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Confessions of a narrow-minded applied biologist, or why do interdisciplinary research?

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Introduction

I can write or talk only on subjects closely related to my own research. So this address will not be a philosophical discourse about the state of applied biology. Nor will it be a well documented justification of the contribution that research in applied biology has made to British agriculture, although there seems to be an ever increasing need for such justification in the face of the attacks agricultural science is suffering from government and from the general public. I found an excellent precedent for this narrow-minded approach while searching for introductory material among my predecessors' addresses, though it is one I scarcely have the presumption to follow. In 1966 F. C. Bawden chose a specific scientific subject – virus research – rather than a philosophical discussion of a contemporary problem because he was, he said, "by nature an experimentalist much more impressed by facts than opinions" (Bawden, 1966*a*). Having decided to stick to my own field, crop physiology, I then turned to what D. J. Watson had said about this discipline in his presidential address. Don Watson taught me most of what I know about crop physiology and said most of it too in 1968, and much better than I could (Watson, 1968). Moreover, little progress has been made in the intervening 17 years in solving what he listed as the outstanding problems. So instead of concentrating on crop physiology alone, I intend to make use of my experience during the past six years and to discuss multidisciplinary experimentation, though no doubt a bias towards physiology will creep in.

A meeting of our Association would certainly appear to be an appropriate place for such a discussion. We have members working in most of the disciplines concerned in the production of agricultural crops, though getting them together to take part in interdisciplinary activities is difficult. Why is this? There is no doubt about the multidisciplinary nature of The Association which is illustrated by the titles of our nine groups. We have multidisciplinary meetings like this one with parallel sessions; but rather a lot of encouragement seems to be needed to get more than two groups to co-operate in a joint session. We had a greater proportion of meetings that crossed the boundaries between disciplines before 1967 when the establishment of groups started. In the three years 1964–1966 approximately a third of the meetings could be categorised as interdisciplinary, another third would have attracted an audience from two of our present groups, and in only a third was the subject narrow enough to come within the field of a single group. The latter category contributed 70% of the meetings held during the past three years, and subjects that I rate as interdisciplinary account for only 15%; co-operative ventures between two groups contributed 6% and 9% were multidisciplinary meetings with concurrent sessions. But the formation of groups has certainly also had desirable effects. It has been correlated with an increase in activity (Hirst, 1980), a correlation which probably does represent cause and effect. Our groups guard their independence jealously and this is right. They are the source of new ideas which should not be suppressed by central pressures for integration. In this connection, the excellent truly interdisciplinary meetings initiated by the Weeds Group should be remembered.

There has been a tendency for a long time in pure science to regard specialised work as better than more generalised, and this continues in spite of a trend to more widely-based degree courses. No doubt much of this is a consequence of the enormous increase in scientific knowledge which makes the broad interests of individual scientists in the last century almost impossible today. But whatever happens in pure science, applied biologists should realise the importance of maintaining interdisciplinary interests. For no farmer would worry about the pests on his crops but ignore the diseases or the nutrition, even though nowadays, regrettably, fewer and fewer need to take an interest in both crops and animals. In the past, scientists also dealt with plants and animals; but few do nowadays, in spite of the policy of the Journal of Agricultural Science. It is not well known that Lawes and Gilbert studied animal as well as plant nutrition (Bawden, 1966*b*). They proved that nitrogenous materials were not essential to supply energy, thus showing Liebig to be wrong about animal nutrition as they had already shown about plant nutrition. However they supported Liebig in showing that animal fat could be produced from carbohydrates in the food and not only from fatty or nitrogenous compounds as was generally believed. These important studies on animal nutrition were not continued at Rothamsted after the deaths of Lawes and Gilbert which may be one reason why the work is unfamiliar to many people today.

