STATISTICAL PROBLEMS
IN PASTURE RESEARCH

BY

P. ROBINSON

Ph D. U.C.T.
1956

CONTENTS

PART I. FORAGE YIELD

PART II. BOTANICAL ANALYSIS

PART III. PROBLEMS RAISED BY THE INCLUSION OF
ANIMALS IN EXPERIMENTS

REFERENCES

INTRODUCTION

The importance of pastures in the agricultural economy of any country has been realised for some time, and is reflected in the amount of scientific literature which has been written on the subject. The methods which have been used to evaluate production and quality of pasture vary enormously in procedure, type of measurement, and accuracy, and there are few generally accepted techniques. In spite, therefore, of the interest in the subject progress in experimental procedure, and consequently in pasture management in the field, has been slow, particularly when compared with the progress made with other agronomic crops.

The first step in experimental work on pastures would therefore appear to be an examination of the various methods which have been used in the past. Fortunately this has been done by a joint committee of the American Society of Agronomy, the American Dairy Science Association, the American Society of Animal Production, and the American Society of Range Management, and, in its reports (1943, 1952) it has attempted to make a "concise statement of accepted fundamental techniques", and to advise research workers on the appropriate method for use under particular circumstances. As there are few generally accepted fundamental techniques these reports are little more than reviews of the literature and conflicting recommendations are made.

The large number of methods developed is due to the complexity of the problems associated with pasture assessment. The main difficulty, from which all others eventually arise, is due to the fact that animals graze the pasture *in situ*, whereas other food crops are harvested by man, and the produce can be weighed directly. Ultimately, therefore, it is necessary to utilise the grazing animal in order to determine the effects of various pastures and treatments. The introduction of the grazing animal into the experiment presents many practical difficulties in addition to a large increase in cost, so that initially it is necessary to use indirect methods for pasture evaluation. Ahlgren (1947) has in fact stated these latter form the majority of the methods at present used in evaluating different methods in pasture improvement.

It is necessary to consider pasture research under two main broad headings - 1) herbage yield and quality and 2) animal production and quality. Herbage yield is measured by clipping (or mowing) and weighing. This is relatively simple but there are various problems arising which require investigation and these are considered in Part I. The quality of herbage may be determined by chemical analysis, and it may also be determined by digestibility trials, but in both cases, from the statistical point of view, the techniques are fairly well established. In trying to overcome the work necessary in digestibility trials it has been suggested that the results of chemical analyses can be used for calculating starch equivalent. If this were possible there would be a considerable saving of time and effort. Watson (1939) has suggested using the expression

$$\text{starch equivalent} = 77.136 - 0.5297(p+2f)$$

where p = crude protein per cent of dry matter

and f = fibre per cent of dry matter

This equation may give a reasonable estimate under European conditions, but it certainly does not appear to hold under tropical conditions. Regression equations were calculated from data collected in East Africa, South Africa, and India. In all cases there was a positive association between starch equivalent and crude protein, and a negative association between starch equivalent and crude fibre. A further method for determining quality is a study of botanical composition, and this will be discussed in Part II.

Animal production can be measured by several factors - increase in live-weight, numbers and weight of offspring, milk production, wool production, and various other animal products. The one most generally used, however, is increase in live-weight, and this again raises several problems, some of which are considered in Part III. The determination of the quality of some animal products, such as milk, wool, and hides, presents little difficulty. The determination of the quality of live-weight increases, however, is extremely difficult; it is, in fact, almost impossible. It is assumed that the quality of the live-weight increases obtained are comparable during an experiment; but it cannot be assumed that the animals when going onto an experiment are of the same quality, even though they may be of the same age and the same weight. Many workers have already pointed out the necessity of a preliminary period of uniform treatment. An experiment using three groups of animals which had been on sub-maintenance, maintenance, and supra-maintenance diets showed that even after 70 days on identical treatments effects of the initial differences in diet were still apparent.

PART I

FORAGE YIELD

1. Introduction

Although it is necessary eventually to introduce the grazing animal into experiments on pasture improvement, the small plot technique in which the herbage is harvested and weighed plays an integral part in pasture research. It is not an alternative to experimentation using animals, but a preliminary step in the train of investigations. It has been stated by many workers that periodic harvesting by hand or by machine is not comparable with grazing by animals. This fact cannot be disputed, but nevertheless, the small plot experiment in which herbage is harvested and weighed direct forms an essential economic preliminary step.

