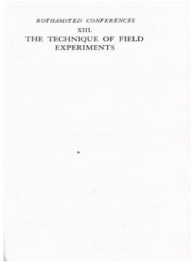
ROTHAMSTED RESEARCH

XIII. The Technique of Field Experiments

Full Table of Content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XIII.
THE TECHNIQUE OF FIELD EXPERIMENTS

The Technique of Grassland Experiments

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THE TECHNIQUE OF GRASSLAND EXPERIMENTS

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The technique of grassland experiments in twenty minutes! I can only be outrageously eclectic, and I think it will be best, although perhaps egotistical, to confine myself to a discussion of the various methods which we in Wales adopt. It is all a question of the factors we think important and the factors we wish to study.

Personally I think the biotic factor is by far the most important one influencing grassland. I mean the influence of the grazing animal on the sward; this, as far as it is legitimate to use the phrase, is assuredly the *Master Factor*. How can we study this factor? By rigid control of the grazing animal on plots representing different types of grassland. So now I come straight away to my tethered sheep; you can tether bullocks or horses if you have the will and the room. The point about tethering is you can regulate your grazing to any intensity you like and you can do so on small plots—and I shall have a lot more to say about small plots. It's simply a matter of a rule of three sum and a proper system of moving so as not to starve the sheep on the plots which are most intensively grazed. On these bioticas (a biotica is a piece of ground devoted to a study of the biotic factor) of ours our most intensively grazed plots carry the equivalent of 17 sheep per acre per day throughout the grazing season. Let me just incidentally remark, I believe in all field experiments of a research nature we should go at each end far beyond what are deemed by practical men to be economic limits.

A few words on the technique of managing tethered sheep. We use the Scandinavian chains—about 10 feet long, then bifurcating into two lengths of 2 feet each, two sheep being tethered together. The sheep must be moved twice a day—half a length at a time—or they will graze the periphery of their circle to death. They must be given water during appreciable periods of drought—we use ordinary garden saucers. It is desirable that they should be given shelter when tethered in exposed situations, *e.g.* at 1000 feet on the Welsh hills—how to do this cheaply and efficaciously is a problem we are still working at; and I am open to suggestions.

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The plots when a chain wide must go with and not across a hilly field or the sheep will always camp on the higher ground and spoil the plots. For biotic purposes we have trained even two to four-year-old Welsh mountain wethers to tolerate the tether chains—for other tethering experiments to be discussed later we use yearling lambs twelve months old at the commencement of the experiments.

We are investigating six intensities of grazing on five sharply contrasting types of grassland on plots of no larger than about $^1/_{20}$ acre.

So far, you will understand, we are simply using our sheep as controlled defoliating, treading, urinating and excreting machines, and we are not in the least interested in the sheep *qua* sheep.

The control of the sheep represents but one aspect of these ecological investigations, the final results are given by the botanical analysis of the plots—botanical analysis, the bugbear—at least so people think—of all grassland investigations.

Again it is all a matter of what you want, and generally what is wanted is a reliable comparison of the type and degree of such differences as have significance *vis-à-vis* the ultimate welfare of the animals that are turned to graze particular fields.

On physiological—nutritional—and morphological grounds, which I have no time to discuss, the elements which, broadly considered as such, matter in the flora of a sward, and which in the case of many types of investigation it is sufficient to categorise, are (1) clover contribution; (2) miscellaneous weed contribution; (3) contribution of bent and (or) fine-leaved fescues considered as a unit; (4) contribution of Yorkshire fog and (or) soft brome considered as a unit; (5) contribution of that particular species of so-called valuable grass, if any, which may contribute in large amount, say over 10 per cent. to the sward—*i.e.* usually perennial rye-grass, and (6) contribution of other grasses considered as a unit. Unless your treatment, no matter what it be, has substantially influenced the adjustments as between these five or six groups as such—I doubt if it has any economic significance worth considering and still less worth talking about—it is only for quite exceptional purposes that one wants a botanical analysis accurate for all the species as such to the limits of 1 or 2 per cent.—and if you do want it I doubt if it is ever worth the trouble of getting.

There is a tremendous food value difference, thinking in terms of all-the-year-round grazing between the fine-leaved fescue-bent unit and the rye-grass-meadow fescue-cocksfoot unit and a still greater difference between these units and the clover unit, and it is the interplay of these units that, from the botanical point of view, primarily matter and which can be comparatively accurately estimated.

In regard to botanical analysis there are two distinctions we always want to hold clearly before us. Do we want to know what the ration actually offering to the animal is day by day and week by week, or do we want to compare the slow ecological changes which under some series of treatments show themselves on contrasting plots? In the case of my bioticas I am interested only in the ecological changes. For these purposes I am in favour of analyses made by lifting representative turfs and counting the plants. We usually lift ten turfs per plot of $^1/_{100}$ acre—or on larger plots per quadrat of $^1/_{100}$ or $^1/_{500}$ acre. We now lift rectangular turfs of size equal to 6 ins. $\times$ 6 ins. or 1 ft. $\times$ 1 ft.