During the present century, research into most aspects of crop production became established at Rothamsted. So a career of more than 35 years spent there should be some qualification for talking about the multidisciplinary approach. I hope, though doubt, that it will be possible for someone to say the same after another 35 years. But staying in one job for so long is not recommended for broadening the mind. Also, like most research workers, I have spent a lot of time doing my own thing by myself. When I started research, teams were rare and most researchers worked alone when young, and, if they were successful or lucky, with an increasing but small number of support staff as they became older. This type of career attracts people who prefer dealing with things to dealing with other humans and consequently such people often find it difficult to adapt to the more co-operative attitude needed for multidisciplinary activities. Although temperamentally preferring things to people, I was fortunate to get a broad-based education at Cambridge, not common in many universities at that time, and this may have fitted me to some extent for the interdisciplinary work I have undertaken recently. Certainly, the only job interview I have had was for a post as an advisory plant pathologist in the then NAAS (now ADAS). However I opted for physiology and had little to do with other disciplines until quite recently. So I cannot give a wide-ranging discourse such as we have enjoyed in the past from A. H. Bunting (1964) or F. T. Last (1978). What I will try to do is to use my experience first as a specialist researcher with my own projects in crop physiology, and latterly as a member of a multidisciplinary team, to examine the advantages and disadvantages of the latter approach.

Background to Rothamsted's multidisciplinary field experiments

The lessons to be learned from the multidisciplinary experiments done recently on winter wheat at Rothamsted cannot be appreciated without knowing something about how the project started and what the design and treatments were. During 1975 there was a lot of discussion at Rothamsted about the future of field experimentation, how best to co-ordinate knowledge and research in different disciplines, how to investigate the causes of variation in yield between similarly treated crops, how to determine why so many farm or even experimental yields were below the best recorded, etc. Internal papers were produced by G. W. Cooke, J. M. Hirst, J. McEwen, G. V. Dyke and others. Eventually P. J. Welbank and I were asked to submit proposals for experiments on factors limiting yield of cereals after consultation with others. Our attempt to follow these instructions was frustrated for various non-scientific reasons. Another group was assembled in 1977 and, after considerable discussion, the first of what became a six-year series of multidisciplinary wheat experiments was sown in the autumn of 1978. In the meantime a series of multidisciplinary experiments on field beans had been started in 1976, sponsored by a group led by J. McEwen.

To sponsor the first multidisciplinary experiment on winter wheat, some scientists from the disciplines that were thought relevant and were represented at Rothamsted were selected. They will be referred to as 'sponsors'. They were all people who would work on the experiment themselves and so were motivated to make practicable decisions. Moreover they were all considered to be of equal status, whatever their scientific grades or ages, and so there was no question of some disciplines being represented merely to provide a service to others. The group was charged with selecting detailed objectives within the broad aim of investigating causes of variation in yield, deciding on the treatments to be compared, the statistical design to be used and the measurements to be made. It is very important in projects of this kind that all the scientists involved contribute democratically from the earliest stages of planning. The main sponsors came from seven Rothamsted Departments and covered eight different 'disciplines' (Table 1). A group of this size cannot function without some kind of leadership. The wheat group was extremely fortunate in the person selected as its convener, R. D. Prew. His good sense and tact have smoothed over many potential difficulties and ensured that the project benefited fully from everyone's expertise.

Table 1. *Members of the Rothamsted Wheat Group, associated scientists and the departments represented*

Wheat Group	*Associates*	*Departments*
N. Carter	P. B. Barraclough	Entomology
B. M. Church	D. S. Jenkinson	Field Experiments
A. M. Dewar	A. H. Weir	Physiology and Environmental Physics
J. Lacey	P. J. Welbank	Plant Pathology (for Mycology and Virology)
A. Penny	F. V. Widdowson	Nematology
R. T. Plumb	D. W. Wood	Soils and Plant Nutrition
R. D. Prew		Statistics
G. N. Thorne		
A. D. Todd		
T. D. Williams		

Plus support staff too numerous to list

The objectives are better discussed after describing the experiments because some objectives were added to the original list as the experiments developed and treatments were changed, and others have been attributed to the experiments by people other than the original sponsors after the wide usefulness of the result was realised.

