of concavity of the film." The most simple and satisfactory proofs of the relative efficiency, as well as the *direction*, of the resultant of these capillary forces, are to be found in the well-known contrary movements of small columns of water and of mercury, when introduced into conical capillary glass tubes placed horizontally. In these cases it is evident, that the effective forces are inversely as the radii of curvature of the terminal meniscuses, and are directed toward their respective centres of concavity.

He maintains, that, if the capillary forces were directed toward the centre of concavity of the film, "the tendency of a column of water raised between two floating bodies by surface-tension would be to lift those bodies: similarly, a column of liquid sustained in a fine tube would tend to lift the tube." Simple mechanical considerations are sufficient to show that he is mistaken in supposing that such a result would follow. Indeed, it is obvious that the elastic reaction of the common meniscus, formed when two such floating bodies are brought near to one another, *does not tend to lift them;* for the *vertical* component of the capillary forces, directed toward the centre of concavity, is exactly counterbalanced by the weight of the adhering liquid elevated between them, while the *horizontal* component is free to draw them together.

So, likewise, the column of liquid sustained in a capillary tube can have no tendency to 'lift the tube;' for it is evident that the weight of the liquid elevated must exactly balance the vertical component of the capillary forces acting at the crowning meniscus within the tube: the horizontal component tends to draw the sides of the tube together.

It is freely admitted that my explanation of this class of phenomena may be imperfect, and may be more or less unsatisfactory; but it seems to me that its shortcomings are not to be found in the directions indicated by the objections put on record by the critic. Such elementary facts as have been elicited above could not appropriately find a place in my paper.

After all, however, the simplest method of reducing this class of phenomena to the reaction of elastic films of liquids is the application (as has been done near the close of my paper) of the principle of Gauss; viz., that this reaction "always tends to reduce the surface to the smallest area which can be enclosed by its actual boundary." JOHN LECONTE.

Berkeley, Cal., March 16, 1883.

A new lecture experiment.

It has long been known, that an iron bar may be permanently magnetized by holding it in the direction of the dipping-needle, and striking it a blow with a hammer. The novelty of this experiment, so far as I am aware, consists in indicating the magnetization of the bar at the instant the blow is delivered. I use for the purpose a reflecting galvanometer (Kohlrausch's pattern), a lantern with detached lens for focusing the reflected beam (or, in the day-time, a *porte lumière*), a piece of gas-pipe 80 cm. long and 45 mm. diameter, and a coil of fine wire large enough to slip freely over the gas-pipe. After carefully demagnetizing the gas-pipe, the coil of wire is connected with the galvanometer, and slipped down against the hand, holding the pipe about 30 cm. from the upper end. With the pipe pointing in the direction of the dipping-needle, a ringing blow is struck on its upper end, and the spot of light on the screen moves promptly from two to four feet, according to the distance of the screen from the galvanometer. A second blow produces only a very small movement compared with the first one. Reversing the gas-pipe, and again striking it, the change of magnetism is indicated by another induced current about equal to the first. The direction of the current is the same as is obtained by moving the coil from the end struck toward the middle of the pipe. By moving the coil along the pipe, before the blow and after it, the induced currents indicate that the temporary magnetism of the pipe produced by terrestrial induction is much weaker than the permanent magnetism produced by the blow. H. S. CARHART.

North-western university, March 20, 1883.

HOUGHTON FARM EXPERIMENTS.

Houghton Farm. Experiments with Indian corn, 1880-81, with a summary of the experiments with wheat for forty years, at Rothamsted. Cambridge, *Riverside pr.*, 1882. 75 p. l. 8°.

——— *Agricultural physics. Series i. Nos. 1, 2. Meteorology and soil-temperatures.* By D. P. PENHALLOW, B.S. Newburgh, *Ritchie & Hull, pr.* [1883.] 57 p., 5 pl. l. 8°.

BESIDES the intrinsic value which these publications have as reports of carefully conducted experiments, they possess additional interest to all who have at heart the advancement of scientific agriculture in this country, because they are the first public reports of what is here a novel undertaking. The proprietor of Houghton Farm, Mr. Lawson Valentine of New York, has, in effect, established upon it an experiment-station devoted to the scientific investigation of agricultural questions. So far as we are aware, this is the first institution of the kind in the country supported by private munificence, and hence untrammelled by the demand for results of immediate practical utility, and by the mass of miscellaneous chemical work which seriously circumscribes the scientific activity of public experiment-stations. The outcome of this form of the 'endowment of research' will therefore be awaited with much interest.

