LETTERS TO THE EDITOR.

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Fertilising Effect of Soil Sterilisation.

WITH further reference to the work of Messrs. Russell and Hutchinson on soil sterilisation (*Journal of Agricultural Science*, October, 1909), it may be interesting to record some information of which I have recently become possessed.

Some of the large growers of cucumbers, tomatoes, &c., under glass for the London market have for some little time adopted the plan of injecting jets of steam into their soil before planting, not with any view of increasing its fertility, but with the view of destroying slugs, insects, &c. In the experience of some growers the productivity of the soil after steaming has become so greatly increased that, if anything like the usual quantity of stable manure is mixed with the soil, the plants grow with such rank luxuriance as to spoil their bearing capacity, exhibiting all the symptoms that would be expected as the result of a heavy overdose of nitrogen.

This experience has been communicated to me by growers who were previously unaware of the Rothamsted work. At the moment they were feeling in somewhat of a dilemma: if they did not steam the soil they suffered from insect pests; if they did steam it they were obliged to curtail the supply of stable manure, at the expense of lowering the subsequent soil temperature, which is normally maintained at a high level by the fermentation of the manure. No doubt means may be found of adjusting the various conditions satisfactorily, but meantime the observation appears to afford striking independent confirmation on a practical scale of the indirect fertilising effect of partial sterilisation in killing off the phagocytes or protozoa which normally keep down the numbers of those bacteria the task of which is to turn organic nitrogen into plant food. BERNARD DYER.

17 Great Tower Street, London, E.C., March 15.

Certain Reactions of Albino Hair.

IN a note in the *Journal of Physiology* (vol. xxxviii.) on the chemical nature of albinism, Mr. Mudge describes some interesting observations which he made upon rats' skins. Starting with the presumption, based upon the work of Miss Durham and Cuénot, that an albino carries a chromogen and lacks the ferment necessary to produce pigment from it, and supposing that fermentation is a process of oxidation or reduction, Mr. Mudge argued that it might be possible to produce pigment artificially by means of an oxidising or reducing agent. He found by experiment that immersion of albino rat skins in a solution of 10 per cent. formalin and 70 per cent. alcohol in equal volumes resulted in a "vivid yellow colour" in the hairs; he further states that these coats, when washed in water and immersed in H_2O_2 (20 vols.), become changed in colour from vivid yellow to a "brownish tint" in about twenty-four hours. He adduces arguments to show that the coloration is due to the presence of a specific body in the hairs diffused through the keratin, and not to mere reaction between the keratin and the formalin.

I have repeated these experiments with various skins. In the case of the single albino rat skin which I used, the vivid yellow was obtained immediately on immersion in the formalin mixture. The change to brown in H_2O_2 was not obtained, but complete decoloration resulted from immersion in this reagent. Prolonged immersion in the formalin mixture also produced almost complete decoloration.

With guinea-pig albinos carrying, respectively, black, chocolate, and red, negative results were obtained, as they were also with a single mouse skin.

What struck me as particularly interesting in connection with the yellow colour produced by the formalin mixture in the coat of the albino rat is the fact that it is a peculiar canary-yellow, which I remember to have seen elsewhere among mammals only in members of the stoat family when the winter whitening is incomplete. A piece of pale yellow stoat fur acquired a much more intense yellow colour as a result of twenty-four hours' immersion in the formalin mixture; a similar piece was decolorised by H_2O_2. There can thus be little doubt that the yellow body produced artificially in the fur of the albino rat is a substance similar to the yellow pigment of the stoat's winter coat, and therefore probably represents a stage in the reduction of the pigment to the condition in which it exists in the white hairs.

Miss Durham tells me that it is a well-known fact that albino rats do not remain pure white if they are exposed to the action of light. Just as darkness is necessary for the production of a pure white coat in the rat, so a certain degree of cold seems necessary in the case of the stoat tribe, though in their case a change to a warmer climate does not at first prevent the usual colour-change in winter. Thus Eric Parker, in "The Book of the Zoo," points out, concerning a certain foreign pine-marten, that "the first winter he spent in the Garden his fur turned almost white to match the snows he would naturally have expected. The last two winters it remained brown, though it has lightened considerably towards yellow." This repetition of a periodic act without the usual stimulus recalls certain phenomena in plants, which Mr. F. Darwin attributes to memory. IGERNA B. J. SOLLAS.

Nitrogen-fixing Bacteria and Non-leguminous Plants.

MAY I be allowed to direct attention to two errors in Mr. Hall's letter in NATURE of December 23, 1909?

Mr. Hall states that "Pseudomonas and Azotobacter together (1·24) are less effective than when grown separately (0·91 + 0·56)." This comparison is incorrect. The fixation of free nitrogen by bacteria is estimated in terms of milligrams of nitrogen *per unit of carbohydrate* in the culture solution. Pseudomonas and Azotobacter together give 1·24 N for *one* unit of carbohydrate. Pseudomonas and Azotobacter grown separately give 1·47 N for *two* units of carbohydrate, hence the correct comparison is:—

	Per unit
Pseudomonas and Azotobacter together	= 1·240 N
Pseudomonas and Azotobacter separately	$= \frac{1·47}{2} = 0·735$ N.

Hence my conclusion that Pseudomonas and Azotobacter together are *more* effective than when grown separately is, I think, justified.

The second error has reference to a mean experimental error of ±10 per cent. Mr. Hall writes:—"By an error which the context rendered sufficiently obvious, I wrote "oats" instead of barley when dealing with Prof. Bottomley's first-quoted experiment with soil." May I point out that oats were the only plants mentioned in the "first-quoted experiment with soil"? Even if the increase of barley (13·6 per cent.) be taken, one fails to see how it is "sufficiently obvious" that a mean error of ±10 per cent. *more* than covers an increase (the lowest of the results quoted) of 13·6 per cent. W. B. BOTTOMLEY.

King's College, Strand, W.C., February 16.

A Sample of Spurious Correlation.

THOUGH regretfully unable to do justice to the mathematical reasoning of Dr. G. T. Walker in NATURE of January 6, I may, perhaps, be allowed to say that it is of the essence of the method that those dots (each expressing a comparison of two sums of thirty items) tend to arrangement in a straight band, or strip, with fairly defined borders. It is expected that future dots will generally come within those limits; but to affirm this in a given case, to say, *e.g.*, that the next dot will not be below a certain level, is it not, necessarily, to say something quite definite as to the character of the coming season, as that its rainfall, frost days, or other feature considered, will not be below a certain numerical value? If the one statement is warranted, so (by the nature of the case) is the other. Thus the essential point seems to me to be whether the past distribution of those dots affords a reasonable clue to their future distribution, and I do not see that my critic throws doubt on this.

I think (with all deference) that anyone who will give the method a full trial will find it distinctly helpful in a number of cases (I do not say in all).

ALEX. B. MACDOWALL.

8 Marine Crescent, Folkestone, January 14.