Description of Rothamsted's multidisciplinary field experiments

A multidisciplinary approach to field experimentation implies that people with interests in a range of disciplines work on the same crop at the same time. The relative importance in determining yield of different factors such as incidence of pests and diseases, supply of nutrients, and response to climatic factors can be studied by detailed monitoring in experiments having relatively few experimental treatments, as done by the multidisciplinary yield variation group at Broom's Barn (Milford *et al.*, 1980). The factors being tested may be combined in packages, as in ADAS tests of farming systems, but it is then very difficult to discover which elements in a package are responsible for the overall effects (Evans, 1978). Alternatively the yield responses to many combinations of separate factors can be measured in factorial experiments, preferably together with detailed monitoring to show how the yield responses occur. The joint study of the measurements made by the scientists in different disciplines adds an interdisciplinary element to what would otherwise be just a multidisciplinary project. This was the approach selected by the Rothamsted wheat group. A multifactorial design testing eight factors each at two levels, as used previously by McEwen *et al.* (1981), was chosen. The distinction between a factorial and a multifactorial design is not clear to me. It is probably generally accepted that in the latter the number of factors tested is large compared with the number of levels of each factor, and there is no replication because the large number of plots makes it unnecessary or impracticable. As these multifactorial experiments were also multidisciplinary, rarely more than two factors were devoted to the same type of treatment, though during two years three factors were concerned with nitrogen fertiliser. But this does not seem to be an essential feature of multifactorials.

Table 2. *Factors investigated in the Rothamsted multifactorial experiments on winter wheat and, in brackets, the number of years in which studied*

Precision or normal sowing	(1)
Oats or barley as previous crop	(3)
Sown in September or October	(6)
Amount of nitrogen	(6)
Time of nitrogen application	(5)
Nitrogen given in one or three lots	(3)
Irrigated or not	(6)
Growth regulator given or not	(3)
Control of:	
Nematodes	(6)
BYDV	(6)
Aphids	(6)
Leaf diseases	(6)
Take-all	(3)
Stem based diseases	(3)

The Rothamsted wheat experiment was a 'half replicate' in which only half of the possible 256 treatment combinations of the 2^8 design were applied to plots. The number of factors investigated was increased above eight by changing the factors tested between seasons, testing restricted combinations of some factors on extra plots, and using a few packages of treatments (Table 2). Careful monitoring was used to discriminate between the components of the packages, e.g. the effects of a pesticide treatment consisting of aldicarb in the autumn plus pirimicarb in the summer were explained using measurements of the number of aphids on the crop in the autumn and summer, the incidence of barley yellow dwarf virus and the numbers of nematodes found on the roots and in the soil.

Brief descriptions of the methods used and the results obtained in these experiments have been published annually in the interdisciplinary section of the Rothamsted Annual Report. Fuller accounts are in Prew *et al.*, 1983; 1985.

Objectives of Rothamsted's multidisciplinary wheat experiments

The most important objectives were to learn which factors restricted yield of winter wheat, their relative importance and whether they interacted with each other. Most of the experimental treatments tested things that were under the control of the farmer, but they were not necessarily applied as would be recommended in practice. Fungicides and pesticides were applied even when the incidence of the disease or pest was small in order to test, or confirm, that such slight infestations had little effect on yield. The likelihood of some other treatments being used in practical agriculture was slight. For example, irrigation was applied to study the effects of drought on yield and on the responses to the other treatments. In an applied subject like ours it is often difficult to decide the proper balance between what is useable in current practice and what is scientifically interesting but uneconomic. Too little attention to the former may mean no or only slight useful outcome to the research and insufficient interest from the customer. But judging by past experience sacrificing science to current economics is very likely to prevent research that will be useful in the future. Few would have forecast thirty years ago when D. J. Watson and I were attempting to increase the longevity of cereal leaves by applying nitrogen in sprays (Thorne & Watson, 1955) that spraying cereals after flowering would become common practice.