The first of these reports gives an account of the field-experiments with Indian corn, executed by Dr. Manly Miles in 1880 and 1881. These experiments are, in the main, modelled after the famous Rothamsted experiments of Lawes and Gilbert, and are to be continued through a series of years, with the design of doing for Indian corn what the English experiments have done for wheat and barley. The experimental plots having been laid out and drained in the previous year, a crop of corn was grown in 1880 *without manure*, in order to test the uniformity of the soil and establish a basis for subsequent comparisons. This was followed in 1881 by a crop to which various kinds and quantities of manures were applied on the several plots, certain plots being left unmanured for comparison.

Unfortunately the season of 1881 was extremely dry, and the manures applied produced scarcely any appreciable effect; so that, although various minor results of interest and value were obtained, the main object of the experiments was scarcely at all advanced by the year's work. The most interesting of these minor results is, perhaps, the striking and beneficial effect exercised on the yield of some of the plots by the thorough drainage which they received. Barnyard manure was the only fertilizer which produced any noticeable effect; and this is ascribed rather to its physical action in making the soil more retentive of water than to any direct fertilizing action.

It is evident that circumstances have conspired to render this simply a preliminary report, whose value consists in its account of the plan and methods of the experiments more than in any results yet attained.

Dr. Miles appears to be fully aware of the complex nature of the problems attacked, and to have taken great care to execute all the operations of tillage, planting, cultivation, and harvesting in a uniform manner on the several plots. He is cautious, too, in drawing conclusions, and not in haste to attribute small difference of yield to the effects of different fertilizers, as is too often the case.

His method of comparing the yields of a manured and an unmanured plot is novel and interesting. Instead of assuming the difference between the two to represent the effect of the manures, as is usually done, he first grows a crop on all the plots without manure. In the crop of the succeeding year, he first notes the gain or loss of yield on the unmanured plot, and then assumes, that, if the plot to be compared had not been manured, its yield would have varied to the same extent. Then the difference between the actual yield of the plot and what it would have yielded without manure is regarded as the effect of the fertilizers applied to it. The following example illustrates the method: —

	Manure in 1881.	Yield 1880, unmanured.	Yield 1881.	Would have yielded without manure.	Gain due to manure.
Plot 1 . .	Muriate of potash .	27.1	43.5	36.2	7.3
Plot 3 . .	Nothing .	28.1	37.2	37.2	–

This method of comparison is evidently intended to take account of the natural unevenness of the soil, and it is to a certain extent an improvement over the direct comparison of yields; but it also involves errors of its own, and not only that, but errors of *unknown amount*. Because plot 3 yielded one bushel per acre more than plot 1 in 1880, it is by no means certain, that, in the very different season of 1881, the same difference would have been observed: indeed, it is highly probable that it would not have been. Dr. Miles recognizes this, and designates the 7.3 bushels of our table as 'probable increase produced by manures.' But he gives us no means of knowing whether this amount is within or without the limits of error; that is, whether the manure on plot 1 actually did produce an effect or not. This cannot but be regarded as a serious deficiency in these otherwise valuable experiments; and it is one that no care in the execution of the experiments can do any thing to remove.

A field-experiment with fertilizers involves one of two assumptions, — either that the several plots have exactly the same crop-producing power, or that the differences observed in a preliminary unmanured crop are constant. Neither of these assumptions is true. With the greatest care in the selection of plots, very considerable differences in both respects will show themselves. Such being the case, the scientific conduct of a field-experiment requires that the amount of error involved in the above assumptions shall be determined, to the end that we may know whether the apparent differences in the effects of the fertilizers have any real significance. This may be done by multiplying the number of plots which receive the same treatment, and distributing them uniformly over the experimental field; the only limit to the multiplication being that imposed by practical considerations of the possibility of treating a large number of plots.

In this way it is possible to obtain, not only the average yield of a certain fraction of an acre under particular treatment, but the amount of variation from that average which may be expected in individual cases. This method calls for a multiplication of the manured, as well as of the unmanured plots: it greatly increases the labor of conducting a field-experiment; but the results, once obtained, are reasonably accurate, and *we know how accurate they are*.