You can count tillers or you can count plants—if the latter, for each species you have to carry a standard for each of what constitutes a plant—we are out for comparative data—and it does not much matter what your standard is as long as it is rigidly adhered to. It is true that tiller counts favour the multi-tillered species—rough-stalked meadow-grass for instance. But this does not in the least matter—the man or woman who is competent to research on swards is competent to put the proper interpretation on the data so obtained—and as far as I see it that is all that data are for.

Your trained and reliable pasture workers—and you can identify such persons by their personal technique in the matter of taking up a horizontal position on a sward, and in the matter of handling herbage—are competent in appropriate connections to dispense with these laborious counts and instead can estimate *in situ* on the plots to an absolutely sufficient degree of accuracy for a very large number of practical purposes—and practical purposes are our aim. Personally I always marvelled at tea-tasters, wine-tasters and wool-sorters, while the Bobby Jones's and Lindrum's of this world are further proof of the capabilities of the human eye and of human judgment. It is all a matter of what you want your data for and the order of differences you are dealing with.

Now we come to the question of studying the day-by-day interplay of the sward and the animal, grassland research *in excelsis*. Personally I do not like unqualified hay data as an aid to formulating opinions as to grassland management—such data leave the animal out of the question (and all meadows are grazed), while the growth of hay covers only a part of the year—and when precisely is the gate shut on a hay field? A most decisive factor this, as influencing the interplay of species—a piece of information which is far too seldom given in reports on hay experiments; while, incidentally, let me remark the influence of gate shutting can only be investigated critically by resort to folding or tethering.

With studies on the day-by-day interplay between the animal and his herbage it is again a question of control of the animal and of botanical analysis. Obviously you must control the animal, because you cannot take reliable samples of what the animal is eating or to give you pasture yield on fields ordinarily extensively grazed. True, you can put wire mesh cages over quadrat areas and sample these on any basis that seems good to you. This method has been employed in America—we have tried it but think our method of controlling the animal preferable. In brief our method is to sample in front of the controlled grazing animal.

The animal can be controlled by tethering and this is the method we are now using to an ever-increasing extent.

At first we used and are still to some extent using, penned plots in size varying from $^1/_{100}$ to $^1/_{50}$ acre—plots of the order of $^1/_{10}$ acre only being occasionally used. When using plots one to four sheep are introduced according to the size of the plot and the amount of eatables offering—24 to 72 hours completes the grazing. Whether we use plots or tether we usually graze and therefore sample on a three weeks' rotation.

To obtain yield data we sample within a rectangular mesh equivalent to a square foot on small plots, and a square yard on larger plots. We take at each grazing ten samples per $^1/_{100}$ acre plot—or in the case of more extensive work, five samples to a quadrat of $^1/_{500}$ acre. We cut hard to ground level with sheep shears within the mesh. You must sample by this means after each grazing—this gives what the animals have left—and sample again before introducing the animals for the subsequent grazing. Thus for example the weight given by the post-grazing sample taken on 31st March, subtracted from the weight given by the pre-grazing sample on 21st April will give you the yield of herbage produced during the first three weeks of April. Note, firstly, you cut as low as is possible —lower than the animals graze. Note, secondly, you are sampling, and therefore you are not subjecting your whole plot at each grazing to drastic mowing machine-wise defoliation; and *note thirdly*, you are causing your plots to be trodden, urinated and excreted upon. After the post-grazing sample we spread the droppings.

Ideally it would be nice to give your yield data as oven-dry fodder, but owing to limitation of time and facilities we only do this in the case of very special experiments. Normally we make what is tantamount to hay in scrim bags and give our data as air-dry fodder. We weigh in a fine dry day after a run of five dry days.

This is the point—grassland problems are essentially complex problems, and in order to enunciate helpful dicta relative to the economic management of grassland you have got to understand the interplay of innumerable factors. You have therefore to deal with a prodigious number of plots and of samples and you are usually dealing with differences of considerable magnitude. The only thing that matters is whether your data ellucidate your problem? If the answer is in the affirmative your technique is meeting its case.

Now for botanical analysis again, and this time the analysis of the ration the animal is eating. We quarter down our samples to 1 lb. During the commencement of these researches we actually separated all these herbage samples—and they have run into thousands—into the appropriate categories; and it is literally marvellous what well-trained girl assistants can do in this respect even with short pasture herbage.

