of which every worker at histology must have seen many examples.

In a work like the present, where all usually received ideas are given, it is curious to find that no reference is made to Schäfer's with regard to striated muscle. Surely this cannot be an accidental omission, especially as Haycraft is twice quoted.

That the action of tannic acid on human red corpuscles is not described in the text, although figured (p. 9, fig. 9a), is clearly an oversight, as that of boracic acid on newt's red corpuscles is both figured and described. In future editions it will be convenient if the same numbers be used in the text as in the diagram when describing the different parts of the kidney tubules, constant reference to the description of fig. 133 being now necessary.

With the exception of the above minor details, unqualified praise must be given, and the "Elements of Histology," which is really a very complete manual, should be used and re-used by every student and practitioner of medicine who wishes to acquire a sound knowledge of the normal histology of man.

J. W. G.

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.]

"Elevation and Subsidence" again

A LETTER appears yesterday, again criticising Mr. Starkie Gardner's general views about pressure, in the same sense as was done by myself a fortnight ago. But, referring to that gentleman's opinion that pressure can render rocks molten or fluid, Mr. Young goes on to remark: "Is not the supposition the exact reverse of what is really the case, viz. that not only does pressure *not* liquefy rocks, but actually *prevents* their melting at a temperature at which they *would* melt were the pressure removed?" Your correspondent, offering this remark with a query, seems as if his mind was not quite made up on the subject; and with reason; for it must, I think, be considered at present an open question whether the temperature of rocky matter is, or is not, raised by pressure.

Sir W. Thomson stated, in an address to the Geological Society of Glasgow in 1878, that certain experiments by Dr. Henry Muirhead and Mr. Joseph Whitley seemed to show that iron, copper, brass, whinstone, and granite are less dense in the solid than in the liquid state at the melting temperature. If so, pressure would assist in liquefying these substances. On the other hand, some observations of Mr. Johnston-Lavis, made on lava at Vesuvius, point in the opposite direction. Granted that the earth is, as a whole, extremely rigid, we cannot gather from that fact any certain information about the effect of pressure on "rocky matter," when near the melting temperature. We do not know whether the nucleus of the earth consists of matter which could, under any conditions, be directly converted into surface rock; nor yet do we know anything certain about its

temperature at depths bearing considerable ratios to the radius. Indeed the state of our knowledge is best expressed by the words of the old song, "Oh dear! what can the matter be?" It is even conceivable that, whatever it be, it may be above its own critical temperature; in which case the laws affecting incompressible liquids become inapplicable.

An interesting paper upon this latter hypothesis was published by Prof. Zöppritz in the *Transactions* of the first Geographical Congress of Berlin, 1881. It is entitled "Ueber die Mittel und Wege zu besserer Kenntniss vom inneren Zustand der Erde zu gelangen," and published by D. Reimer, Berlin.

Harlton, Cambridge, August 24

O. FISHER

I OBTAIN NATURE in monthly parts, and am indebted to a friend for calling attention to the article on "Elevation and Subsidence" by Mr. J. Starkie Gardner in vol. xviii. p. 323, in which he considers that, "wherever considerable weight is added to any part of the earth's surface, a corresponding subsidence of its crust almost invariably follows." As it is evident from the last paragraph in Mr. Gardner's paper that he esteems this opinion to be novel to the readers of NATURE, and being the first time it can be considered as having been discussed in your pages, it might have been more satisfactory perhaps had he passed in review the conclusions arrived at by others who have preceded him.

Sir John Herschel (see "Physical Geography," § 132, 1862, and "Familiar Lectures," Lecture I.), assumed in a general manner that "if continents are lightened they will rise; if the bed of the sea receives additional weight it will sink." It is to be regretted that the facts advanced as evidence by so great an authority did not prove sufficiently conclusive to claim general acceptance. Mr. T. F. Jamieson, F.G.S., in 1865 (*Quarterly Journal of the Geological Society*, vol. xxi. page 178), considered that the enormous weight of snow accumulated during the glacial period "may have had something to do with the depression of the land which then occurred, and that the melting of the ice at its termination would account for the rising of the land."

Under the advocacy of Prof. James Hall ("Palæontology of New York," vol. iii., 1859), the subject has received much consideration in America; this has been so great that Capt. C. E. Dutton, of the United States Geological Survey, was enabled to say that "few geologists now question that great masses of sedimentary matter displace the earth beneath them and subside" (NATURE, vol. xix. p. 251).

