merely a collection of these spheres in a very high state of condensation. To illustrate this tendency to condense, the author compares it with the tendency of the sun and stars to cool and contract, and eventually to form bodies like the moon. This theory of the constitution of matter, we are told, explains all the natural phenomena of light, heat, electricity and magnetism, without a single contradiction.

Having tried to upset the existing theories, and having told us that this new theory will explain practically everything, the author, to our surprise, fails completely to put forward convincing proofs in support of its application to electricity and magnetism. A good example of the class of explanation with which the pamphlet abounds is to be found on page 48, where the loss of electricity from conductors in damp weather is alluded to. A positively charged body is supposed to be surrounded by layers of negative ether spheres—that is, by spheres having a larger radius than the mean ether sphere. These negative ether spheres are in a high state of tension, and when a water molecule comes into the space which they occupy it relieves this tension, and so partly discharges the conductor. If we accept this explanation, there is absolutely nothing to prevent us supposing that small particles could discharge a conductor without touching it, or without being connected to it by any other material except the ether, as the author supposes that the layers of negative ether spheres, above alluded to, extend to finite distances from the conductor.

Experience, however, will not allow us to accept such an explanation at all, for it has been perfectly well established that the vapour rising from an electrified surface carries with it no charge. In connection with the magnetism of the earth we find, on page 90, an interesting piece of information. We are there told that it is only those heavenly bodies which rotate that have polarity, and that, *consequently, the moon is non-magnetic!*

It is consoling to learn that the author has suffered hitherto so much from hostile critics that he can no longer be stung by the suggestion that his philosophy is "blank Unsinn." J. S. T.

Farm and Garden Insects. By Prof. Wm. Somerville. Pp. viii + 127. (London: Macmillan and Co., Ltd., 1897.)

A USEFUL little *text-book* for beginners, and an excellent *reference book* for practical farmers. The three parts into which the book is divided are judiciously arranged. The first gives in a clear and distinct manner the rudiments of entomology, and forms, therefore, a useful introduction to the second part, which describes some of the most common insect pests whose ravages cause so much loss to the farmer and gardener. This loss may be very much modified if the simple precautions and remedies contained in the book are adopted. The appendix in a few pages gives most useful information about mites, ticks, &c.; not true insects certainly, but which, by attacking our domestic animals, and even man himself, cause an immense amount of irritation, inflammation, and consequent loss. Farmers, gardeners, and all interested in rural economy will do well to carefully study its pages.

Geology of North-east Durham. By D. Woolacott, B.Sc. Pp. vi + 84. (Sunderland: Hills and Co., 1897.)

THIS is an orderly account of the geological characteristics and history of North-east Durham. It is written in language easily understood by readers unacquainted with the elements of geological science, and will, therefore, interest a popular public as well as the student of British geology. The diagrams are very coarse; but this is doubtless due to the fact that they were prepared for publication in a weekly newspaper, in which the articles now reprinted originally appeared.

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.*]

Rate of Racial Change that accompanies Different Degrees of Severity in Selection.

IT is well known, in a general way, that better results are obtained by breeding from very select specimens than from the less select; but the statement deserves to be expressed with greater precision. I will do so here, on the lines laid down in "Natural Inheritance" (Macmillan, 1889), using the constants there determined for the stature of British men, including among them the coefficient by which female stature may be corrected to its equivalent male value, and thereby eliminating all trouble due to sexual differences.

On this basis, it will be shown, by way of illustrating a general problem, how much the stature of a breed of British men would be raised in each successive generation, under different specified degrees of severity in selection; also the utmost limits of stature to which they could be raised in the several cases, no change of type being supposed to occur in the interim.

Degrees of severity in selection admit of being defined by the method of *centiles* (or *percentiles*) fully described in the above book. No ambiguity need arise in interpreting such a statement, as that a man occupies the ninetieth centesimal grade in stature among a population whose mean stature is 68·5 inches, and whose individual statures are normally distributed about that mean with a quartile of 1·5 inches. Referring to Table 8 in the book, it is seen that the normal deviation at 90° in a series whose quartile is 1, is 1·90; therefore, in the above case, its value is 1·9 × 1·7 inches = 3·23 inches. The mean stature of the population is 68·50 inches, which has to be added to this, making a total of 71·73 inches. Consequently when it is said that those persons are selected for parents who occupy the grade of 90° in their respective series, the degree of severity in the selection has been strictly defined. Similarly in respect to any other grade, such as 80° or 70°. This method of defining severity of selection is applicable to every measurable character, and to every form of distribution, skew or other, revealed by observation.