This whole subject has recently been very thoroughly discussed by Wagner; and a perusal of his papers[1] cannot fail to be in the highest degree interesting and suggestive to all who contemplate making field-experiments.

[1] *Journal für landwirthschaft,* xxviii. 9; *Landw. versuchs-stationen,* xxviii. 123.

The account of the Rothamsted experiments on wheat, from the pen of Mr. Lawes, which is appended to the report, will be read with special interest, as showing what important gains to our knowledge may result from such experiments as those initiated at Houghton Farm.

The papers on agricultural physics contained in the second report relate to local meteorology and soil-temperatures. Under the first of these subdivisions the most interesting statement is, that local predictions, based on the signal-service and on local observations, were made at noon for the succeeding twenty-four hours, with only two per cent of error. Confidence in them was established, and they served an important purpose for the time during which they were issued. The observations on soil-temperatures will, of course, yield more trustworthy averages when based on more than a single season's work; but results of value are already obtained. Eight thermometers with the bulbs immersed in oil within wooden cases, to prevent change of record during their observation, were placed at the surface, and at depths of three, six, and nine inches, and one, three, five, and eight feet, and were observed hourly between seven A.M. and nine P.M., from May to October, 1882, and sometimes throughout the twenty-four hours. The soil was gravel upon hardpan and clay. The observations are elaborately discussed by Mr. Penhallow, who obtains the following results. The penetration of the surface-heat to a depth of three inches requires one and a half to two hours; to one foot, eight to ten hours: hence, at a little greater depth than the latter, the diurnal waves of temperature would be reversed. Hourly change of temperature ceases at about eighteen inches, and daily, near eight feet; but these, as well as the average daily variations, being only for the hours from seven A.M. to seven P.M., need supplementary observations to show their full measure. The use of minimum thermometers would greatly increase the value of the results. Irregularities in the daily temperature-curve are considered first as shown in a diminished total variation ('mean depression of hourly variations'), and, second, as seen in marked irregularities in the curve ('sudden depressions'). The first of these is found to be always connected with rainfall and consequent excess of moisture in the soil, probably aided by absence of direct sunshine; the second generally comes either from a temporary obscuration of the sun, as by a passing cloud, or about as frequently from the reaction after a sudden rise of surface-temperature much above that of the soil below.

Of more interest are the comparative results of observations made in June, three inches below the surface, in one uncultivated, and two plots of cultivated ground, referred to in the report as *a* and *b*. One of the cultivated plots, *a*, had been treated with composted stable-manure; the other, *b*, with an equivalent mixture of commercial fertilizer; and both were planted with corn. The uncultivated ground had the greatest daily range, chiefly from its higher maximum temperature; plot *a* had the least range, as its minimum was $\frac{1}{2}°$ to 1° C. higher than in plot *b*. This diminished variation would seem to result from heat evolved by the decomposing manure.

All the observations are neatly recorded in tables and diagrams. Their only inconvenience arises from the use of even numbers of feet or inches in determining the depths for observation, while the records are kept in fractional centimetres; so that 3, 6, and 9 inches are always rendered 7.6, 15.2 and 22.8 cm. One system or the other should be fully adopted. As the first season of observation includes only the warmer months, studies of frost are not yet published.

FOSSIL ALGAE.

Apropos des algues fossiles. Par le marquis de SAPORTA. Paris, *Masson*, 1882. 76 p., 10 pl. 1. 4°

IN a fine imperial quarto, the author critically examines the nature of some impressions described by phytopaleontologists as remains of fossil Algae, but which a Swedish naturalist, Nathorst, in a considerable work published at Stockholm (1881), has considered as representing tracks of invertebrate animals. In his memoir, Nathorst illustrates by a large number of figures the tracks and impressions which the author himself and others have observed, as produced by the movements of small crabs, insects, worms, even of water-currents and waves, upon sand, or soft, muddy surfaces. As points of comparison, the Swedish author gives a list of the works where, to his belief, are represented so-called Algae corresponding to his figures. Among the memoirs quoted in the list are Saporta's *Paléontologie française* (vol. i.) — where, among the Jurassic plants, all the Algae, excepting Itieria and perhaps one or two others, are considered as true tracks — and the *Evolution du règne végétal*, by Saporta and Marion, where most of the impressions described as Algae are regarded as tracks of divers kinds. It is to defend his