Another objective was to get some idea of the maximum grain yield attainable at Rothamsted in a particular year with the selected variety. Hence the upper level of each of the tested factors and the basal treatments were chosen to ensure that, as far as possible, nothing under the experimenters' control was restricting yield. This was desirable because the responses obtained to the experimental treatments could be greatly affected if some factor other than the weather were restricting growth, and because the agricultural relevance of the experiment depended on large yields being obtained with the best treatment combinations. The experiments were done at a time when commercial yields were increasing rapidly and when there was a lot of interest in large yields, due partly to the publicity generated by the ICI Ten Tonne Club. Before 1979 some people thought that yields as large as the best commercial ones were unobtainable on the Rothamsted farm. Our results proved otherwise. Achieving the maximum possible yield was a less important objective than has sometimes been suggested (Cooke, 1981; Tinker, 1984). Higher levels of many inputs would have been more appropriate in a 'maximum yield experiment'. But the causes of differences in yield would have been more difficult to understand if, in order to guarantee maximal responses, a lot of treatments had been applied in excess of what was necessary.

Learning about the causes of effects on grain yield was an important objective. By monitoring the incidence of pests and diseases and measuring development, growth and nutrient uptake the sponsors aimed to get some understanding of the mechanisms involved. In this aspect the multidisciplinary experiment became interdisciplinary. We sought to establish some general principles by studying when during growth the different factors affect the crop, how growth and nutrient uptake interact with each other, how pests and diseases affect growth and nutrient uptake and are affected by differences in growth and nutrient status. These principles could then be used to extrapolate from our particular results to, for example, other sites and seasons or to other agricultural situations resulting from economic changes.

Another use of the multidisciplinary experiments has been to provide data for the construction and validation of the AFRC Inter-institute wheat model (Weir, Bragg, Porter & Rayner, 1984). Had use in mathematical modelling been a primary objective, the experiment would have been very different with more levels of the factors of interest to the modellers, fewer plots without good control of pests and diseases, more true replication, and more frequent and detailed measurements of growth. The modelling group was set up in response to a recommendation arising from early discussions in the AFRC Inter-institute Yield Variation Programme (Tinker & Widdowson, 1982; Lester & Prew, 1983). This programme was initiated in 1977, at the same time as the Rothamsted wheat group was planning the first experiment. It was not considered desirable to alter the experiments later to meet requirements of the modelling group, but some extra measurements were made on sub-sets of plots protected against pests and diseases. The experiments did provide data in six years from crops whose yields were not restricted by shortages of nitrogen or water, as judged by the responses to the nitrogen and irrigation treatments, which could be compared with the simulated crops also lacking these limitations. To the disappointment of the modellers, the experimental yields were somewhat less than the maximum ones recorded in Great Britain (Weir *et al.*, 1984).

The Rothamsted experiments have come to be thought of as the 'core' or standard with which contemporary experiments done on contrasting sites can be compared (Lester & Prew, 1983). However the Rothamsted wheat group can take little of the credit for the investigations of the causes of variation in yield between different sites done between 1979 and 1984. These derive directly from similar experiments done at Rothamsted and Broom's Barn in 1971–73 by F. V. Widdowson and P. J. Welbank (Welbank, Taylor & Widdowson, 1974). However, the 1979–1984 Rothamsted experiments provided good data that are being used for comparison with data obtained from experiments at Woburn (Tinker & Widdowson, 1982), Saxmundham (Widdowson *et al.*, 1985) and on several commercial farms (Darby, Widdowson & Hewitt, 1984).

Multidisciplinary experimentation: problems and disadvantages

Before elaborating the many advantages of working with people in other disciplines, the disadvantages should be mentioned. In many ways multidisciplinary experiments are difficult to do. This matters because bad experiments rarely produce good science. Problems arise because of the individualistic nature of many research scientists mentioned already. Thus scientists accustomed to working only on their own experiments may resent having to agree times and methods of treatment application and measurements in advance with their colleagues and perhaps having to help sample plants for growth measurements when they do not realise immediately how useful these data will be to all sponsors. A good convener can do a lot to overcome such problems. Worse are those that can arise when there is a strong departmental structure, based on disciplines, and work in an inter-departmental team is seen to be competing for resources of money, time and labour with purely departmental work. Interdisciplinary effort may even get less credit than equally worthy work done entirely within a department. Such competition may increase as resources are diminished by cuts which is unfortunate as multidisciplinary experiments can use resources very efficiently (see p. 211). Successful multidisciplinary research requires commitment from the individual sponsors and from their superiors.

