

Agricultural Science and Experiment in the Eighteenth Century: an Attempt at a Definition

By G. E. FUSSELL

PLATO in the *Phaedrus* made the statement that the invention of writing would weaken, if not destroy, memory. Its result would be a forgetfulness in the minds of those who use it because they would not exercise their powers of remembrance. Writing was an elixir of reminding, and would allow pupils to read many things, thus gaining the appearance of learning. If he had been living in the present he would no doubt have remarked upon the changes taking place in the use of language as a result of the indiscriminate use of the art in propounding the vast extension of knowledge made since his day, and on the galloping way in which it is recorded, often by a torsion in the meaning of words.

This last can lead to confusion and often does. Words should not be used with a novel significance, or one known with exactness only to the writer, because that involves explaining this novel use to the reader, not a very easy job. This practice leads only to confusion between them, possibly entire incomprehension, certainly only a misty, foggy degree of clarity. It may be permissible in advancing technology, where processes and parts that were not known until they were invented must needs be made identifiable in words as well as in fact; but it is deplorably present in some of the less exact disciplines where it is presumed that previously unrecognized areas of knowledge are being discovered.

The reverse is equally true. When I was in America in 1967 I was accused of giving a too restricted meaning to the word "science" as applied to the investigations into plant physiology and nutrition in the narrower sense; and

it was argued that I should have included as "scientific" the various trial plots cultivated with various well-known as well as novel crops, or of manures tried out on a limited scale and by quite imprecise methods in attempts to win the premiums offered by the various improving societies in the eighteenth century, perhaps more particularly the premiums offered by what has become the Royal Society of Arts. Some of these trials were described in my essay, 'The Technique of Early Field Experiments'.¹

The technical changes in the conditions of life in the most mechanized countries, and the extreme rapidity of modern transport, have had a serious impact on the use of language. The widespread education of total populations, if only sufficient to make the majority literate and readers of the ephemeral products of the day, has supplied a means of exchange of words and phrases, often used in a loose and ill-defined sense, both in the written and in the printed vocabulary. Unfortunately this tends to be reflected in writing, popular and erudite.

Henry Hallam feared this a century and a half ago. He felt that "where literature is on the decline . . . there will be no longer any standard of living speech, nor any general desire to conform to it, if one could be found; and thus the vicious corruptions of the vulgar will entirely predominate. The niceties of ancient idiom will be totally lost; while new idioms will be formed out of violations of grammar sanctioned by usage, which among civilized people would have been proscribed at their appearance."²

Oliver Wendell Holmes was even more severe in the *Autocrat of the Breakfast-Table*, in

¹ G. E. Fussell, 'The Technique of Early Field Experiments', *J.R.A.S.E.*, 96, 1936.

² Henry Hallam, *View of the State of Europe in the Middle Ages*, III, 1819, p. 318.

which he declared that "to trifle with the vocabulary which is the vehicle of social intercourse is to tamper with the currency of human intelligence. He who would violate the sanctities of his mother tongue would invade the recesses of the paternal till without remorse, and repeat the banquet of Saturn without remorse."

Maturity of this kind is rarely to be found at the present day when every excuse is made for popular locutions that have no precision, and that attach new meanings to words whose purpose has been sanctified by ancient usage; but that is a consideration which has become only a cock-shy. Indeed, Brian Foster has produced a book not to excuse but to laud these verbal iconoclasm.¹ He is only one of the protagonists of the new prose. Its critics in the popular press do not have so great an impact: all must bow to the modern or, shall I say, the uncritical.

The contrast I am trying to define is well marked by what was perhaps the first demonstration plot and the careful investigations of van Helmont into plant nutrition—both events of the seventeenth century. These things are perhaps too well known to need any very detailed exposition here. Van Helmont concluded that water was the sole nutrient of plants, in which, in point of fact, he was not too far wrong, because it is now accepted that water provides a medium for the nutrients in the soil solution to be absorbed by the plant. This was an elementary scientific experiment leading to a working hypothesis, although that hypothesis, like all others, has been, or is likely to be, superseded. Bacon, before van Helmont, had tried out the effects on germination of soaking seeds in solutions of various substances, but admitted that his results did not lead anywhere.