In spite of the obvious differences due to preferential grazing by animals and the effect of trampling, urine, and faeces under actual grazing conditions, there is still a tendency among some workers to convert herbage yields from these preliminary trials into terms of increase in live-weight or other forms of animal production. Such terms are possibly more easily understood by the farmer, but they do not increase the value of the data, and are, in general, completely meaningless. Yield of herbage only should be considered and the experiments treated on exactly the same lines as normal agronomic experiments with other perennial crops.

The use of these preliminary experiments lies not in their ability to provide simple measurements of animal production, but in their ability to distinguish the less beneficial treatments. Treatments and pastures giving poor results under these conditions can be eliminated from further trial in more expensive experiments utilising animals. The use of these preliminary trials is in effect parallel with the use of variety trials testing a large number of new varieties; the poorer ones are eliminated in the initial stages and the better ones are further investigated on a larger scale.

In general, techniques in pasture research raise new difficulties which are seldom met with in other agricultural crops, and these comparatively simple preliminary experiments are no exception. One of these difficulties is to determine when the plots should be mown. When all plots are at the same stage of maturity at the same time the problem is relatively unimportant, for the results will be comparable at whatever stage the experiment is harvested, but if certain treatments tend to affect the rate of growth and development the problem becomes more important. If this occurs, then if all plots are mown on the same day, some may be flowering, while others are still producing more leafy growth. This would occur reasonably often when different species are being tested, and the problem cannot be ignored. Again drawing a parallel with other perennial crops, it is obvious that misleading results will be obtained if the crop is not at the same stage of development on all plots when harvested. It is therefore necessary to decide at what stage the herbage is to be mown, and to use the total yield over a period instead of analysing the results at each time of harvest. This means of course that the experimenter must decide when each plot has reached the stage of development required, and, because of this subjective element in the technique, may lead to a certain amount of bias and personal error. It does not appear, however, that this can be avoided, for it seems better to choose a course which will give more nearly comparable results than one which will definitely lead to an invalid comparison.

A second difficulty which arises in these preliminary trials is a determination of size and shape of plot to use in order to obtain reliable results. This is basically not a new problem, for many uniformity trials (sometimes called blank experiments) have been carried out on agronomic crops in an attempt to settle these questions, but few have utilised grass or forage crops. It is therefore a comparatively new problem in pasture research, for there is little information on the variability likely to be obtained in pasture experiments. It is generally recognised that this variability will be somewhat higher than that obtained in experiments with arable crops, but apart from that little more is known, particularly under tropical conditions, where the soil is far more heterogeneous than in countries such as England where the land has been tilled for centuries. In addition, the highly variable rainfall which is a characteristic of tropical regions will have a profound effect on the variability of herbage yields.

A number of uniformity trials utilising pasture and forage crops have been carried out at Muguga in Kenya, and these will be discussed in the following sections, and a new method of determining plot size is suggested.

2. Historical

2.1 General

A great deal has been published on the results of uniformity trials during the past 30 or 40 years - mainly with crops other than herbage - and Cochran (1937) realising their basic importance compiled a list of results published before 1937. One of the earliest discussions was written in 1911 by Mercer and Hall, using mangolds and wheat. In an attempt to determine the size of plot for use in experimentation they considered the coefficient of variation (defined as the standard deviation per plot expressed as a percentage of the mean). They compared the actual decrease in this coefficient as plot size increased, with that expected on the assumption of a random and independent distribution of the basic plot yields on the ground. A graph was constructed showing the relationship between coefficient of variation and the number of basic plots used to form the ultimate plot. (In earlier papers the term "ultimate plot" has been used to designate what is here called the "basic plot", but the sense in which it is now used seems more appropriate). This curve has in general a fairly sharp drop in variation initially with lower numbers of basic units followed by a much more gradual fall. It was suggested that the number of basic units corresponding to the "elbow" in the curve was the "optimum" size of plot.