Multidisciplinary experiments tend to have many small plots and still occupy a lot of land – 182 plots in 1·4 ha in the largest Rothamsted experiment. The presence of small plots and many paths may require the use of special machinery or labour-intensive methods of treatment application or harvesting. The design is unsuitable for very detailed studies: the plots are too numerous and too small for a lot of frequent observations; selection of only a few factors for detailed observation is hindered by small plot size, no or little replication, wide separation of contrasting plots; the number of levels of any one factor is limited.

The results of multidisciplinary experiments are difficult to prepare for publication. Much of their value would be lost if not presented in a single paper to show the comparisons and interactions between factors. But the amount of data is so large, especially when several years are involved, that much detail has to be left out. Moreover writing a paper with nine other authors is no easy task. Having got a paper published, the authors are faced with the problem of obtaining recognition for themselves and their support staff. Getting a paper with ten authors accepted is possible, but with twice that number of support staff added is unlikely. Moreover persuading promotion boards of the value of a tenth part of even the most concentrated 27-page paper will be difficult. Each person's contribution might get fairer recognition if the results were published as a joint superficial introductory paper followed by separate ones dealing with each discipline in more detail. However this could obscure the benefits of integration, the detailed papers might not be considered sufficiently original, and publication of the results could get unacceptably delayed. Already many interim reports of the Rothamsted results have appeared while the sponsors have been busy doing the experiments and condensing and coordinating the results.

Like all large projects and organisations, multidisciplinary experiments are at risk from disasters. These could be to the experimental crop or to the project, for example from sudden changes in policy regarding priorities. Some, at least, of an equivalent number of separate experiments might escape.

Advantages of multidisciplinary experiments

There are a lot of advantages of a multidisciplinary multifactorial design that relate to the interactions between the tested factors. These are discussed later. But even if the interactions are few or small a single multidisciplinary experiment has many advantages over a collection of separate experiments.

Use of resources

The execution of the experiment uses resources more economically. One set of plots and one lot of treatment application serve the parallel studies of the different scientists. Moreover many different measurements can be made on one lot of crop samples. For example the 0·8 m^2 samples dug from each plot for physiological studies of growth can be sub-sampled after weighing to provide data on nutrient uptake, severity of take-all and eyespot infection and the incidence of stem borers. An experiment designed solely for a physiological study, such as the interaction between sowing date and time of application of nitrogen fertiliser, would need as a service: estimation of a suitable amount of nitrogen, the application of nitrogen fertiliser, plant analyses to measure the uptake of nitrogen, advice concerning pests and disease control and the application of the necessary control treatments. In a multidisciplinary experiment the physiologist gets all this help automatically from the other sponsors as part of their scientific input. Because of its large potential value, a multidisciplinary experiment may be given a higher priority for agricultural operations than would smaller experiments. If the project is in favour, some additional resources may even be allocated to the experimnt, above those that each sponsor can contribute.

Size of responses

The results also are more valuable than those of a number of separate experiments, even when interactions are absent. The relative size of effects of different factors can be measured. Thus the Rothamsted experiments showed that controlling disease was more important than applying nitrogen at the right time. Fungicides to control leaf diseases increased yields significantly in five years out of six, the response ranged from 0·2 to 1·7 t ha^{-1}. Absence of take-all, obtained by using oats rather than barley as a previous crop, had an even larger effect, increasing yield in each of the three years it was tested by between 1·9 and 3·2 t ha^{-1}. In contrast, giving the main lot of nitrogen in early March instead of mid-April affected yield significantly in only two years out of five, with yield being increased by 0·4 t ha^{-1} in one year and decreased by 0·5 t ha^{-1} in the other.

