and laid stress on the need for conserving ether space in the transmission of television signals. This phase of the subject will become of enhanced importance when the possibility of colour television is discussed, and also in connexion with the relaying of programmes over very great distances. Ten supporting papers contribute much detailed information to this question, and two of these deal with some fundamental aspects of colour television of both a subjective and objective nature.

There is no doubt that the holding of this convention brought together a large amount of detailed and expert technical knowledge on the various aspects of television described above; and this knowledge will prove very valuable to those responsible for the future development of television everywhere. The complete proceedings of the convention, containing the addresses, the full text of all the papers and reports of the discussions, will be published in four issues (Nos. 17–20 inclusive) of a special volume of the *Proceedings of the Institution of Electrical Engineers*, Part III A. The first of these special issues is expected to appear shortly. Those requiring copies should apply to the Institution of Electrical Engineers, Savoy Place, London, W.C.2, from whom details of the contents and subscription-rates may be obtained.

R. L. Smith-Rose

PRINCIPLES GOVERNING THE AMOUNT OF EXPERIMENTATION IN DEVELOPMENTAL WORK

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Great Britain has lagged badly behind the United States in the practical utilization of scientific discoveries. This is reflected in the demand for men of science in the two countries. The recently published Fifth Annual Report of the Advisory Council on Scientific Policy[1], for example, directs attention to the fact that the United States is turning out nearly three times as many men of science in proportion to the labour force as Great Britain is, and is planning further increases, particularly at the higher level. In part this lag in putting scientific discoveries to practical use is attributable to our neglect of the experiments and tests which are necessary in developmental work. These experiments differ markedly from the laboratory experiments required in pure scientific research, since their main function is to establish empirical rules of operation which are applicable under practical conditions. With the present drive for economy there is serious danger that even such facilities as are available for experimental work of this kind will be curtailed or not used to full advantage. It is therefore important to stress that such curtailment will result in much more substantial and immediate losses through failure to determine the best practices. Developmental work must be expanded, not contracted, if we are to survive.

It is perhaps somewhat remarkable that no very precise consideration appears to have been given to the expenditure that is justifiable in developmental work. Instead, experimenters have tended to rely on their intuitive judgment on the accuracy that should be aimed at, and are frequently influenced in this judgment by the demands, often misplaced, for economy. In its essentials the problem is very similar to that of deciding the accuracy required in a sample survey. For this latter problem I put forward the general principle that the accuracy should be such that the sum of the cost of the survey and the expected losses due to errors in the results should be minimized[2]. The same principle can be applied to experimental work. It is indeed here more widely applicable, since it is usually easier in experimental work to assess the losses due to errors of a given magnitude in the results.

In experimental work, however, we have at the outset to consider the type of results that are likely to emerge and the way in which they will be used. In the case of a treatment of the all-or-nothing type we may simply require a decision on whether or not to apply the treatment uniformly to the whole of the material, or to certain categories, already defined, of the material; or we may hope that the experiments themselves will reveal a way of classifying the material into categories for some of which the treatment will be profitable and for others unprofitable. Similarly, in the case of a quantitative treatment, we may require only to determine what uniform level of treatment gives the greatest economic return, or we may hope to divide the material into categories for which different levels of treatment will be most economic, or find some quantitative characteristic of the material which is correlated with the most economic level.

If the discovery of appropriate categories for non-uniform treatment is the aim, more elaborate experiments will often be required. We may also be prepared to sacrifice some of the smaller gains resulting from pushing the simpler type of investigation to its economic limit in the hope of obtaining the much larger gains which will result from the effective use of differential levels of treatment. For the moment, however, we will leave aside these more difficult problems, and consider the simple case in which the whole of the material is to be treated uniformly.

When a decision whether or not to apply a treatment is required application of the above principle is complicated, and we shall not discuss the matter further here. The problem is probably only soluble in terms of fiducial probability, using the sequential approach, and then with difficulty. It is essentially that propounded by Wald in his "Statistical Decision Functions"[3]. When the most economic level has to be determined, on the other hand, the solution is very simple, and reveals a number of features of general interest.