Walter Blith, on the other hand, a man who had been one of Cromwell's captains, was possibly a practical farmer, and his comparative trial plots are the first recorded, if not the earliest carried out. In the course of these trials, which lasted several seasons, he measured the results by an untreated plot, i.e. one that got no manure, which he supposed would act as a

control; but the whole affair was so perfunctory that no reliance could be placed upon the results, because there was no arithmetical criterion. Nothing, apparently, was measured. Nothing is related of the amounts of the different manures applied, or of the treatment of the control plot, and no measure of the harvest of the different crops reaped from the various plots in the successive years. Nor is there any indication of the soils in which the trials were made.² All this, which in modern times is elementary, was not in the consciousness of a man who sought information from what may be regarded as the first trial plots. Clearly, this trial, novel as it was, cannot be assessed as scientific in any sense of that word as it is understood today. The work done by van Helmont was!

Other people were working on what may be described as scientific lines. Amongst them the English exemplar, after Ray, is the Rev. Stephen Hales. The most critical could hardly denigrate his efforts to understand the underlying principles of plant life. Such work was being done while many members of the Royal Society were engaged upon toys and curiosities. These had little relation to the problems of the farmer, who was unaware of more than his normal difficulties created by the vagaries of the weather, the incidence of disease in plants and animals, and the awkwardness of the workers.

With the formation of agricultural societies, the earliest being the Society of Improvers in Agriculture founded in Scotland in 1723, a mild passion for trial plots was inflamed, and far too much importance was placed on their rather limited results. This development was more or less in parallel with the so-called agricultural revolution of the late eighteenth century. Premiums and medals were offered for this and that, e.g. growing 3 acres of one crop or another. The Society for the Encouragement of Trade and Manufactures was one of the most prolific in making such awards, and the opportunity of winning a premium or medal became greater as the number of societies increased. Prominent were the Highland and Agricultural Society and the Bath and

¹ Brian Foster, *The Changing English Language*, 1968, *passim*.

² Walter Blith, *The English Improver Improv'd*, 1652, pp. 136-7.

West, and related were the numerous farmers' clubs etc., of more limited membership and consequence. These premiums and medals were fine, and there can be little doubt that they encouraged some degree of improved farming, though probably limited to a minute area of the countryside in the neighbourhood of the so-called experiments. The apotheosis of this sort of thing was Arthur Young's 2,000 experiments made in the brief space of five years, and described in his *Course of Experimental Agriculture*, of 1770. Most, if not all, of these efforts were no more than trials to see what happened, but carried out with no controls, and without any appreciation of the numerous and varied matters that must be taken into consideration if any adequate comparison with what may be called normal farming was to be advantageously made; and indeed there is no indication, and probably little possibility of being able to produce the same results, if they appeared advantageous, in the following years.

All the same, Young, characteristically, had the essence of the matter. He realized that an untreated plot was some kind of control upon the results obtained. In the *Annals of Agriculture* he described a series of experiments on the food of plants. These were really trial plots treated with a variety of substances that then might have been supposed to have provided the plant with nutrient. Some were rather eclectic to modern ideas: but he himself admitted that experiments of this sort would never have uniform or consistent results. The only consideration that seems to have been taken into account was that these trials (they were not in the modern sense controlled experiments) were carried out "in the same soil." Like Blith, from whom he may have learned, Young kept one plot untreated as a rough sort of control. He also made some "pot" trials which were the noble ancestry of many modern inquiries of a similar sort. He was a man of the most singular penetration, considering the mental environment of his era. He promoted the idea of an experimental farm which, as is well known, came to nothing at that time, and for long thereafter.

Carrying out trials was fraught with danger in that age of aristocratic domination of the

land and the people who served it. Young realized that no agricultural trial could be carried out in less than six months, few in less than one year. Many demanded three, four, or six years to effect. The expense of this was very great, and disastrously the process was in the hands of clowns, and "what is oftentimes much worse of ill-educated, conceited, ignorant, pert bailiffs, who find either pleasure or profit in a miscarriage." Shades of Tull! The higher ranks were no better. When out shooting, sportsmen would often trample on a trial plot, and vermin, rabbits, rats, and such would consume the crop. Nobody could control the sportsmen, and the vermin of a smaller kind were equally difficult to restrain. Contemporary social and feral conditions were not indeed favourable to agricultural trials although this statement must be qualified. Many farmers and landowners were trying new ideas, as I suppose they always had, but not in the controlled, precise manner of the modern research station, though even that remarkable institution will doubtless be superseded as our descendants progress further—into what it is quite undesirable to think. It would be possible to labour the point about what was called agricultural experiment in the eighteenth century, but enough has been said to show that many farmers and landowners were making trials of this and that, not only upon the growth of crops in variable conditions of soil structure, drainage, and manuring, but also into a variety of methods of breeding and feeding animals.

