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 1*s.* 6*d.*

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 λλ=6678, 5876, 5016, the relation between the amount of separation δλ and the strength of the field H is exactly linear, so that δλ/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

λ=6678	$e/m=1{\cdot}86\times10^7$
=5876 (D_3)	$=1{\cdot}68\times10^7$
=5016	$=1{\cdot}80\times10^7$

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 δλ/H is not constant in weak fields. With D_3, the curve representing the relation between H and δλ 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 δλ/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-

cussed here. Mr. Johnston will find this subject admirably treated, from a theologian's point of view, in Mr. Waggett's contribution (chapter xxiv.) to "Darwin and Modern Science."

The question of the antiquity of land-plants is of great interest. Assuming, for the sake of argument, that the highly organised lycopods and fern-like plants described by Prof. Potonié as Upper Silurian ("Die Silur und die Culmflora des Harzes Geb.," *Abhandl. d. k. Preuss. Geolog. Landesanstalt*, Heft 36, 1901) were really of that age, it would appear certain that land-plants must then have already passed through a very long course of evolution. No one is likely, in these days, to suppose that Bothrodendron and Sphenopteridium were specially created. The doctrine of mutation (of which Mr. Johnston appears to be an adherent) does not materially help in hurrying up the process of evolution, for, as Prof. de Vries himself says, "Mutations do not necessarily produce greater changes than fluctuations" ("Darwin and Modern Science," p. 73).

The problem confronts us, and its solution must simply await further evidence. D. H. SCOTT.

Oakley, Hants.

The Perseids of 1909.

THE weather, fortunately, took a very favourable turn on August 3, and observations have been obtained nearly every night since, though moonlight has rather seriously interfered with the work and obliterated many small meteors. Up to August 9, and including that date, the display of Perseids was decidedly poor, and quite disappointed expectation. On August 7 and 9, particularly, there appeared to be very few meteors, but the moon was shining rather strongly in the east.

On August 8 there were several brilliant Perseids observed. At 10 p.m. a fine, long-pathed meteor shot from Camelopardalus across κ Draconis towards ζ Ursæ Majoris. At 10.8 p.m. another Perseid, equal in brightness to Jupiter, passed from χ to between β and η Draconis. At 10.34 p.m. a third shot exactly from α Cygni to close to β Cygni. These meteors left streaks, and, in combination with a few other paths recorded on the same night, indicated a radiant at 41°+57°.

The shower will probably arrive at its most abundant phase on August 11 or 12 this year, so that the meteors already reported are merely the vanguard of the approaching main group. W. F. DENNING.

Bristol, August 10.

The Ringing of House-bells without Apparent Cause.

KINDLY allow me space for a few remarks upon Sir Oliver Lodge's theory, put forth in NATURE of July 22 (p. 98), to the effect that "the bells get charged with electricity (atmospheric), and are attracted to a neighbouring wall or pipe, and then released suddenly by a spark." Now, while it is conceivable that a bell might be rung under certain conditions in this manner, during the progress of a thunderstorm or display of sheet-lightning, and granting that ordinary non-electric bells have been rung and wires fused when a house has been struck by the electric current during such storms, still, this theory is inadequate to explain those cases of mysterious bell-ringing on record, and for one reason, among others, that these ringings, often violent and prolonged, have been extended over a term of several weeks or months, and have constantly taken place when no storms or strong electrical conditions were apparent, and when every effort was being made to ascertain the cause.

I speak from personal experience of a case which occurred in my house when resident in the south. For a period of two months there were constant ringings—often violent, the bell lashing to and fro—of the indoor bells, without apparent cause. In the case of one bell the wires were cut, but still it rang. The utmost endeavours were made to solve the mystery, but it defied all our efforts. There were no rats, the house having been made rat-proof, nor did we see one rodent during our stay. The wires were carefully traced and examined. Pendulums were affixed to all the bells to detect slight motion, and they were strongly illuminated by a powerful light and a watch kept, sometimes all through the night. The chief offender among the bells was one communicating with a private room. The wire from this ran, high up near the ceiling, upon the varnished paper, except where it passed through a wall, which it did through a half-inch pipe. It was impossible for a rat or mouse to touch it all along its course. This bell rang repeatedly from early morn to late at night. The room was thoroughly searched and secured—the shutters put up and barred and the door locked. Still the bell rang, and defied all our efforts to elucidate the mystery. On one occasion, when the whole household was together in another room, some little distance away, one of them said, "I wonder if it will ring to-night?" The words were scarcely spoken before the bell rang out, first faintly, then so violently that the bell lashed from side to side. All ran out and saw it swaying. I can state that during the whole period we had no thunderstorm, it being winter, and the ringings were so frequent that it would have needed scores of storms and abnormal electrical conditions to produce them, even if these had been the cause. This theory is ingenious, but one doubts whether Sir Oliver advances it seriously. Whatever is the cause of these mysterious ringings, it is patent to anyone having had experience of them, or knowing the cases on record, that it is not electricity, atmospheric or other. C. L. TWEEDALE.

Weston Vicarage, Otley, Yorks, August 6.

Variation in Relative Intensity of Helium Lines.

IT has long been an open question whether the spectra of gases were subject to any general law of intensity variation such as the Wien-Paschen displacement law for incandescent solids. Reasoning from Kirchhoff's law, it is frequently assumed that the Wien-Paschen function must be an envelope to the radiation spectrum of any gas, at least for certain selected lines. Pflüger's work with the mercury lamp (*Ann. Ph.*, July, 1908) indicated some such variation in relative intensity, but the energy (temperature) effect was obscured by the effect of varying vapour density. The latter is known to be large, so that the existence of the former was not proven.

We have recently taken up this question, using a pair of helium lamps containing very pure gas at 5 mm. pressure, and found *no variation* whatever when the intensity was varied by a factor of eighteen. The method was to hold one of this pair of lamps constant at a moderate current (5 milliamperes per mm.^{2}), while the intensity of the other was varied from 0·15 to 2·70 times this value. Spectrophotometric settings were made on the brightest red line (λ 668) and the bright blue line λ 447. The uncertainty in the ratios of high/low intensities was less than 2 per cent. in both red and blue; the observed difference in these ratios between red and blue was less than 1 per cent. The red and blue lines then increase in the same proportion over a wide range of intensity.

According to the Wien-Paschen function, an incandescent body should, for an increase in total radiation of 18 fold, increase in temperature 2·06 fold. If this increase is from 1500° to 3090°, then λ 668 would increase in that range 1721 fold, and λ 447 would increase 66,850 fold in intensity, so that the blue increases 38·8 times as much as the red, whereas for helium we found no difference so great as 1 per cent. P. G. NUTTING.
ORIN TUGMAN.

Department of Commerce and Labour, Bureau of Standards, Washington.

Kohlrausch's "Physical Measurements."

I SHOULD like, through the medium of NATURE, to direct attention to an erratum in Kohlrausch's "Physical Measurements" (third English edition, Churchill, 1894), p. 434, table 8. The value for $k(s=2{\cdot}0)$ is given as 0·458, its true value being 0·457(1). Other editions, English or German, not being at hand, I do not know whether the mistake is repeated or not. As this occurs in a very useful table in a universally used reference work, it should be of interest to a number of your readers.

E. W. NELSON.

The Laboratory, Citadel Hill, Plymouth, August 2.