The same method was used by Batchelor and Reed (1918) when investigating yields from citrus and walnut trees, and has subsequently been used by several later workers for determining size of plot. This argument, however, is fallacious, and no progress was made in the technique of dealing with results from uniformity trials until 1938, when Fairfield Smith pointed out that the "elbow" in the curve could be made to occur in different positions corresponding to different numbers of basic plots, depending on the scales chosen for ordinate and abscissa. The method he suggested was an important step forward, and will be considered in some detail later.

A different approach to the problem was made by Harris in a series of papers (1913, 1914, 1915 and 1920).

He suggested the use of the average correlation coefficient between all basic plots, which together form the ultimate plot, as a measure of soil heterogeneity. This measure, however, will depend on the size of the basic plot as well as the size of the ultimate plot, and is in no sense an absolute measure of heterogeneity, so that results from different experiments will not necessarily be comparable. The method has been used by several workers (e.g. Garber *et al* (1926) and Wiebe (1935)), but, as Fairfield Smith pointed out, the coefficient of heterogeneity "does not appear to serve any other purpose than to demonstrate that the fertilities of adjacent areas are correlated". Wiebe attempted to go one stage further, and determined coefficients of heterogeneity, which, as we have already pointed out, are merely average correlation coefficients between the basic plots, for ultimate plots of different sizes. He then used these to determine what he called "theoretical" coefficients of variation. Exact agreement was found between these "theoretical" coefficients and the actual ones obtained. This, however, is no argument in favour of the method, and merely demonstrates that the calculations were correct.

The problem of shape of plot for use in experimentation has been discussed by several workers, and the most extensive treatment of the problem was given by Christidis (1931). After a lengthy theoretical discussion he concluded that "in no case can square plots be more uniform than long narrow ones". The results of a number of experiments he examined appeared to confirm this, for out of 7 sets of data, 4 indicated the superiority of the long narrow plot, while the remaining 3 indicated no difference between the use of long plots and the use of square plots. The theoretical assumptions leading him to this conclusion, however, are too simple to obtain in practice, and it has been shown on several occasions that a square plot is less variable than a long narrow plot. Day (1920) had shown, for example, that plots which were long in the direction of least variation in field fertility (i.e. plots running parallel to the contour lines of fertility) were more variable than plots more nearly square, and those, in their turn, more variable than plots long in the direction of greatest variation (i.e. plots running across contour lines of fertility). Wood and Stratton as early as 1909 had also shown that long plots could be more variable than square plots, and subsequently Wassom and Kalton (1953) have reached the same conclusion. The disadvantage of plots long in the direction of least variation is clearly shown in a paper by Lord (1931), but he did not appear to realise the reasons for this effect. In this experiment rice was used, and under tropical conditions water is in general more important than soil fertility; it is not therefore surprising that plots long in the direction of the water gradient showed lower variation.

A number of other workers have found little or no difference due to shape of plot. Mercer and Hall (1911) in their early experiments found no advantage with a long plot. Batchelor and Reed (1918), McClelland (1926) and Fairfield Smith (1938) also found no differences between long and square plots in the results of their experiments. On the other hand, however, Summerby (1923), Stringfield (1928), Justesen (1932), Kulkarni *et al* (1936), Zuber (1942), Hodnett (1953), and many other workers, using a variety of crops, found that an increase in the length of plots was more effective in reducing variation than an increase in width. Presumably basing his reasoning on these results, Boyd (1949) definitely recommended the use of long narrow plots in pasture research even though this may cause increased difficulties in management.

Although most of the literature has been concerned with crops other than pasture or forage, the general techniques and conclusions can be applied with little alteration. But before progressing to a more detailed examination of technique, the few papers which have dealt with pasture and forage crops will be discussed in greater detail. One of the first reports of a uniformity trial with pasture was written by Davies in 1931, using natural pasture in Australia. In common with all other uniformity trials, the reduction in coefficient of variation was much less than would be expected on the assumption of a random and independent distribution of the yields of the basic plots on the ground. The method suggested by Mercer and Hall (1911) using the "elbow" of the curve was followed in order to determine the "optimum" size of plot. The long narrow plot appeared to have lower variability than the plot which was more nearly square. As botanical composition of the plot had an important effect on the yield it was suggested that the superiority of the long plot was due to the fact that "societies" of different species tended to be roughly circular in shape, and therefore the long narrow plot would tend to cut across more of these "societies". Although this may be a contributory factor, in view of the results from other experiments, it would appear more probable that fertility differences were more important. It was also found that with plots smaller than 150 square links, the distribution of plot yields was not normal.