The multifactorial design permits very small effects to be detected if interactions of the factor concerned with others are small or absent, because the comparisons involve so many plots. Thus in the 1979 Rothamsted experiment the effect of aldicarb applied before sowing persisted to increase grain yield from 9·52 to 9·77 t ha^{-1} (means of 64 plots, S.E.D. 0·10). Similarly it was shown that chlormequat definitely did not affect yield in 1982 and 1983 (with 9·01, without 8·98 t ha^{-1}, means of 64 plots, S.E.D. 0·06), whereas in 1984 when it decreased lodging it increased yield from 10·34 to 10·52 (means of 32 plots, S.E.D. 0·07). These results are interesting in view of persistent reports that chlormequat increases yield of semidwarf wheat in the absence of lodging. But experiments are rarely designed with the aim of showing that the treatments tested have no effect. An absence of response, as for example can happen in tests of pesticides when the target organism fails to appear, usually means that the effort involved in setting up the experiment is considered to be largely wasted. However, when there is a similar occurrence in a multidisciplinary experiment the motivation to continue the measurements remains and there is the opportunity to show that the pest control treatment has no effect in the absence of the target organism. Moreover, an absence of response to one factor can provide some useful replication The value of the Rothamsted experiments scarcely suffered because the aphid control sprays had no effect in 1980 and 1981 and the leaf disease sprays had none in 1984 (all effects less than 0·13 t ha^{-1}).

Application of results

A valuable property of multidisciplinary experiments is that effects of one factor may be shown to occur across a wide range of other factors that produce crops differing in type and yield. This gives confidence in the wide applicability of the agronomic results and derived scientific principles that could otherwise be obtained only by repeating the tests in many different situations. Many of the significant effects of main factors found in the Rothamsted experiments occurred at all levels of the other factors because interactions were relatively scarce and rarely altered the direction of a response, but only its size. Nevertheless it is these interactions that provide most of the advantages of the multidisciplinary multifactorial design.

Table 3. *Grain yield, concentration of N in grain, N uptake by grain + straw, and infection of the flag leaf by septoria in winter wheat given 80 or 150 kg N ha^{-1} and unsprayed or sprayed with fungicides in May and June 1981*

	With fungicide		Without fungicide		S.E.D.
kg N ha^{-1}	80	150	80	150	
Grain yield, t ha^{-1} 85% DM	9·2	9·1	7·8	7·2	0·10
Grain N% DW	1·70	1·94	1·79	2·02	0·022
N uptake, kg ha^{-1}	175	220	164	192	3·9
% flag leaf infected with septoria, 20 July	0·6	1·6	10·7	17·8	1·85

Interactions

Experience has shown that the interactions between factors can be understood quite easily because relatively few of the 28 possible two-factor interactions and the 56 possible three-factor ones have been statistically significant. Explanations for most of these have been deduced from measurements made by the various sponsors. Three examples of agronomically interesting interactions illustrate the kind of information obtainable. In 1979 fungicide without aphicide increased yield by 0·8 t, aphicide without fungicide by 1·1 t, but both together by 2·3 t ha^{-1}, giving the farmer an extra 0·4 t grain for no extra cost. However the effects of aphicide and fungicide on yield were close to being multiplicative, which is what would be expected if leaf diseases and aphids independently decreased the amount of effective leaf area.

Results obtained in 1981 illustrated the benefit that can be got from applying more nitrogen than necessary to obtain maximum grain yield provided leaf diseases are controlled. When fungicides were used, an extra 70 kg N ha^{-1} did not affect grain yield but increased grain quality, as measured by the concentration of nitrogen in the grain, because uptake of nitrogen was increased (Table 3). In the absence of fungicides, the extra nitrogen decreased yield because septoria was worse; grain nitrogen concentration was again increased because nitrogen uptake increased slightly.

The 1982 Rothamsted experiment confirmed "field experience gained over many years" in showing that the yield loss from take-all was lessened by applying nitrogen early in the spring

(Lester, 1983). But this was not so in 1983 and 1984 (Table 4). This inconsistency can be explained by the smaller amount of nitrogen supplied by the soil in the first year, as shown by the uptake of nitrogen in late February.