The principle has been fully explained in the above book, by which successive generations of the same population are able to maintain the same statistical peculiarities notwithstanding the "scatter" of families. It was shown that the sons of parents of similar statures form a co-fraternity, whose mean is more mediocre than the parental statures. In other words, the mean of the co-fraternity *regresses* towards the mean of the race, the coefficient of regression in stature being 2/3. Thus the children of parents of grade 90° in stature, deviate on the average no more than 2/3 × 3·23 inches, or 2·15 inches, above the mean of the race. So much for the first generation of the selected parents.

In the second and subsequent generations, the "scatter" of the co-fraternities has to be considered. The quartile of every one of them was shown in the book to be 1·5 inches, consequently the individuals who occupy the grade 90° in a co-fraternity, are 1·5 × 1·90, or 2·85 inches taller than the mean of the co-fraternity, which itself is 2·15 inches above the mean of this race, making a total of 5·00 inches. The mean of their offspring, that is of the individuals forming the second generation of the selected series, is 2/3 of 5·00 inches, or 3·33 above the mean of the rest of the race.

These results are easily generalised and thrown into a formula, as follows: w = coefficient of regression; t = tabular deviation at the specified grade; q = quartile of race; q' = quartile of co-fraternity; $\alpha = tq$; $\beta = tq'$. Then the mean deviation of the pedigree stock from the mean of the race, in each successive generation, is:—

1st generation, $w\alpha$.
2nd ,, $w(w\alpha + \beta)$.
nth ,, $w^n\alpha + \frac{w - w^n}{1 - w}\beta$.

When n is large, w^n disappears and the limiting value becomes $\frac{w}{1 - w}\beta$. If $w = \frac{2}{3}$ as above, the limiting value is equal to 2β.

On these bases the following table is calculated :—

Excess of the mean stature of the pedigree breed in each successive generation, above that of the rest of the population, when both parents occupy the undermentioned grades in their own generation of the pedigree-breed.

Generation.	99°	95°	90°	80°	70°
	inches.	inches.	inches.	inches.	inches.
1	3·9	2·8	2·2	1·4	0·9
2	6·1	4·3	3·4	2·2	1·4
3	7·5	5·3	4·1	2·7	1·7
4	8·4	6·0	4·7	3·1	1·9
5	9·1	6·4	5·0	3·3	2·1
&c.	&c.	&c.	&c.	&c.	&c.
Limiting values ...	10·4	7·3	5·7	3·8	2·3

The importance to the breeder, of using highly selected parents, is *measured* by these tables, and shown to be very great. Thus one generation of the 99° selection is seen to be more effective than two generations of the 90° selection, and to have about equal effects with those of an 80° selection carried on to perpetuity. Two generations of the 99° selection are more effective than four of the 95°, and than a perpetuity of the 90°.

It must be borne in mind, that there is no stability in a breed improved under the supposed conditions; but that, as soon as selection ceases it will regress to the level of the rest of the population through stages in which the deviation at starting, sinks successively to $w, w^2 \ldots w^n$ of its value. It may, however, happen that a stable form will arise during the process of high breeding, that shall afford a secondary focus of regression, and become the dominant one, if the ancestral qualities that interfere with it be eliminated by sustained isolation and selection. Then a new variety would, as I conceive, arise; but into this disputable topic there is no need to enter now.

We can thus understand the facility with which races of butterflies acquire mimetic forms, the severity of selection in their case being very great, while one of their generations occupies only a year. FRANCIS GALTON.

The Effect of Röntgen Rays on Liquid and Solid Insulators.