Further uniformity trials on pasture were reported by Wassom and Kalton (1953). They used Fischer bromegrass and studied variability in three fields; one of which had been sown in rows the previous year; the second had been broadcast about 3 years previously; and the third had been broadcast with a mixture of bromegrass and alfalfa about 5 years before harvest. The first two fields were harvested in 1950 and the third in 1951. They concluded that the basic plot was most "efficient". Their definition of "efficiency", however, is open to question, and due to the correlation between neighbouring units, whatever they had taken as the size of the basic plot would have been termed the most efficient. Such an argument is therefore valueless. They used the method suggested by Fairfield Smith (1938) to determine the "optimum" size of plot. The use of two terms which should basically mean the same, but which did not, is rather confusing. Further confusion also arises by a faulty interpretation of the method suggested by Fairfield Smith. This will be clarified in the next section. Although the relative differences in variation due to plot shape varied from field to field they concluded that plots long in the direction of the fertility gradient gave the smallest estimates of variance.

No other uniformity trials on pasture appear to have been published, but a trial using sorghum harvested green as forage has been reported by Stephens and Vinall (1928). The method of dealing with the data was similar to that suggested by Mercer and Hall (1911), and they found little advantage in the use of long narrow plots.

2.2 The Method Suggested by Fairfield Smith

The method suggested by Fairfield Smith (1938) represents the first real progress in dealing with results from uniformity trials. He first of all pointed out the fallacy of using the "elbow" in the curve showing the relationship between variation and size of plot as the 'optimum' size of plot. As this method had been used for some years giving misleading results, this, in itself, was an important step forward. He also pointed out the general unscientific approach when comparing the decrease in variability due to an increase in plot size with the reduction to be expected if the yields of basic plots were distributed independently and at random on the ground. The curve is possibly useful for comparison, but, as a zero correlation between neighbouring plots on the ground cannot be accepted, it is therefore meaningless to regard this curve as "theoretically expected". This again was a useful point to make in that it directed attention away from meaningless conceptions and onto a more fruitful line of approach.

Developing his argument, he showed that there was an empirical linear relationship between the logarithm of the variation and the logarithm of plot size, which appeared to be true in a number of experiments covering a variety of crops in different parts of the world. This relationship may be expressed in the form

$$\log V_x = \log V_1 - b' \log x \qquad .....(1)$$

where V_x is the variance of yield per unit area, for an ultimate plot of x basic units, and b' is a constant. The value of b' can vary between unity and zero. When b' is unity, this relationship reduces to

$$V_x = \frac{V_1}{x}$$

which is the normal expression when plots are independently distributed on the ground with no correlation. When b' is zero there is no reduction in variation, and the relationship becomes

$$V_x = V_1$$

This can only happen when the average correlation coefficient between all basic plots which together form the ultimate plot is unity. This means that the correlation between any two plots taken at random must also be unity, and this can only occur when all basic plots give the same yield. This then means that V_1 will be zero, and as a variance cannot be negative, it is obvious that no reduction can be obtained. This, of course, can never happen in practice, and therefore b' must always be greater than zero, which means there must always be a reduction in variation when basic plots are combined to form a larger ultimate plot.

This point does not appear to have been realised by Hasson and Kalton (1953), and a certain amount of confusion has arisen in the interpretation of their results. They state that a lower value of the regression coefficient b' means that the crop is more uniform; this is the exact opposite of the correct interpretation. The higher the value of b', the more nearly the distribution of yields on the ground approaches a random normal distribution. This is essentially a "uniform" distribution. If the distribution of yields on the ground is "patchy", with some association between high and low yielding areas, the distribution is no longer "uniform", and the value of b' will drop. As the area of these patches increases, indicating increased lack of "uniformity", the value of b' decreases.