Table 4. *Effect of take-all on grain yield, t ha^{-1} 85% DM (yield after oats minus yield after barley), and uptake of nitrogen at end of February, kg N ha^{-1}, of winter wheat given the main application of nitrogen in early March (early) or mid-April (late) in three years*

	1982	1983	1984
Effect of take-all (t ha^{-1} grain)			
Early N	2·8	1·9	2·2
Late N	3·5	1·8	2·2
S.E.D.	0·21	0·27	0·20
N uptake end February (kg N ha^{-1})			
Early N*	22	25	36
Late N*	14	23	26
S.E.D.	1·0	1·3	1·0

* Early N = 40 kg ha^{-1} applied in early February
Late N = no fertiliser

Interpretation

As well as explaining interactions of treatments with each other and with years and providing insight into mechanisms of yield response that allow extrapolation to other situations (see p. 209), interdisciplinary thinking helps in the selection of treatments. Treatments and observations are often selected to prove a hypothesis in the mind of the experimenter, though this may not be stated explicitly. This is generally more efficient than relying on serendipity, but can result in biased conclusions. A useful check to this tendency is provided by the interaction between sponsors who look at one problem from different view points. This can be illustrated by an example related to physiological and nutritional aspects of the Rothamsted experiments.

Physiologists at Rothamsted used to believe that nitrogen should be timed according to apical development of the crop rather than calendar date. This idea has recently received considerable publicity from elsewhere (Biscoe & Willington, 1984*a*, *b*). For many years nitrogen had been applied in our experiments shortly after the crop had reached the double ridge stage (Kirby & Appleyard, 1984) and in 1977 an experiment was designed to justify this procedure (Taylor, Thorne & Welbank, 1979). Delaying application of nitrogen until the double ridge stage, reached on 17 April when tillering had almost stopped, gave a bigger grain yield than applying it at the then more usual date of 6 March. But later results showed that we were not justified in regarding this as proof that nitrogen should generally be applied at the double ridge stage.

In the multifactorial experiments sowing date had a big effect on the timing of tillering and apical development in the spring, though not on flowering and maturity. No test of timing of nitrogen was included in the first experiment and we compromised on an application date for the main spring dressing of 6 April, shortly before double ridges were seen in the later sown crop. I was able to persuade my colleagues that we should introduce a test of nitrogen timing into the next experiment and sacrifice another factor. Nitrogen applied early, at the date when the wheat sown early reached the double ridge stage (early March), was compared with applying it late, at the date when the later-sown wheat reached the same stage (mid-

April). As this test was combined factorially with sowing date, we could compare September- with October-sown wheat when nitrogen had been applied at the same growth stage or at the same calendar date. The later application had a distinct advantage in 1980 and a slight one in 1981, mainly in the early-sown crop (Table 5), which was not what the physiologists expected. The result could be explained by the measurements of soil and plant nitrogen. The September-sown wheat absorbed sufficient nitrogen in the autumn to support growth until the late dressing was applied in April; the October-sown wheat being smaller needed less nitrogen and could also wait until April. When nitrogen was applied in March some was lost, either by leaching or denitrification, and consequently nitrogen uptake after flowering and grain growth were both less than when application had been delayed until April. This result was obtained on a soil rich in nitrogen residues in the autumn after a crop of potatoes. Hence when the late application of nitrogen was given in April, there were at least 900 shoots m^{-2} although some tillers had already died in the early-sown wheat.

Table 5. *Effect of applying most of the nitrogen fertiliser in mid-April as compared with early March on grain yield (t ha^{-1} 85% DM) of winter wheat sown in mid-September or mid-October in five years*

	1980	1981	1982	1983	1984
Sown September	0·87	0.22	−0·64	0·30	0·30
Sown October	0·06	0·08	−0·26	0·03	−0·03

Pooled S.E. of effect = 0·101

When planning the second three-year series of experiments there was some pressure from the chemists to abandon the test of nitrogen timing on the grounds that the advantage of the later timing was proven. This was resisted by the physiologists who suggested that when the preceding crop was changed to a cereal that left only smaller residues of nitrogen the early-sown wheat might need early nitrogen. In the first year this proved to be so, although the later sown crop also benefited slightly from earlier nitrogen (Table 5). The physiologists' triumph was shortlived as the next two years produced results very similar to those obtained in 1980 and 1981. The benefit from early nitrogen in 1982 was clearly related to the very small amount of nitrogen in the soil in that season as already mentioned.

