tree-lifter and of its advantages in transplanting large trees. The principle of transplanting trees with a large ball of earth attached to the roots is, however, so well known, and tree-lifters of similar construction to that here described are now so generally used, that we follow the example of the author in his brevity, and simply dismiss this part of the subject which he calls the "practical part of transplanting," and turn to Part 2, which is devoted to the "theory of transplanting, or physiology of trees in reference to transplanting." It is apparently for the purpose of recapitulating and condensing the views of various authors of acknowledged reputation in the several branches of vegetable physiology, and of expressing his own opinions thereon, that the author has put this book together, all that is really directly connected with the title being contained, as we have before said, in the first eleven pages. The author, however, at the beginning of Part 2, candidly says: "Before entering on physiology, I would say one word to defend myself from the charge of egotism and plagiarism. When I mention Sir Humphry Davy, I may say that immortal names are among those who have written on the physiology of trees; yet so much doubt and difference prevail among the authors on the subject, that one cannot adopt a single opinion without opposing many, held by minds, perhaps, as clear and comprehensive as Sir Humphry's. It is, then, to save the reader's time if I lay down as certain what men have doubted or controverted, or if I use the words, 'I think this,' or 'I think that,' in stating other people's opinion."

Space will not permit us to follow the author through all his reasonings; it will suffice to mention that no less than forty-one pages are given to the consideration of the subject of the course of the sap, in which the author tilts at several well-known English and Continental botanists whose theories are adverse to his own. Lindley is the most recent authority quoted in the original edition, and the opinions of later writers have not been embodied in subsequent issues. What we have already said will show the character of the book. The style of writing may be gathered from the first paragraph in the chapter on the course of the sap, where the author says: "However much we may dispute on *how* the sap gets into the tree, we shall all agree that it *does* get in somehow; and but for Dr. Lindley, I believe we should all agree on the *course* which it then takes." Further on, in connection with the theories of the contraction and expansion of the wood, caused by alternating heat and cold, and of the pumping action from the motion caused by wind, Col. Greenwood writes: "Look into the hot-house and the hot-bed. In these neither of these causes exists. Not a breath of wind enters; nor is any alternation of heat and cold allowed. Yet in these the ascent of the sap is freest. And if we look out of doors, I should say that the sap would be a slow traveller if its ascent depended on wind and cold. Here, then, I cannot back the favourite, and have a sort of blind leaning for *Turgescence*, or Swelling, a dark horse, certainly, and I am all in the dark about him myself;" and the author is in a similar state of gloom upon other points besides.

Practical Portrait Photography. By Wm. Heighway. (London: Piper and Carter, 1876.)

THE author of this little book of 152 pages has endeavoured to "provide simple and intelligible rules of working," as he states in his introduction, so that those who take up photography as an art should be helped over a number of difficulties certain to occur, and not always provided against in more ambitious works. The lessons chiefly enforced are cleanliness and accuracy in preparing the requisite solutions, and method in carrying out the rest of the processes; though these lessons may seem trite to the regular student of science, they are no doubt much needed amongst photographers who are not at the same time practised chemists.

The necessary instructions are well and carefully given, and the author has omitted no point of importance, taking the reader *seriatim* through every detail, from cleaning the glass plate to finishing the paper print.

We notice some errors in chemistry, where the author has given reasons for some of the processes, which we hope will be corrected in a future edition. R. J. F.

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.]

On the Discovery of Palæolithic Implements of Inter-glacial Age

THE opinion that palæolithic man was a post-glacial being has been steadily losing ground among certain geologists whose studies render their opinions of considerable weight. Mr. Pengelly and Prof. Ramsay have stated their conviction that the old stone folk may have witnessed the commencement of glacial conditions, and have been driven south by the increasing severity of the climate. Prof. Dawkins has expressed his belief that while our rude ancestors hunted the elephant, glaciers still lingered in our mountain-valleys. Mr. Tiddeman goes further, and ascribes to them an inter-glacial age, and, as it seems to me, proved his point by the discovery of implements and a human bone beneath glacial-clay in the Settle Cave. Mr. James Geikie boldly advanced the opinion that *all* our palæolithic implements are of inter-glacial age, and an intimate knowledge of the glacial beds and gravels of the central and eastern counties led me independently to a similar conclusion. In making this last statement I particularly desire it to be understood that no claim is preferred to the theory as my own, for while I was almost fearful of my temerity in even thinking such things, my friend Mr. Geikie had brought his great stores of knowledge to bear upon the point, and has made it peculiarly his own. Nevertheless the fact that two geologists working independently in different districts should arrive at similar conclusions is no mean argument in our favour. The evidence upon which my convictions are based is given in my "Geology of the Fenland," and in my "Manufacture of Gun-flints," &c., to be shortly issued by the Geological Survey, and in the forthcoming edition of the "Great Ice Age."