At the outset it will be well to make clear exactly what is meant by the concept of expected loss due to errors in the results. Consider, for example, the question of the use of fertilizer on an agricultural crop. If the whole of the crop is to be treated alike, the most economic level of dressing, which for convenience we will call the *optimum* level, will be the level at which the cost of a further small increment of dressing exactly equals the value of the resultant average increment in response. In the neighbourhood of the optimum the net loss due to departure from the optimum will in general be proportional to the square of the difference of the actual dressing from the optimum. If, therefore, instead of the optimum dressing $\hat{x}$ we apply a dressing $\hat{x} \pm \delta x$ there will be a net loss of $\lambda(\delta x)^2$ per acre, where λ is some constant.

If $\hat{x}$ is estimated by means of a set of experiments, and the estimate is subject to an error variance $V(\hat{x})$, then the average value of $(\delta x)^2$ over a large number of such sets of experiments will be $V(\hat{x})$. The expected loss due to error in the optimum will therefore be $\lambda V(\hat{x})$. The actual loss may of course be less or greater than the expected loss. An inaccurate set of experiments, for example, may by chance give the correct answer.

If, therefore, the cost C, apart from any 'overhead' or constant component, of an amount q of experimentation is cq, and the error variance of the estimate of the optimum level determined from such experimentation is v/q, the expected loss L will be $\lambda Av/q$, where A is the amount of the material to be treated. We therefore have

$$C + L = cq + \lambda Av/q.$$

This is a minimum when

$$q = \sqrt{(\lambda Av/c)}.$$

In this case

$$C = L = \sqrt{(\lambda Avc)}.$$

In other words, *the most economic amount of experimentation is that in which the cost of the experimentation apart from overheads is equal to the expectation of loss due to errors.*

If an amount A of material has to be treated each year for an indefinite period, and the experiments take a year to carry out, A must be replaced by A/r, where $100r$ is the interest rate. If constancy of conditions cannot be assumed indefinitely and it is decided to repeat the experiment after t years, then A is replaced by

$$\frac{A}{r}\left\{1 - \frac{1}{(1+r)^t}\right\}.$$

The net loss resulting from undertaking a fraction or multiple f of the most economic amount of experimentation is

$$(f + 1/f - 2)C.$$

If, for example, the cost, apart from overheads, of the most economic amount of experimentation is £4,000, the costs and losses when f has the values of 1, $\frac{1}{2}$ and $\frac{1}{4}$ will be as follows:

f	Cost of experiments (C)	Expected loss (L)	Cost + loss ($C + L$)
1	£4,000	£4,000	£8,000
$\frac{1}{2}$	£2,000	£8,000	£10,000
$\frac{1}{4}$	£1,000	£16,000	£17,000

This table can be looked on as giving the expected additional gains from additional amounts of experimentation in the neighbourhood of the most economic amount. The total gain from the experimentation as a whole cannot, of course, be assessed by this means. In the common case in which the application of the treatment cannot be risked without *some* experimentation, the total gain will be that due to the use of the treatment. This will often be large compared with the marginal gain in the neighbourhood of the most economic amount of experimentation. If, for example, the gain from using the treatment at the correct optimum level is £100,000, and overheads and preliminary fundamental scientific research are costed at £5,000, with other costs as above, the total and net gains will be as follows:

f	Research expenditure (C + £5,000)	Expected total gain (£100,000 − L)	Expected net gain
1	£9,000	£96,000	£87,000
$\frac{1}{2}$	£7,000	£92,000	£85,000
$\frac{1}{4}$	£6,000	£84,000	£78,000

In other words, the return on the first £6,000 of research expenditure is £84,000, on the next £1,000 is £8,000, and on the next £2,000 is £4,000.