The principle that accumulation of material causes subsidence and that denudation results in elevation of the crust of the earth has been advocated by myself on numerous occasions during the last eighteen years, being considered equally applicable to rocks of every age during the whole series; in England from the Cambrian rocks of Shropshire to those now in process of deposition in the seas which surround our coasts. The idea originated to me from observations in the Longmynd and of the Upper Silurians of Shropshire and North Wales during 1864. Its universal application and the physical effects dependent on the phenomena formed the special subjects of two addresses as president of the Liverpool Geological Society in 1871 and 1872. The conclusions were deemed by NATURE (vol. vi. p. 379) of such importance that you considered my "interpretation of the facts deserved further consideration." Abstracts of these essays also appeared in the *Geological Magazine*, vol. ix. p. 119, and vol. x. p. 202. The views entertained have been subsequently advocated by me in the *Proceedings of the Liverpool Geological Society*, the *Geological Magazine*, and the *Reports of the British Association*, the last time being during the meeting of the British Association at Southampton (Report, 1882, p. 540), which paper has appeared in full in the *Geological Magazine* for July and August, 1883.

The only author who has considered this subject and to whom Mr. Gardner refers, is the Rev. O. Fisher, F.G.S., whom he deservedly praises for his masterly work, "The Physics of the Earth's Crust," 1881.

In spite of much adverse criticism I have been content to wait all these years, feeling convinced that after commendation similar to that accorded by you (by no means a singular occurrence), the subject of oscillation, as the result of changes in the distribution of sediments, would eventually be taken into consideration; for a frequent remark has been that "there appears to be something in it"; and no geological fact is more persistently referred to than that the formation of sedimentary strata of every age "has occurred during a period of subsidence."

Birkenhead, August 22

CHARLES RICKETTS

"Decentralisation in Science"

I FULLY agree with the remarks on this important subject made in your leading article of last week; and the necessity for local scientific societies being in some way placed in direct communication with each other and with the central metropolitan societies has long been present in my mind. It is perhaps as yet premature to broach any definite scheme for effecting this object, which, as the writer of the article points out, would be surrounded by very great practical difficulties. The whole subject might very well be discussed by the Conference of Delegates about to attend the meeting of the British Association at Southport.

There are numerous scientific societies and field-clubs throughout the country whose work is being frittered away in useless directions solely from the want of proper scientific guidance. As a preliminary step towards this most desirable economising of individual energy it appears to me that centralisation in the various counties is the first essential. This has been well enforced in the Preliminary Report of the "Local Scientific Societies" Committee of the British Association, published in NATURE a short time ago by Mr. Francis Galton, the Chairman of the Committee.

It is most satisfactory to know that the British Association has taken the matter in hand, as this body is of all others the most competent to deal with the subject, if for no other reason because the Association is the only scientific society that holds its meetings in various provincial centres. Among the difficulties that would have to be met in any scheme of county affiliation not the least formidable is the purely local feeling existing in many small societies, which leads their officers and members to reject all overtures from larger and more influential bodies in the mistaken belief that cooperation would entail a loss of individuality. A good illustration of this kind of difficulty has quite recently come under my notice in attempting to bring about some kind of amalgamation between the local societies of the county of Essex.

Till such narrow views of the functions of a local society are successfully combated no great advance towards centralisation can be made.

R. MELDOLA

21, John Street, Bedford Row, W.C., August 27

The Earthquake in Ischia

IN 1878, when touring in the Himalayas, we spent the last two Sundays in August at Kyelang, in the Lahoul Valley. On each of these days I felt a sharp shock of earthquake about 4 p.m.

On both occasions I was sitting in a room on the upper floor of the German missionaries' house. A broad wooden verandah runs round the front and sides of the lower floor of this building.

I was about to rise and leave the room, when I heard a loud rumbling noise; my first idea was that the children of the house were amusing themselves with dragging each other in a small wooden waggon up and down this verandah as they were in the habit of doing, but the sound was much louder, as loud as that of a railway train when near the spectator. A second later I felt a violent oscillation, and a padlocked door, opposite the door of exit, shook violently backwards and forwards several times. A week later another earthquake occurred almost at the same hour, and under the same conditions.

Three years later, in 1881, we again passed through the same part of the Himalayas on our return from the Spiti Valley, which we had reached by way of Kunowar.

This time no earthquake took place during our stay in Lahoul; we crossed the Rotang Pass, and went to stay in the Kulu Valley with our friend Col. S—, Deputy Conservator of Forests at Mañali, about sixteen miles on the southern side of that mountain. Col. S—'s house is raised high above the river on the right bank of the Beas; it is placed in the midst of a Deodar forest, and built of wooden logs placed horizontally, and alternating with courses of large stones laid one upon the other, but not mortared together. A wooden verandah runs all round the building, and forms a balcony to the rooms on the upper floor. I imagine it is its mode of construction which enabled this house to resist the severe test to which it was subjected on this occasion.

On October 1, about 1 p.m., we were sitting, a party of three persons, in a temporary verandah resting on the bare earth, and floored with matting, which our host had erected to supplement the permanent one where our native tailor was seated at work.