Mr. Geikie has proved, and the work of Mr. Tiddeman, myself, and others, has confirmed the observations that the glacial epoch, instead of being an uninterrupted period of cold, was one of fluctuating climate, there being known at least four ice ages with intervening cold, mild, and warm epochs. The greatest severity of cold took place towards the early part of the Great Ice Age, and the great chalky boulder-clay which extends nearly to the Thames was then formed; no subsequent ice-sheet having left its traces further south than Lincolnshire. Travelling northwards from East Anglia we find this boulder-clay running under the purple boulder-clay, and this again overlaid still further north by a yet newer glacial bed. These are well-known facts accepted by all geologists, but as the old chalky boulder-clay has unfortunately been named the "upper" boulder clay, it has been supposed to mark the close of the glacial epoch, whereas it is only "upper" so far as East Anglia is concerned, and merely marks the last glaciation of that area, the more northern districts having been more than once glaciated since then. The East Anglian "upper" boulder-clay is probably as old as the Lancashire "lower" boulder-clay.

In consequence of this confusion of terms, the beds which overlie the chalky boulder-clay have been confidently relegated to post-glacial times, whereas all that can be determined by this superposition is that they are "post-boulder-clay." From valley and other gravels occupying such positions, great numbers of palæolithic implements have been obtained, especially from the basins of the rivers Lark and Little Ouse. Wherever bones are found in these gravels they belong to what Prof. Dawkins calls the pleistocene, and not to the pre-historic fauna; and this mammalian fauna is continued into the estuarine gravels of the Fenland, which contain extinct shells, such as *Cyrena fluminalis*, a shell which has often been found in gravels yielding palæo-

ithic, but never in beds yielding neolithic implements. As we travel northwards, say to Hesse, these *Cyrena*-bearing gravels are seen to underlie boulder-clay newer than the upper one of East Anglia; and in cave deposits still further north, beds yielding the same pleistocene mammals appear in similar situations, notably in the Victoria Cave at Settle. Now bearing in mind that in East Anglia, &c., where these beds are best developed, there has been no subsequent glaciation to sweep them bodily away, or show their age relatively to the glacial epoch, it seems to be a legitimate deduction that they are of inter-glacial age, when we find that to the north, wherever fragments have escaped destruction, they are overlaid by glacial beds. This is the conclusion to which Mr. Tiddeman arrived from a study of the Victoria Cave deposits and an intimate acquaintance with the glacial phenomena of the district, and my own work in the Fenland and East Anglia led me to a similar result. Mr. J. Geikie has, however, done more than any one to develop this idea, and was the first to propound it. He claims, then, that no palæolithic implement is of post-glacial date; and when we reflect upon the vast changes which have occurred since palæolithic times in the physical configuration of the country, in the mammalian fauna—changes which are even impressed upon the stable molluscs—the theory appears more than probable, and the difficulties which surround the post-glacial hypothesis steadily increase.

Palæolithic implements, however, are not all of one age (it would be strange indeed if they were), though it is very difficult to discriminate their relative antiquity. I have been much struck with the aged aspect of certain of the ruder tools as compared with some of the better finished ones and with the stones in the gravel in which they occur, and this gave me hopes of tracing such tools to an older deposit, a desire which has been abundantly fulfilled, and remarkably confirms my friend Mr. J. Geikie's bold surmise.

Here and there along some of the minor valley sides around Brandon are preserved patches of brick-earth, which are valuable as affording the only workable clay in the district. Whenever these beds are well exposed they are seen to underlie the chalky boulder-clay. Of this there cannot be the slightest doubt, for the glacial bed is typically developed and not in the slightest degree re-constructed. In these beds I have been so fortunate as to find palæolithic implements in two places; and in one of them quantities of broken bones and a few fresh-water shells. The implements are of the oval type, boldly chipped, but without any of the finer work which distinguishes the better made palæolithic implements. Although it would be rash to lay too great a stress upon the characters of these implements, it is, nevertheless, worthy of remark that they do belong to the crudest type. Equally rough specimens are found in the gravels above the boulder-clay and even among neolithic finds, still these very antique implements certainly do seem to belong to an earlier stage of civilisation if we regard them as examples of the best workmanship of their makers.