If, therefore, experimental resources are scarce there may be a case for not carrying out the full amount of experimentation that can be justified on economic grounds, since the returns on the last few incremental steps are relatively small, and it may therefore be possible to use the immediately available experimental resources more effectively on other problems. However, the experimentation which is required to determine an optimum for uniform treatment is often capable also, if properly planned, of providing a basis for differential treatment. If successful in this respect further large gains may accrue. Consequently, since gains must on the average result from increasing the experimentation to the most economic level for uniform treatment, even if no effective basis for differential treatment is found, there is every justification for building up an experimental organization which is capable of dealing with this volume of experimentation.

There is one further point which is often of considerable practical importance. In many types of experimentation it is somewhat difficult to ensure that experimental conditions fully conform to the conditions under which the treatment will be used in practice. If this is not the case a constant component of error, that is, a bias, will be introduced. An error of this kind will not be eliminated by increasing the amount of experimentation, nor will it be revealed by the variability of the experimental results. Although the gains resulting from increased experimentation will *on the average* be the same whether or not a bias exists, the elimination of the bias may result in much larger gains. Additional experimental effort may therefore in such cases be much better directed to the elimination, reduction or correction of the bias, rather than to the reduction of the relatively small random component of error. To effect this a radically different type of experiment may be required.

As an example of the application of the above principles to a practical situation we may consider the experimental work on the effect of fertilizers on sugar beet, a crop introduced into Britain after the First World War. The early recommendations were based on Continental experience, but after various unco-ordinated experiments had been carried out a co-ordinated series of factorial experiments was started in 1933 and continued to 1949 to test the responses to nitrogen, phosphate and potash. The average number of experiments was twenty-two per year, and the total annual cost, exclusive of overheads, was about £700 per annum at pre-war prices.

As an example of the practical value of these experiments it may be mentioned that the results obtained up to 1939 enabled confident recommendations to be made in 1940 on the manuring of sugar beet under war-time conditions[4]. It was recommended, for example, that a dressing of 0·7 cwt. nitrogen per acre would be optimal, in contrast to the dressing of 0·45 cwt. nitrogen which had previously been recommended[5], and which was, in fact, about optimum at the prices ruling prior to the War. The net gain resulting from the change from 0·45 cwt. nitrogen to 0·7 cwt. nitrogen was about 15*s.* per acre at 1940 prices. If this change were made on the whole of the sugar beet acreage the total net gain would be £300,000 per annum. The actual net gain was probably considerably larger, since under-manuring of sugar beet (and other crops) was common before the War.

The determination of the most economic number of experiments in a situation of this type is complicated, owing to the fact that there is an additional random component of variation in response from year to year due to variation in meteorological conditions, and there is also a possibility of long-term changes due to changes in agricultural conditions. Without going into details, which will be dealt with elsewhere, it may be stated that an analysis of the variation in the nitrogen responses indicates that as far as this fertilizer is concerned about sixty experiments per year, possibly falling to forty per year after the first few years, may be regarded as most economic. In view of the fact that many farmers do not as yet follow at all closely the recommendations emerging from such experiments, the programme can be regarded as about adequate for the simple function of determining optimum uniform dressings. Indeed, from this point of view, the most serious defect of the experiments was the fact that the selection of the sites was not fully random, so that appreciable biases may have been introduced into the results.

The experiments were, however, quite correctly undertaken with the additional and more ambitious aim of investigating possible differences in response on different soil types, and establishing relations between responses and chemical analyses of the soils. In the case of nitrogen, for example, it was confirmed that the responses to nitrogen on fen soils (constituting 10 per cent of the sugar beet acreage) were very small, and would not pay for the cost of nitrogenous fertilizer. This fact is still not fully appreciated by the farmers concerned, 85 per cent of whom, according to a 1945 survey of fertilizer practice, applied nitrogen averaging 0·3 cwt. nitrogen per acre. Omission of this nitrogen would save about £25,000 per annum at present prices, that is, about twenty times the annual cost of the experiments. Considerably larger gains may be expected from the better utilization of the differences in response to phosphate and potash on different soil types and their relations with the chemical analyses of soils. That there is room for substantial economies is obvious when it is realized that the total cost of fertilizers applied to sugar beet is of the order of £3 million per year. For these more ambitious objectives a larger number of experiments would certainly have been economic.

