

"MANURING."*

A TEN YEARS' RETROSPECT.

T. EDEN.

INTRODUCTION

This week sees the tenth anniversary of the organisation of a department of Agricultural Chemistry in the Tea Research Institute, coinciding with my arrival to take up my appointment. In response to your Chairman's invitation to address you on the work of this department I have chosen to give you a retrospect of the manurial history of tea during that period. Much of what I have to say will be familiar to you, but memories are proverbially short and few of us would be prepared without looking up records to say precisely what has happened in the last decade.

This subject of tea manuring, well worn as it is, will I hope give me an opportunity to do two things. It will let me try to give you some picture of how research work is planned and carried out, and it will allow me to answer, as far as my department is concerned, a published statement made in London to the following effect:—

"What we want to see from our Research Institute are practical results, and economic benefit for the expenditure involved. Up to date these are lacking"

THE STATUS QUO

One of the first matters to which I turned my attention ten years ago was the compilation of a recipe book of manures, to include every formula I could lay my hands on. In the near future, I should be called on to advise on manuring and to work out a scheme of experimentation, and I was seeking a datum line from which to work. I soon found that there appeared to be few, if any, guiding principles, on the quantitative side, that welded together into an intelligent whole the immense variety of prescriptions in vogue. It would hardly be an exaggeration to say that any combination of amounts from 10 to 40 lb. per acre for all the three standard nutrients, if picked at random out of a hat, would have been found to correspond closely to examples in actual use.

* A lecture delivered to the Sabaragamuwa Planters' Association on October 22, 1937.

Looking through the published literature at that time and through the original correspondence files of my department, I find that the following were some of the statements and arguments on which manurial prescriptions were presumed to be based:—

- (1) Nitrogen was necessary for plant growth, but not too much, or quality would be impaired.
- (2) A considerable proportion of the nitrogen should be in the form of 'organic' manures such as blood, oil cakes or fish offals, which, by reasons of slower availability would last longer, cause no rush of crop, and not impair quality. Such precautions as these might be relaxed in pruning mixtures because rapid growth was desirable in tea recovering from pruning and quality was no consideration in tipping leaf.
- (3) By having a number of constituents providing nitrogen, all with varying availability, the latter could be put onto the basis of a relay race and ensure even growth.
- (4) Potash was sometimes held to be unnecessary in manures because of the adequacy of soil supplies; it was sometimes given in substantial doses as an aid to wood growth.
- (5) Phosphoric acid has been suggested as a factor in quality, but its chief function according to the ideas of the time was to stimulate root development. About this time it became prominent in pruning mixtures in quantities from 50 to 70 pounds per acre, and in the Institute files is an example of a pruning mixture containing no less than 300 lb. of concentrated superphosphate or 126 lb. of phosphoric acid per acre.
- (6) Pruning mixtures were distributed and forked in as soon after pruning as possible to ensure rapid recovery and healing of the pruned cuts. For this reason although the distinguishing feature of pruning mixtures was high phosphoric acid, nitrogen was also added.
- (7) Pruning mixtures and general mixtures were generally different in both ratio and constituents, and when a general mixture was given twice in a cycle there was frequently some difference between the constituents of the two dressings.
- (8) There was a vague fear lurking in people's minds that acidity would be promoted by certain artificial manures and that

this acidity, as in the case of temperate crops, would be detrimental. Basic manures such as Basic-slag were in moderate use.

If time permitted I could substantiate these items over and over again from my files, but I must merely ask you to use your own recollection and will content myself by giving two examples out of many. In a *Tropical Agriculturist* article in June, 1931 the following statement occurs:—

"One method of reducing cost is to substitute cheap inorganic nitrogen for more expensive forms, but it must be remembered that inorganic nitrogen, while it gives a rise in crop over a short period, tends to lower quality of the tea produced."

The second example is a manure mixture which contains in the following 9 constituents 7 sources of nitrogen:—

Groundnut cake.	Steamed bonemeal.
Whale Guano.	Sulphate of ammonia.
Bloodmeal.	Nitrate of potash.
Castor cake.	Mineral phosphate.
	Muriate of potash.

EXPERIMENTAL APPROACH

Most of the statements in this somewhat formidable list were capable of experimental verification, so the next stage in the enquiry was to look for confirmation in published records. There were but two sources of information, the experiments on Pittakande made by the late Joseph Fraser, and those of the Department of Agriculture. Both these sets of experiments gave highly anomalous results, such as the plot with the least manure in the nitrogen series giving the highest yield but one in the Pittakande experiments, and the complete fertiliser plot being bottom but one in the Peradeniya series.