The demonstration by Fairfield Smith that there was an empirical linear logarithmic relationship between variation and plot size was an important step, but realising that this was not quite sufficient he went on to extend the theory to cover any size of field by incorporating the concept of an infinite field. By a slight change in notation he used $(V_x)_n$ to denote the variance of the mean yield per unit area, of plots x units in area, over a block of n plots. Equation (1) can then be rewritten as

$$\log(V_x)_{n/x} = \log(V_1)_n - b' \log x \qquad \ldots\ldots(2)$$

where n is the total number of basic plots in the field.

He suggested that the simple empirical law he had observed would be strictly true only when dealing with an infinite field, and this could be expressed in the form

$$\log(V_x)_\infty = \log(V_1)_\infty - b \log x \qquad \text{.....(3)}$$

(In this case b has no dash to distinguish it from the constant in equation (1)). He went on to show that

$$(V_x)_m = \frac{m(1-m^{-b})}{(m-1)} (V_x)_\infty \qquad \text{.....(4)}$$

which may be rewritten as

$$\log(V_x)_{n/x} = \log(V_1)_\infty - b \log x + \log \frac{n(n^b-x^b)}{n^b(n-x)}$$

$$\text{.....(5)}$$

and he suggested that this should be used instead of equation (1). This is a complicated expression, and a maximum likelihood solution for b appears to be extremely tedious. No method for the determination of b was in fact given, but it was stated that its value may differ substantially from that of b' calculated for a finite field. This would tend to indicate that the use of equation (1) instead of equation (5) would lead to large errors in estimating plot size. That this is not correct is obvious from the fact that both expressions must fit the observed data, and apart from the nicety of establishing a generalised equation it is doubtful whether there are any advantages in the more complicated expression. No useful purpose appears to be served by its calculation, for it cannot be utilised in other areas, or on the same area for a different crop. It is an interesting mathematical exercise, but its practical use is extremely limited. Workers who have subsequently used the method have sensibly ignored the complications of the more involved equation; they have, however used his method for calculating the "optimum" size of plot.

The "optimum" size of plot was defined as that which gave the most information for the minimum cost. (Here "information" is used in its statistical sense, and the amount of information per plot is defined as the inverse of the variance per plot). This appears to be a reasonable definition of "optimum" size until it is examined in more detail. It was suggested that the cost per plot of all the operations involved in finally obtaining yield data could be expressed in the form

$$K_1 + K_2x$$

where x is the size of the plot,

K_1 is the cost of the various operations irrespective of size of plot

and K_2 is the cost of the various operations per unit area.

If this is a reasonable expression for the cost per plot, then the "optimum" size of plot is given by

$$x = \frac{bK_1}{(1-b)K_2} \qquad .....(6)$$

This depends on the cost factors K_1 and K_2. If actual monetary cost is considered, then these factors will vary in different parts of the world in the same year, and in different years in the same area, so that no comparable results would be obtained. If time is considered and cost is expressed in man-hours, the cost factors may be fairly stable on a given experimental station, but owing to differences in skill of labour, comparison with other stations and with other countries will not be possible. In addition the determination of the relative proportions of the cost of even a single operation which should be included in K_1 or K_2 appears entirely arbitrary. Fairfield Smith, for example, included the whole of the cost of seed preparation in K_2, while Wassom and Kalton (1953) included only one third. With such discrepancies it is obvious that the estimates of K_1 and K_2 must necessarily be very arbitrary, and the expression in equation (6) must therefore also have little practical value. Even if these objections did not arise, and it was possible to obtain reliable estimates of K_1 and K_2, the prime consideration in experimentation is not cost at all, but reliability of results. If this is used as a criterion to determine plot size, the estimate of cost could then be applied to the experiment as a whole to determine whether it is worth while, or even possible, to carry out. This problem will be further considered in the next section.

3. The Modified Method

The equation used by Fairfield Smith expressing the relationship between plot variation and plot size involved the variance of yield per unit area per plot as the measure of variation. With the use of this variance as the measure of variation comparisons can only be made within the experiment. If coefficient of variation is used instead, however, results from different experiments can be compared. Because of the simple relationship between the coefficient of variation and the variance suggested by Fairfield Smith, it is simple to substitute in equation (1). This simple relationship can be found as follows:-

$$V_x = \frac{s_x^2}{x^2}$$

$$= \frac{s_x^2}{m_1^2 x^2} \cdot m_1^2$$

$$\text{or } V_x = \left\{\frac{C.V.(x)}{100}\right\}^2 m_1^2 \qquad \text{.....(7)}$$

where s_x is the standard deviation per plot of x basic units in size,

and m_1 is the mean yield per basic plot.