Note in passing, please, that not even the best trained girl can analyse the mince-meat type of herbage delivered by a garden mowing machine. Now that we have all served a long apprenticeship—we of the research staff and the senior girls are largely adopting Mr. William Davies' admirable scheme of estimating. You divide your little 1 lb. heap of herbage into ten approximately equal heaps. You give ten marks to each heap. Then for each heap allocate out the ten marks between your categories—noting "trace" against the category represented but not worthy of a mark. Adopting this means a properly trained person will get a percentage by weight contribution for the categories as near as quite literally does not matter to that obtained by all the laborious separations and weighings. The people concerned have tested themselves repeatedly. It is not too much to say that when we had the courage to adopt the Estimating Method as part of the regular technique employed at the Station, by the stroke of a pen, as it were, we were able to give a tremendous impetus to all our endeavours, and I think we are now definitely at grips with the grassland problem. We have, in short, developed a technique based on a method of controlling the animal, a method of sampling and a method of obtaining botanical data on a very large scale which is making it increasingly possible for us to study the interplay of the innumerable factors involved.

A word as to live weight increase. We are interested in this because we want to test our pedigree strains and because it is an important aspect of the grassland problem.

To get the best results I am certain you want rigid control of your animals. You may either rotationally graze over small folds, or you may tether—both methods are infinitely superior to merely dividing off large plots of a sufficient size to carry a sufficient number of experimental animals as one extensive unit. Our experience, and it now goes over three seasons—but mark you, I am only talking in terms of sheep—is overwhelmingly in favour of tethering. Such is our faith in tethering that we are setting up an experiment this year to test rye-grass against cocksfoot that will entail the use of forty-eight pairs of tethered sheep. Here are the advantages of tethering: (1) you can replicate your plots sufficiently well, I should imagine, to satisfy Dr. Fisher himself; (2) your experimental sheep will be handled and examined twice a day—they will be properly looked after; (3) your grazing will be uniform—soil inequalities will therefore count less; (4) your carrying capacity of thriving animals will be increased per unit of area, and therefore your number of experimental animals per unit of area can be greater than under more extensive methods. Statistically what a blessing this is to those of us who have to operate on a limited area of ground, or who are dealing with pedigree strains of grasses of which seed is necessarily limited, or for that matter to all who appreciate the multiple factor aspect of grassland problems.

I would like, in conclusion, to say three words about small plots. I have said the grassland problem is a complex problem; it is largely an ecological problem, and although eventually no doubt we shall be able to pack an increasing number of its sub-problems into the laboratory and the greenhouse, it will always remain in its wider and more definitely economic aspects a problem that will lend itself to a marked degree to that type of mind which has about it a considerable streak of the naturalist—a turn of mind which has an outdoor rather than a laboratory way of looking at things. I like the small plot not only because it is an adjunct to adequate replication for statistical purposes, but because it makes you look at it as a whole—makes you concentrate attention upon it—and thus the greater your number of replications by that much the deeper and more intensive your contemplations. In grassland studies you must collect data, and you want data of a degree and precision adequate to your term of reference; but I believe the problems are solved just as much—indeed I am heterodox enough to say perhaps more—by what one notices in the field when collecting the data as by the actual yields or other precise—but usually end-stage—results which the analysis of your data gives you.

If I may detain you one more minute, I would like to say a word as to control plots and a word as to lay-out. In seeds mixture work the ideal control—and a most valuable plot—is your "no seeding" plot, and such a plot should, I think, always be generously introduced if necessary as an extra control. For one thing it is likely to tell you from where a bit of your wild white clover has come from, and it is the basal plot relative to competition—weeds have so much to do with it. As to lay-out I would like to emphasise the enormous amount that is to be learned by a "one issue" experiment carried over the widest possible area and the greatest possible diversity of conditions. For this purpose I like a strip, say a harrow or two harrow width, carried right through the middle of a field and if possible on and over contiguous fields. Two controls and your treatment—up hill down dale, over every sort of soil. We have quite a number of these, our largest being nearly a mile long, in connection with our work on the open hills. What, for example, would not a basic slag strip right across England have taught us, and I would like to join up Rothamsted and Aberystwyth with a single mixture strip.

THE TECHNIQUE OF VARIETY TRIALS

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Introduction

This is rapidly becoming an extensive subject, and as our programme to-day is lengthy I shall confine myself to such points as I feel are especially worthy of emphasis. These points are largely the outcome of experience gained by the staff of the National Institute of Agricultural Botany while engaged in actual field trials.

The first essential for those engaged in this work is that they should realise something of the difficulties and complexities of their task. We are dealing with living organisms. We are setting out to get a measure of their output—*yield*—itself the result of a large variety of causes. We are attempting to measure that even more elusive and undefinable thing—*quality*; and besides this we have still to take into account many other " crop behaviour points " which often to a large degree modify the " crop value " in farming practice. Moreover, we are dealing with " variety trials," that is to say, with the comparison of closely related plants. Therefore the differences we are attempting to measure are not usually of a large or obvious kind. For these and other reasons a well-defined and scientific technique is essential to success.

In variety trials it is necessary that we have a suitable standard of measurement by which we may judge the comparative performance of any variety as regards yield, quality or value of produce. The best known means available to us is to place a similar organism (or mass of organisms) under similar conditions. We employ a closely related variety as a standard or control and in this way obtain a