It was evident that my department would have to make field experiments its main concern, and that work would have to start at the very beginning. There was no doubt that the disappointing results from previous experiments was due to their lack of accuracy. It was essential to discover why experiments were inaccurate and whether anything could be done to improve them. Was it possible to assess experimental accuracy and could experiments of known accuracy be interpreted objectively, i.e., so that other workers would draw the

same conclusions from the same results? These questions had been engaging people's attention in Europe and England and the broad principles that would have to be adhered to in order to achieve what the scientist calls accuracy or low error, and what I will call representativeness, had been worked out, notably by Professor R. A. Fisher. It had been my good fortune in conjunction with Fisher to make the first practical test of these principles, and it was accordingly possible to start work on similar lines even before the results had gone through the somewhat lengthy process of being written up and published in the scientific journals for general information. The Tea Research Institute accordingly 'got in on the ground-floor.' For two years before the Institute acquired an estate, problems of field experimentation occupied the department's attention with the result that, when actual agricultural experiments were started, it was possible to ensure that differences between treatment yields as small as 6 or 7 per cent should be reliably interpreted.

From the point of view of advisory value many experiments fail in scope if not in accuracy. By keeping the treatments small in number, certain advantages making for accuracy may be gained. The demonstration of the effect of the presence or absence only of a single nutrient has however a distinct limit in usefulness, and as quantity was an important consideration in the list of 'principles' previously stated, it was desirable that varying combinations of manures and quantities should be tried.

From the questions so freely discussed, three were chosen as a start. These were: what is the effect of nitrogen in three increasing quantities at three different levels of potash; what is the effect of potash in three increasing quantities at three different levels of nitrogen; and how do inorganic sources of nitrogen compare with organic? That was as far as I considered we could go at that time; phosphate was kept constant as it was not then known how to vary phosphate too without invalidating the accuracy of the other portions of the trial.

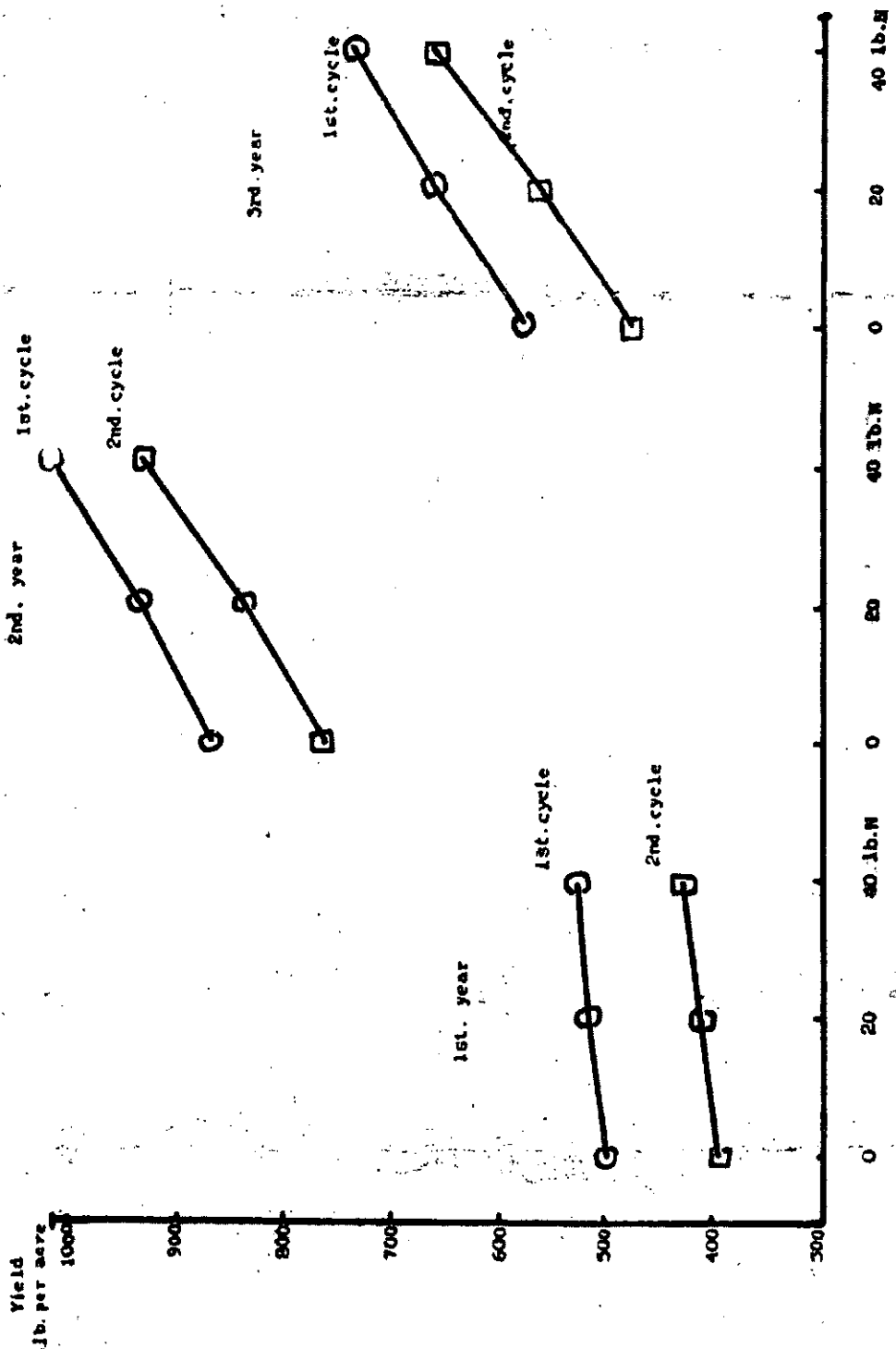
At the end of the first cycle, owing to work done in the meantime, it had become possible to extend the experiment to different quantities of phosphate too, so that altogether the experiment demonstrated 27 different combinations of quantities of manure and three different types of nitrogen. In our first ten-year plan we have covered the behaviour of the three main nutrients and systematised the knowledge regarding them. Let me try now to answer some of the statements on which this exploration was based,