OWING to my absence from Cambridge in the Easter vacation, I have not until to-day seen the paper by Lord Kelvin, Dr. Beattie and Dr. M. Smolan (NATURE, March 25), on the influence of Röntgen rays on electric conduction through air, paraffin, and glass, in which the authors state that they cannot detect any influence of Röntgen radiation on conduction through solids. I think that the difference between this result and the one obtained by Mr. McClelland and myself arises from the temporary character of the effect of the radiation on solids. The increase in the conductivity of solids is only appreciable for a short time after the application of the electric force (see NATURE, July 30, 1896, p. 306); under long-continued electromotive forces the conductivity seems unaffected by the rays. The effect might perhaps be more accurrately described as an increase in the electric absorption, rather than as an increase in conductivity. I have been for the past few months engaged in experiments on the effect of the rays on solids and liquids, particularly liquids; and, though the experiments have been much interrupted by the pressure of other work, I hope soon to have them ready for publication. There is one experiment, however, which may be of interest. Of all the liquids tried, that sold as vaseline oil has proved the best insulator; in its pure state it is very transparent to Röntgen rays, so to increase the absorption of these rays I stained the oil with iodine, when it became very opaque to them. The oil does not insulate so well after staining as it did before, but the effect of a slight amount of conductivity is not of importance when the following method is used. Three electrodes, A, B, C, are placed in a leaden vessel filled with the oil. B, which is between A and C, is connected to one pair of quadrants of an electrometer, A and C to the terminals of a battery of 1000 small storage cells. If there is any leakage the potential of B will, in general, not remain zero after the battery is put on, but it will do so if an earth connection is made at the proper place in the battery. The base of the vessel below B C was cut out, and an aluminium vessel inserted, so that the liquid between B and C could be exposed to the Röntgen rays. A balance was obtained with the rays off; when the rays were turned on, the potential of B no longer remained zero, but changed in the way it would if the conductivity between B and C had increased. This effect was small but well marked, and seemed to last however long the electromotive force was kept on. J. J. THOMSON.

Cavendish Laboratory, April 24.

The Theory of Dissociation into Ions.

MR. SPENCER PICKERING has, in your number for January 7, brought forward certain difficulties which he says the advocates of the dissociation hypothesis have persistently ignored. I have been waiting in the hope that some one who supports the gaseous theory of solution as well as the theory of electrolytic dissociation would answer his letter. As no one has done so, I venture once more to trespass on your space.

First let me say that the experiment described by Mr. Pickering, in which water or propyl alcohol exudes through the walls of a semi-permeable vessel containing a mixture of these liquids, according as propyl alcohol or water is placed without, appears to me, as it does to him, to be very strong evidence that it is complex molecules of solution to which the walls are impervious. The experiment is one which certainly needs explanation at the hands of those who uphold the gaseous impact theory of osmotic pressure.

As I have already said, the idea that electrolytic conductivity depends on dissociation of the ions from each other, does *not* involve, as is so often assumed to be the case, the gaseous view of solution. The evidence for such dissociation appears to me to be exceedingly strong, as I will explain very briefly below, so that some explanation of the second experiment described by Mr. Pickering is necessary.

The experiment is this: The freezing point of a large quantity of acetic acid, to which is added a mixture of sulphuric acid and water in the proportions represented by $100H_2O + H_2SO_4$, shows that considerably less than 100 molecules have been dissolved. This result indicates that chemical union has occurred. Mr. Pickering says that, on the dissociation theory, the freezing point should be lowered by an amount corresponding to something between 101 and 103 molecules.

In such a case, however, we have conditions very different from those which hold when sulphuric acid is dissolved in water. In fact the liquid is in reality a mixed solution of water and sulphuric acid in acetic acid, or possibly, as Mr. Pickering suggests, of the hydrate of sulphuric acid in acetic acid. It does not at all follow that because sulphuric acid is dissociated in water, it is, therefore, dissociated in other solvents; in fact, the freezing points of its solutions in acetic acid show that, on the contrary, aggregation has occurred. We should, therefore, expect that dissolving sulphuric acid in acetic acid would have little or no effect on the conductivity; and this is also indicated by the low specific inductive capacity of acetic acid, which implies a low ionising power. There is no reason to suppose that the presence of a small quantity of water would modify the properties of the solvent enough to cause any appreciable change in the conditions.

But, even if these considerations were insufficient to explain the facts, the dissociation theory would not be discredited. As I pointed out in your issues of October 15 and December 17, 1896, dissociation of the ions from *each other* does not forbid the assumption that the ions are linked with one or more solvent molecules. Such a combination would explain Mr. Pickering's observation.

Mr. Pickering says that the dissociation theory depends solely on the numerical agreement obtained when properties of solutions are interpreted by its means. Although these numerical relations may have suggested the theory, they by no means furnish the only basis for it to rest upon. Other facts, to my mind, give much more conclusive evidence in its favour. As Mr. Pickering has challenged the supporters of the theory to explain his experiment, I may be allowed to ask the opponents of the theory to explain the following phenomena in any other way than by a dissociation of the ions from each other :—

(1) The velocity with which an ion travels through a dilute solution under an electric force is independent of the nature of the other ion present.

(2) The conductivity of a dilute solution is proportional to its concentration. The alternative to the idea of dissociation is to