Substituting this value for V_x in equation (1), we have

$$\left\{\frac{C.V.(x)}{100}\right\}^2 m_1^2 = \left\{\frac{C.V.(1)}{100}\right\}^2 \frac{m_1^2}{x^{b'}}$$

$$\left\{C.V.(x)\right\}^2 = \frac{\left\{C.V.(1)\right\}^2}{x^{b'}}$$

With a slight alteration in notation this becomes

$$C_x = \frac{C_1}{x^a} \qquad .....(8)$$

where $a = \frac{1}{2}b'$, and $C_x = C.V.(x) =$ coefficient of variation for plots of size x.

This form of the relationship, instead of that given in equation (1) will be used throughout the following discussions.

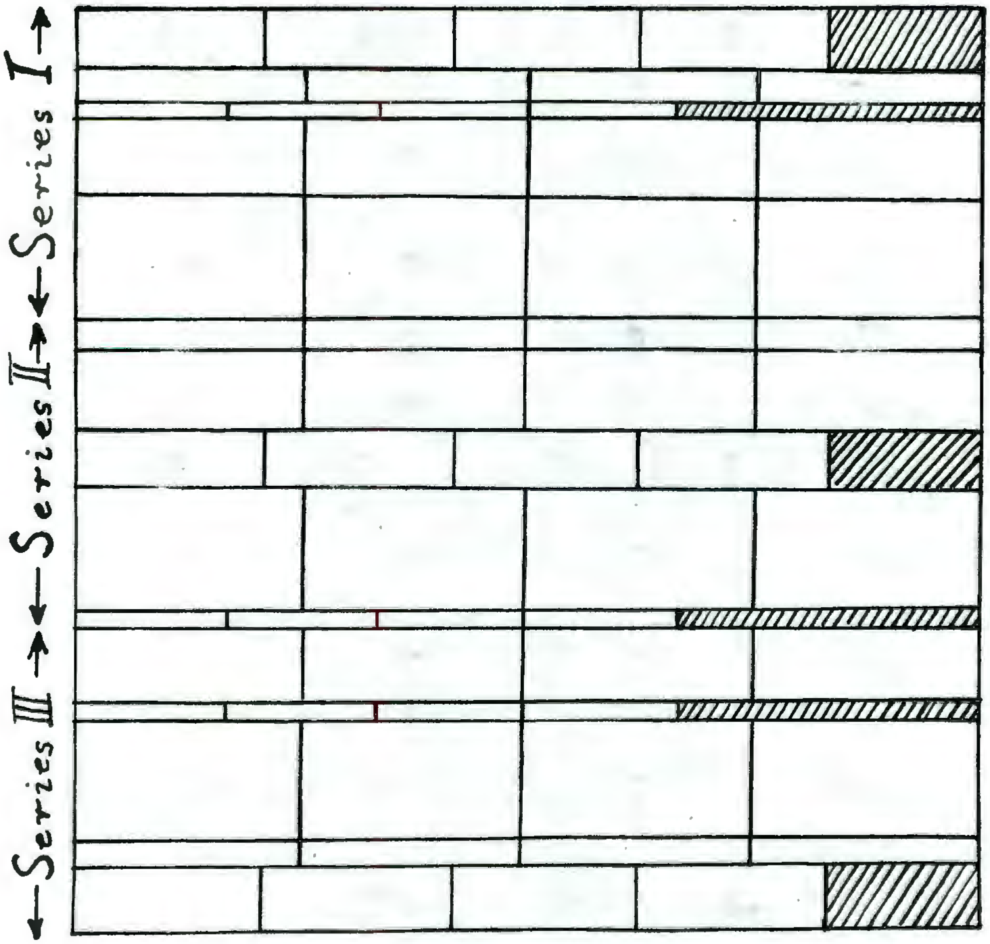
It has already been pointed out that Fairfield Smith recognised the limitations of using this simple equation, and he was therefore tempted to incorporate the concept of an infinite field, but this method is of very limited practical value. In equation (8), the constant a is dependent upon the crop, the weather, and the variation in soil fertility in the field.