The interest attaching to these specimens lies in their exceeding antiquity—an antiquity greater than can be ascribed with certainty to any others. I have shown this boulder-clay to belong to the earlier part of the ice age, and beneath it these tools were found. I am not yet certain whether they belong to the so-called "middle glacial" series of Mr. Searles Wood, jun., to a somewhat newer date, or to a preceding period, for the beds lie directly upon the chalk. This much, however, is certain, that they conclusively prove man to have been a denizen of our land before the culmination of the glacial epoch.

Another point is deserving of notice. The tools are decidedly of palæolithic aspect—the difference between them and those which overlie the boulder-clay is slight in comparison with the differences between the latter and neolithic implements. Who shall say how long East Anglia was swathed in ice? Yet that interval was not long enough for man to advance greatly in his manufactures, and it appears to me we have here another argument in favour of the glacial age of all palæolithic tools and against the theory which relegates them to after the close of the ice age. It seems to bring the brick-earth tools and the gravel implements closer together, and withdraw them still further from the newer stone age.

As soon as the bones are examined and the survey of the brick-earths completed, I hope to write more fully upon this question, and here only indite a few preliminary notes in the hope that they may prove interesting to brother geologists.

SYDNEY B. J. SKERTCHLY

The Inverse Rotation of the Radiometer an Effect of Electricity

IN my communication published in NATURE, vol. xiv. p. 288, I endeavoured to show that the direct rotation of the radiometer was an effect of electricity. Before attempting to explain the inverse rotation it will be necessary to expound briefly some new facts which my electroscopic researches have led me to establish.

In order to ascertain the electric state of their inner surface, I exposed to solar radiation glass receivers such as are used for the air-pump. By means of the proof plane and electroscope I found that this surface was electrified negatively, and even to a greater degree than the exterior. This excess of energy I attribute to the numerous reflexions from the interior. If, however, we hold one of these electrified receivers near the Bohnenberger electroscope, taking care that it does not come in contact with it, the electroscope at once indicates the presence of *positive* electricity. As both the outer and inner surfaces are negatively electrified, this phenomenon must be attributed to the electricity developed in the interior of the glass itself by its molecular polarisation and feeble conductivity. The following experiment confirms this explanation. If we remove from the exterior by means of the proof-plane a portion of the negative electricity and then approach, as before, the globe to the electroscope, a remarkable increase of positive electricity is at once shown. The same results are observed in the radiometer.

I next examined the electric state of the exterior of the radiometer globe when placed in partial obscurity and moistened with ether. There are no signs whatever of electricity as long as the inverse rotation continues, but as soon as the direct rotation commences—on account of the obscure radiations given forth by the surrounding bodies—positive electricity manifests itself and rapidly increases. While in this state I exposed the radiometer to solar radiation, and I found that this positive electricity remains quite a long time, and that, notwithstanding the positive charge on the exterior, the direct rotation continues with its usual rapidity.

The fact last-mentioned enabled me to determine, by experiment, the electric state of the inner surface of the radiometer globe. Only two suppositions can be made in regard to it: either the electric state of the inner surface is dependent by means of molecular polarisation upon the electric state of the exterior, or it is independent. In the first supposition the interior face is electrified positively when the exterior is electrified negatively, and *vice versa*. The second supposition may be divided into three hypotheses, for we can admit that the interior is constantly, under the same circumstances, either neutral, or negative, or positive. Hence we have in all four hypotheses, *a priori*, viz. :—

1. Inner surface is dependent upon electric state of exterior.
2. Inner surface is independent and neutral.
3. Inner surface is independent and negative.
4. Inner surface is independent and positive.

Now of these four hypotheses the fourth alone is verified by experiment. This I have established as follows :—

In one series of experiments I charged the exterior of the radiometer with positive electricity by exposing it to solar radiation.

In a second series I charged the same surface with positive electricity by exposing it to solar radiation after moistening it with ether.

Each experiment comprised two operations. I touched a certain number of times the exterior of the glass globe with the proof-plane and I carefully observed the electroscopic signs of the Bohnenberger electroscope when brought in contact with the proof-plane; then I approached to the electrometer the glass globe which had been partially discharged by the preceding experiment, and I again observed the signs given by the electroscope. In the case that one of the first two hypotheses expresses the real state of the inner surface of the radiometer under the influence of radiation, on approaching the glass globe we should have, in both series of experiments, electroscopic signs of equal intensity for equal electric charges of the exterior surface, manifested by the equality of those of the proof plane. Now this does not take place. In my experiments on the approach of the globe the electroscopic signs in the second series surpass in intensity those observed in the first series. These results agree perfectly with the fourth hypothesis, but are in open discord with the third. Any one can easily see this, with a little attention, by considering the layers of electricity produced in the interior of the glass walls by molecular polarisation. The fourth