Meanwhile, the scientists were investigating the physical constitution of plant life, the absorption of a variety of plant foods, the intake and exhalation of gases and so forth; and, in addition, were occupied with the composition of the atmosphere, the inhalations and exhalations of vegetable life of all kinds. Later all this became of the utmost importance to the farmer when knowledge was sufficiently advanced to make it helpful, and the farmer was sufficiently educated (if such there at first were) to appreciate what it meant to the success or failure of his operations. The wide gulf between scientific experiment and the trial plot of the amateurish kind was difficult to pass over, and it was not bridged until well into the nineteenth century, dare it be said,

until the work of Thaer, Lawes and Gilbert, and Liebig.

A trial is not an experiment though an experiment is a trial. A trial is planting a crop and waiting to see what happens. This is in a sense, I suppose, an experiment, but it is not a scientific experiment from which definite results can be secured, results that enable the crop to be grown in subsequent years with a substantial foundation for the expectation that it will behave in the same way. The single trial was the sort of thing for which the premiums were offered in the eighteenth century, e.g. for growing 3 acres of this or that, and recording what happened. This sort of thing was not a scientific experiment, a process that was not fully developed until well into the nineteenth century. However, as Hugh Kearney has pointed out, "experiment for its own sake is apt to be pointless."¹ A trial plot of potatoes, for example, may have resulted in a useful crop, but it was not a guide to what might happen in the next year or the year after that, although optimistic people might think so. In order to secure that kind of certain knowledge something quite different and much more complex was necessary, and checks and cross-checks must be imposed. It was not until the 1840's or thereby that such methods were devised and more or less perfected. There is always in the most carefully planned experiment a number of unknown and uncontrollable factors, as modern scientists would be the first to acknowledge. Nevertheless, if a scientific experiment is reasonably planned and repeated, results can be deduced that will make it possible—within limits—to duplicate the conditions and cultivate a crop with a proper expectation of success.

But in or about 1750 a Frenchman made a field trial on the control of black bunt very much on the lines of modern trials of the kind, which may be looked upon in some sense as the progenitor of this method. He laid out several sets of plots arranged in rectangles of

six plots each 18 feet long by four plots wide. Careful record was made of the different treatments applied, manuring, and so on, checked by an untreated plot, and the results were carefully recorded. A good many factors were neglected, but this is in many ways quite the modern process. This experiment was defective, but it was as ingenious as was possible in the circumstances of the day. Even with the modern random distribution of trial plots over an experimental field, and care to make the conditions as uniform as possible, there can be no guarantee that the yield of the best plot can be repeated on a larger area, which is in fact always doubtful. Moreover, in the major part of this kind of work no notice is taken of the cost of production, a major consideration, and one upon which Young was insistent, perhaps because of the separation today of the scientist and the economist.

It is an impertinence on my part, but I would like to suggest (no new thing) that "scientific method" is to formulate a hypothesis based on exact work, test, and confirm it or cast it aside and proceed from a new base. This was the method adopted by Stephen Hales and other eighteenth-century scientists, but until it was vulgarized it is my belief that the findings of the early scientists were unlikely to reach the farming districts or the farmers working there. On this point I am a confirmed agnostic after some fifty years of reading on the subject.

"Incredulity is a grand power," said Blackmore's *Cripps the Carrier*. He was speaking of the ordinary affairs of life, but maybe it is an even greater power in scientific and historical investigation; and so I would like to say that trials and demonstration plots are in some sense educational, while scientific research is, or should be, directed to the discovery of new knowledge; though its application must be left to those who can make use of it in a practical way. This is the distinction that has been criticized, but I do not think conclusively, if at all.

¹ Hugh Kearney, *Science and Change, 1500-1700*, 1971, p. 49. Cf. D. S. L. Cardwell, *Technology, Science and History*, 1972, p. 111.