The joint interdisciplinary conclusion regarding nitrogen timing was that at least four factors could be involved:

(1) The development of the crop, especially in relation to tillering. If maximum tiller number would be insufficient without added nitrogen, some should be given during tillering. Otherwise nitrogen should be used to retain tillers. Apical development is a convenient way of recognising the end of tillering: tiller number usually changes little between the double ridge and terminal spikelet stages. Zadoks growth stage 30, the French "épi à 1 cm" and the start of stem extension occur shortly before the terminal spikelet stage (Kirby & Appleyard, 1984). Delaying nitrogen application until stem extension is obvious could risk uptake occurring too late to prevent excessive tiller death, especially in later-sown crops because at the time that this stage is reached the weather may be warm and hence development fast, and drought may also delay uptake. Direct effects of nitrogen on spikelet or floret number are negligible compared with effects on shoot number, dry weight and leaf area, except with severe deficiency.

(2) The amount of nitrogen available in the soil and hence the amount absorbed by the plant.

(3) The weather and the soil type. Cold weather delays uptake putting nitrogen at risk from leaching or denitrification in certain soils if there is much rain (Widdowson, Penny, Darby & Bird, 1984).
(4) Risk of take-all. Crops with severe infection that develops early may in some circumstances benefit more from early nitrogen than healthy crops (see p. 213).

Publicity

Multidisciplinary experiments, and an interdisciplinary approach to them, have advantages in relation to demonstration and publicity especially when this is aimed at interdisciplinary customers. Farmers appreciate seeing such a wide range of treatments being tested on one site, though there can be problems when plots showing interesting comparisions are not adjacent or in a convenient part of the field. The results have proved to be a good basis for demonstrations such as 'Wheat 83' (Rothamsted Wheat Group, 1983) or 'Cereals 85'. At the latter, it was particularly valuable to be able to discuss on one exhibit the consequences of different sowing dates on incidence of take-all, uptake of nitrogen, growth and development of the crop.

Physiology in multidisciplinary experiments

Physiological thinking, as practised by good agronomists as well as by crop physiologists, is very important in determining the causes of response to treatments. A physiologist may be better equipped than some other specialists to appreciate the relative importance of responses to different factors or to ones that occur at different stages of growth. However it is not appropriate that I should claim any special importance for physiology. Although I have got very interested in the pests and diseases, my main concern has been with the plots given pesticides and fungicides in relation to their nitrogen nutrition, as already illustrated, and in relation to effects of climatic factors. The effort devoted by physiologists to plots receiving other treatment combinations has been more than compensated by the physical help received from the other specialists and by the information obtained about the health and nutrient status of the 'healthy' plots. This knowledge, as well as the large yields obtained, gives us confidence that the grain yields obtained with the best combinations of treatments were restricted by little apart from climatic and perhaps also soil factors not under the experimenters' control. These yields were: cv. Hustler 1979 11·1, 1980 11·2, 1981 9·9; cv. Avalon 1982 8·4, 1983 10·4, 1984 11·1 t ha^{-1}, 85% DM. The small yield in 1982 was partly a consequence of incomplete control of disease, particularly sharp eyespot and fusarium on the ears. But the temperature and radiation during grain filling predict that yield would be smallest in 1982 and largest in 1979, 1980 and 1984. Further use of this six-year collection of data is still being made to elucidate the environmental control of growth and yield, and further publications on this aspect are planned.

Conclusion

The value of the Rothamsted multifactorial wheat experiments should be evident from the wide use made of the results, shown by the papers already published. The approach has been considered so successful at Rothamsted that a similar one has been in use with winter barley for five years and for oilseed rape for one. This personal account of a lot of detailed experimental work is intended to highlight principles which can be applied to other crops and other fields of research. I believe the co-operation between scientists in different disciplines was productive. I hope the evidence I have presented will convince others.

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