But if the experimental work on sugar beet must be judged to have been scarcely adequate for the purposes for which it was undertaken it is, in fact, the high-light of all experimental work on fertilizers in Great Britain. Sugar beet is the only crop on which any co-ordinated series of modern well-designed factorial experiments has been carried out and the only one for which there has been any attempt to secure a selection of fields for the experimental sites which would be reasonably representative of the whole of the land growing sugar beet. For no other crop is there any adequate series of experiments for which chemical analyses of the soils of the experimental sites have been made, or for which there are any adequate descriptions of these soils. On the more difficult questions of fertilizer practice, such as how to use fertilizers on grassland, and the residual values of phosphate and potash, the amount of co-ordinated experimental work is negligible.

If experimental work in agriculture is to be expanded to a more economic level it will, of course, require a complete reconstruction of the existing machinery for experiments. More work is required not only on fertilizers but also on the many other aspects of crop and livestock production that are capable of simple and exact investigation by empirical experiments. If properly organized, such activities need not interfere with more fundamental scientific research, since technicians rather than research workers are the main requirement. The principal functions of the research workers will be to determine what experiments are worth while, to see that they are properly planned, and to examine the results for relationships which are unknown or only suspected when the experiments are begun.

I have chosen an example from agriculture because this is the field with which I am most familiar, and because it can be demonstrated by the principles set out in this article that the amount of empirical experimentation is here entirely inadequate. The general principles enunciated are of wide application in many other fields, and in particular in many branches of industrial research. The technique of empirical experimentation on highly variable material is relatively new, depending in large part on statistical developments made by British workers during the past thirty years. We have yet to learn how to use this technique to the best advantage, but that it justifies wider and more thorough use there is no question.

[1] Fifth Annual Report of the Advisory Council on Scientific Policy (1951–1952). (H.M.S.O., London, 1952, Cmd. 8561.)

[2] Yates, F., "Sampling Methods for Censuses and Surveys" (London: Griffin, 1949).

[3] Wald, A., "Statistical Decision Functions" (New York: Wiley, 1950).

[4] Crowther, E. M., and Yates, F., "Fertilizer Policy in Wartime: the Fertilizer Requirements of Arable Crops" (*Emp. J. Exp. Agric.*, **9**, 77–97, 1941).

[5] Ministry of Agriculture and Fisheries, "Arable Crops on the Farm", Bulletin No. 72, H.M.S.O., London, 1937.

OBITUARIES

Prof. H. A. D. Neville, C.B.E.

AGRICULTURAL education and the University of Reading have both suffered a heavy loss by the death on June 17 of Prof. H. A. D. Neville, emeritus professor of agricultural chemistry in the University of Reading.

Neville was born at Blackburn in 1880 and was educated at Queen Elizabeth's School and the Technical College, Blackburn, obtaining, from the latter institution, the degree of B.Sc. (London) as an external student. After a brief spell as demonstrator under Prof. W. J. Pope, at the Manchester College of Technology, he returned to Blackburn as lecturer and demonstrator under Dr. R. H. Pickard. From there he went, in 1906, to the County Laboratories at Chelmsford, and it was here that he made his first contacts with agricultural chemistry. In 1911 he proceeded to Cambridge to work with Prof. T. B. Wood and Prof. F. Gowland Hopkins; there he was in the midst of agricultural chemical teaching and research. August 1914 found him in the Royal Engineers. He rose to the rank of captain and was mentioned in dispatches. By 1917 he was carrying out research in the Munitions Investigation Department. Neville returned to Cambridge early in 1919, but by September of that year he was appointed to the chair of agricultural chemistry at University College, Reading. Within a year he was elected dean