EXPERIMENTAL RESULTS.

The nitrogen response has been demonstrated beyond dispute, as is shown by the diagram of yields obtained in the first two cycles. There is here no question of the lowest quantity of nitrogen coming out near the top of the list. What is striking is that there is a quite definite proportionate response of yield to quantity of nitrogen in the presence of the other nutrients: there is so far no sign of the operation of the law of diminishing returns which postulates that the return from each successive increment of nutrient brings diminishing increments of crop and consequently of profit. As far as yield is concerned there is no doubt, under normal conditions, of the economic benefit of 40 lb. per acre of nitrogen. Provided that the dearest unit of nitrogen is not used there is a profit even making full charges for the manufacture of the extra crop, but as is a common experience, the cost of harvesting and manufacturing extra crop is rather less than check-roll averages owing to the more beneficial spread of some of the overhead charges in the cost of production.

Closely connected with my subject are the results of the experimental manufacture of the Biochemical Department which show that this extra yield has been achieved without the sacrifice of quality. At the end of six years' cumulative effect Colombo finds in 17 valuations an average difference of less than one cent in price in favour of no nitrogenous manuring. This amount is due almost entirely to one extraordinary favourable value which London failed to confirm. On the same series of teas, London found in favour of no nitrogen to the amount of 1/12th penny.

The experimental evidence on the question of 'organic' nitrogen is equally clear. At the end of six years the excess yield due to sulphate of ammonia, an 'inorganic' manure, in comparison with blood, and 'organic' compound, amounts to only 2.58 per cent of the yield. This has not been accompanied by sudden rushes of crop after the application of 'soluble' nitrogen, followed by a compensating period of starvation. On the whole, the curves follow each other very closely indeed. Groundnut cake which has been tried for a shorter time lags behind sulphate of ammonia by 4.3 per cent. There is a certain amount of nitrogen in cakes and blood which is practically non-available and to this extent over an extended trial one must expect a lag in yield from this class of manure, but the rest is to all practical purposes as available as sulphate of ammonia. This is supported by laboratory nitrification experiments and by what we know of the carbon-nitrogen ratio of blood and cakes. The relay-race hypothesis thus loses its significance. Nor is there reason to



fear the acid-producing properties of commonly used inorganic fertilisers. Experimental proof has been forthcoming that tea definitely prefers an acid soil and will tolerate none else. The quality valuations bear out this new conception. In the course of 15 manufactures, Colombo found sulphate of ammonia superior by an average of one cent compared with bloodmeal and half-a-cent compared with groundnut cake. London also put the value of sulphate of ammonia higher than the other two but reversed the order of preference for bloodmeal and groundnut cake. The superiority was expressed in exceedingly small figures, $1/22d.$ and $4/22d.$ for blood and cake respectively. In terms of practical value these figures merely confirm the equivalence of the various sorts of nitrogen, though the Colombo figure actually shows the difference in favour of sulphate of ammonia to be significant.