The object of the present series of uniformity trials was to determine a plot size which would give reliable results in pasture research. If the fertility variations in the field actually used for the experiment unduly affect the value of a, then the general application of the results may be misleading. It is therefore necessary to utilise some method which will eliminate this effect so that the results obtained here may be applied in other areas. The best method for eliminating as far as possible the variations in soil fertility is that employed in the analysis of a Latin square experiment, when differences between rows and columns are taken out. This cannot of course entirely eliminate fertility differences, but if this is done, the major effects will be reduced, and the constant a will then depend largely on the crop under trial. It is of course necessary to demonstrate that the empirical linear logarithmic relationship still holds, if this method is to be of practical use. This will be shown when discussing the various experiments.

The other criticism of the method suggested by Fairfield Smith was made against his definition of "optimum" plot size. In view of the practical points which have already been made, an experiment was conducted at Muguga in order to determine the practical value of the results. Kikuyu grass growing in a field of approximately ¾ acre in area was used, and strict timing of the various operations was taken.

Three replications, or series, of plots were used. In each series, five different sizes of plots were marked out, in sets of four plots each, and the sets were randomised within each series. The dimensions in yards of the five different sizes of plot were 1 x 10, 2 x 15, 4 x 12½, 5 x 15 and 8 x 15, giving areas of 10, 30, 50, 75, and 120 square yards respectively. The actual layout is given in Fig.I.1.

Fig. I.1. The field layout for the experiment designed to record the times taken for different operations with different sizes of plot



▨ Areas not harvested

Table I.1. Time in seconds of various operations per plot of various sizes

Operation		Size of Plot (square yards)				
		1x10	2x15	4x12½	5x15	8x15
Cutting*	1	155	415	736	1152	2112
	2	78	115	118	113	125
Bagging*	1	68	210	309	527	808
	2	13	16	16	14	18
Weighing		69	180	250	468	772
Sampling		72	98	114	143	171
Loading		18	30	49	87	120
Unloading		10	25	34	55	78
Total		483	1089	1626	2559	4204

*The first row for cutting and bagging times indicates the time spent on the actual operations while the second row indicates the time spent in walking from one plot to the next, and time spent in tidying up and other miscellaneous activities. It was found by subtracting the result in the first row from the total time taken.

The time taken to carry out the following operations was recorded:-

1) Marking. This was done by one European and two African assistants.

2) Cutting. The grass on each plot was cut by hand; three African labourers cut the grass with pangas, and stacked the grass in piles on each plot, ready for putting into bags. The time of actual cutting of each plot was recorded as well as the total time taken to cut and stack each set of four plots.

3) Bagging. This was done by the same three labourers, and the bags were left in piles on each plot ready for putting onto a trailer to be taken for weighing.

4) Weighing. The bags were emptied into a small weighbridge which was normally used for weighing sheep and pigs. Three African labourers emptied the bags and the weight was recorded by an

