of animals, to investigate life-histories of various insects, parasites, &c., and generally to advise on subjects relating to economic biology, agricultural chemistry, and bacteriology.

The articles in the journal are mainly summaries of work done elsewhere rather than accounts of original work; perhaps this was only to be expected from an almost new laboratory. Mr. Collinge deals with the use of lime, with special reference to its influence on plant diseases like potato-scab and finger-and-toe fungus; he has also collected a good deal of scattered work on the woolly aphis. Mr. Barlow deals on similar lines with the effect on plants of copper salts used as fungicides. The summaries themselves call for no special comment, but the journal as a whole is well got up. We shall be interested to see how Sir Richard Cooper's experiment works—whether the laboratory can maintain the detached position essential for the publication of scientific work, or whether, as has happened elsewhere, it becomes merged in the purely commercial side.

Cambridge County Geographies: Somerset. By Francis A. Knight, assisted by Louie M. (Knight) Dutton. Pp. xi+192. (Cambridge: University Press, 1909.) Price 1s. 6d.

THE characteristics of the series to which this volume belongs were enumerated in our issue for May 13 (vol. lxxx., p. 305), and much of what was written on that occasion applies to the present book. The authors' interpretation of the scope of geography is wide enough to include a history of the county, its antiquities—ecclesiastical, military, and domestic—its administration and roll of honour. Like previous volumes in the series it is well illustrated, brightly written, and generally attractive.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Difference between Longitudinal and Transversal Zeeman Effects in Helium Lines.

IT is easily shown on the theory of electrons that the amount of separation of the outer components of a transversal Zeeman triplet must be slightly different from that in a longitudinal doublet. Some time ago I showed that the transversal separation in weak magnetic fields does not strictly follow the linear relation with the magnetising force, but, owing to an indirect method of measurement, the exact amount of the separation could not be measured with accuracy. By measuring the longitudinal effect of helium lines with an echelon spectroscope of thirty-five plates, each of 1 cm. thickness, made by Hilger, I found that doublets can be distinctly separated in a field of 180 gauss, when the right- and left-handed circularly polarised light is linearly polarised in mutually perpendicular directions, by interposing Fresnel's rhomb in the course of the beam. Taking a number of points at intervals of about 300 gauss from $H=0$ to $H=2000$, and ten to thirteen points from $H=2000$ to $H=14,000$, I found that for the three lines $\lambda\lambda=6678$, 5876, 5016, the relation between the amount of separation $\delta\lambda$ and the strength of the field H is exactly linear, so that $\delta\lambda/H=$constant also in weak fields. In these experiments it was necessary to gauge the strength of the field accurately for each point before and after each micrometric measurement by means of a small coil. The values of e/m were found to be for

$\lambda=6678$	$e/m=1\cdot86\times10^7$
$=5876\ (D_3)$	$=1\cdot68\times10^7$
$=5016$	$=1\cdot80\times10^7$

The separation of the satellite of D_3 is complex, but there is one component which gives the same value of e/m as D_3.

With the transversal effect the ratio $\delta\lambda/H$ is not constant in weak fields. With D_3, the curve representing the relation between H and $\delta\lambda$ is such that it increases very slowly to $H=800$, then rapidly to an inflexion point in $H=1700$, makes a bend, and from $H=2000$ follows an accurately straight course up to $H=14,000$, which is the strongest field used in the present experiment. In the latter part of the curve $d(\delta\lambda)/dH=$constant, which is smaller for the transversal than for the longitudinal effect, so that the curves representing these effects cross each other in $H=1200$ and $H=10,900$. The initial part of the curve for the transversal effect shows a striking resemblance to that of magnetisation in ferromagnetic substances. The satellite accompanying D_3 shows remarkably complex separation, as shown by Lohmann, but there are two components which take a similar course to the principal line D_3. The lines are already separated before reaching the inflexion point above mentioned, so that the method which I used in my former experiments, is confined only to weak fields. With the line 6678, the initial course of the curve for transversal effect is similar to that of D_3, but the inflexion point is reached in a higher field $H=2700$, and the curve becomes a straight line from $H=3600$ upwards. The curve for longitudinal effect lies entirely above that for the transversal, and $d(\delta\lambda)/dH$ in strong fields is greater for the former than for the latter.

The usual calculation of e/m is made on the supposition that $\delta\lambda/H=$constant, which is strictly obeyed in the longitudinal, but not in the transversal, effect; the discrepancy in the value of e/m calculated from longitudinal and transversal effects is at once explained. The initial course of the curve can be accounted for by Voigt's theory, but the appearance of the inflexion point before attaining the straight course presents some difficulty. The resemblance of the curve of transversal effect to that of magnetisation seems to have an important bearing on the exposition of the theory, which would explain these characteristic features. The extension of these experiments to stronger fields and with different elements is being undertaken. H. NAGAOKA.

Physical Institute, University of Tokyo, July 16.

Natural Selection and Plant Evolution.

THE letter from Mr. James B. Johnston in NATURE of August 5 touches on many important points, which cannot be fully dealt with in a letter of reasonable length.

In his opening sentence the writer, speaking of chapter xii. in "Darwin and Modern Science," says that "there, perhaps for the first time, the evidence of the fossils with regard to the influence of natural selection has been fairly tackled"; I may point out that the chapter cited really relates mainly to evolution, and especially phylogeny; only the last section refers to natural selection, a subject on which, from the nature of the case, the fossil record can throw comparatively little light.

I cannot think that, on the main question, there can be any very fundamental difference between the writer's views and my own, for he says:—"In the case of the Tertiary mammals the action of natural selection can be very clearly demonstrated in numberless cases." Mr. Johnston cannot seriously mean that he accepts natural selection for animals and rejects it for plants. The question is simply one of evidence. As I have myself pointed out, the direct evidence for the derivation of one species from another is at present less satisfactory in the plant than in the animal record ("Darwin and Modern Science," p. 204); on this point we may hope for new light from further research, though, as regards the efficacy of natural selection (an essentially different question), I doubt if palæontological evidence will ever be really decisive.

My point in speaking of the evolution of the pollen-tube and seed was to show that such characters are *adaptive*, a view to which Mr. Johnston is not likely to object. In the present position of biological science evidence of adaptation is commonly accepted as presumptive evidence of the action of natural selection.

The question whether a belief in the efficacy of natural selection can be regarded as "barring out all design from the world in which we live" is not one that can be dis-