Turning to potash it is evident from the close similarity of the yields that potash response under ordinary conditions is negligible. The maximum difference between potash and no potash is under 2 per cent, but although yield shows no signs of falling-off there is some evidence of loss in quality. The Colombo results give no conclusive sign owing to one highly abnormal figure but London gives a consistent preference of $.64d.$ for teas having potash as manure.

Phosphoric acid has behaved in a very striking manner. The doses used were 30 and 60 lb. per acre. The response to phosphoric acid is not great being only some 139 lb. in the three years but it is entirely confined to the lower dose: the extra 30 lb. gives no enhancement of crop. This is also confirmed by our experiments in Uva. Another sidelight given by the experiment is that when the total amount of phosphoric acid removed from the ground by the crop is determined, it is found to be markedly less than any of the other constituents dealt with.

PRUNING MIXTURES

I want to turn now to another aspect of these results. The very regular way in which nitrogen acts in promoting yield increases is too striking to be accidental, but you will notice from the slope of the graphs for the two cycles that it is only in the second and third years that it is of marked effect. You will notice how the cumulative effect of manuring is checked by pruning. These figures leave little doubt that the pruning mixture is uneconomically used. In these two cycles the production of dry matter (including tippings) during the separate years of the cycle is as follows for the addition of one pound of nitrogen.

<u>Pruning Year</u>	<u>Second Year</u>	<u>Third Year</u>
2.2 lb.	4.0 lb.	4.4 lb.

This result calls in question the whole practice of putting on pruning mixtures as an encouragement to growth. It suggests that the early growth of a tea bush after pruning is conditioned more by bush reserves than by manure. This hypothesis is obviously one which needs a specific test and in the new pruning cycle the 'pruning mixture' is being applied at 0, 3 and 6 months from pruning. So far, only the tipping growth has been harvested but the yields are highly instructive. At the time of tipping the first two times of application were already passed, the third application remains to be done. I have had no time yet to calculate the errors of these results, but the yields from plots which received a pruning mixture averaging 60 lb. of nitrogen immediately after pruning are only 2 per cent greater than those which have so far had nothing. The yields of those plots which had a dressing at 3 months are actually lower than those of both the other sets though not I fancy significantly so. It thus appears that considerable doubt is thrown on the efficacy of pruning mixtures both as regards time of application and phosphoric acid content. The remaining yields of this year will give an indication of when first year applications can best be applied: we are planning an experiment in which the pruning mixture is altogether omitted.

I think it can be fairly claimed that a considerable amount of information has already been extracted from this series of experiments and more will follow. I have spoken incidentally of the testing of different times of application: a further modification is that the level of nitrogen has been raised in the present cycle by 40 lb. for each plot, *i.e.*, we now have doses of 40, 60 and 80 lb. This will not only prevent the nitrogen deficiency on some of the plots from ruining the experiment, but will give information that is urgently needed for its own sake.

I hope I have succeeded in showing you how an experimental scheme is conceived, developed and modified. I cannot go into technical details, but one of the features of the technique employed is that these developments and modifications which arise out of results already obtained, can be made without breaking the essential continuity of the experiment or invalidating its interpretation. The addition of phosphoric acid was made in a way that still left it possible to compare the members of the nitrogen and potash series as before; the differing times of application have in no way disturbed the interpretation of the other factors; nor has the general change of nitrogen level.

PRACTICAL AND ECONOMIC BENEFIT

These results I claim are practical results; they are being used on a commercial scale on St. Coombs. On all the points on which the findings of the Institute's experiments differ from the prevailing opinions of ten or less years ago, the policy on St. Coombs is to adopt the findings of the experiments. This policy has the unanimous sanction of the Committee appointed by the Board to work out estate policy. We use inorganic manures ourselves: our phosphoric acid level is kept down to experimental limits: we have discontinued the use of the pruning mixtures at pruning time and apply it four months later when the bush has a cover of foliage developing. As experiments progress doubtless estate policy will accurately reflect that progress.

Moreover these practical results bring economic benefit. Comparing the original manuring on St. Coombs with that now being used I find there is a saving of Rs. 7/- per acre per annum exclusive of freight, which provides an additional saving of some 30 cents. If the whole expense of the Institute since its start is divided by five in order to allocate a fair proportion of expenditure to the Agricultural Chemical Department, that amount is less than the saving which would accrue to the industry *in one year* if only 20 per cent of the acreage under tea cultivation followed our principles in this one aspect alone.

There is a philosophical theory that matter only exists by being perceived. You probably know the old argument as to whether a clock strikes when you don't hear it. By paying no attention to the findings of the Tea Research Institute it is possible, using similar sophistry, to persuade oneself that the Institute has done nothing. I hope however that one result of this lecture will be to persuade you otherwise.