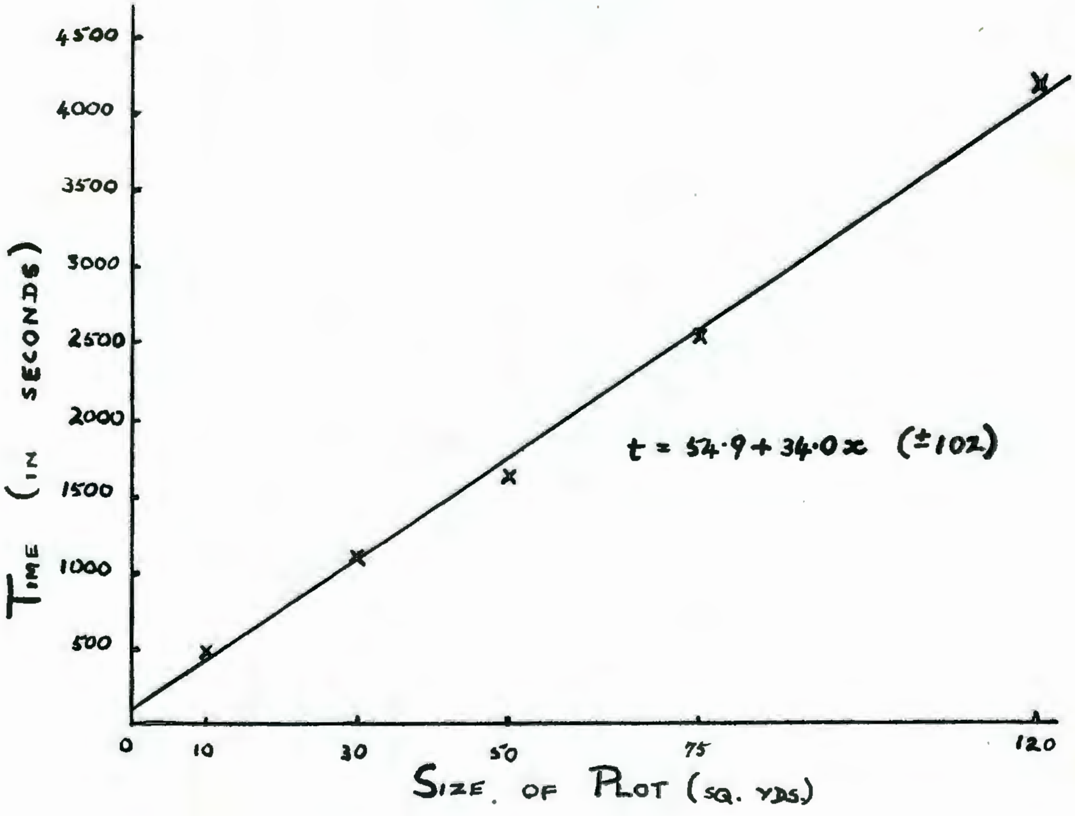
African overseer.

5) Sampling for dry matter determinations. When the produce from each plot had been weighed, it was put into a concrete yard and there mixed as thoroughly as possible with hay forks, and two samples were taken.

6) Chaffing. Each sample was separately chaffed by a tractor-driven machine, and a subsample of about 1 lb. extracted.

The times taken to load and unload the trailer were also recorded, as well as the time taken in the journey to and from the weighbridge. The average times taken per plot for the three series for most of these operations are given in Table I.1., and shown graphically in Fig. I.2. The times taken for marking, for the journeys to and from the weighbridge, and for chaffing have been omitted from the table. Each sample taken after weighing in this trial consisted of about half a sack of fresh material, so that the time taken for chaffing was constant irrespective of the size of plot. In some experiments, the size of sample

Fig. I.2. The relationship between the total time taken per plot for those operations given in Table I.1. and the area of the plot, together with the regression line calculated from the data.



may depend on the size of the plot (if, for example, a fixed percentage of the total produce is taken), and in this case the time taken for chaffing will depend upon the size of the plot. When a tractor-driven machine is used, the difference between a small and a large sample will not be very great, and for all practical purposes could be regarded as constant. When the chaffing is done by hand-operated apparatus, the difference may be considerable, and would have to be taken into consideration. Somewhat surprisingly the time taken for marking also showed little dependence upon the size of plot. The times taken per plot for these two operations were 33 seconds and 85 seconds respectively. The average time taken in travelling from the field to the weighbridge was 2 minutes 31 seconds. It is difficult to reduce this to a time per plot with the small numbers involved in this investigation, for, obviously in a full experiment more journeys would be required if an equal number of large plots were used.

Fig. 1.2. shows that the assumption of a linear relationship between cost (measured in time consumed)

and plot size is a reasonable one. The standard deviation of the estimated time from the regression equation of only 102 seconds is small, and only about 5 per cent of the average time taken per plot for all plot sizes. The standard deviation obtained in the analysis of variance of the times taken per plot for the full experiment was 112 seconds, or 65 seconds for a mean of 3. This is in very close agreement with that obtained by using the linear relationship, and indicates that little improvement could be obtained by utilising a different type of relationship.

The regression coefficient of 34.0 is equivalent to K_2 in Fairfield Smith's notation. The K_1 of the expression he used will be equal to 54.9 (the constant term in the equation) plus the time taken in any operation which is constant for all sizes of plot. The time taken in marking and chaffing must be taken into account, and also the time required to weigh the sample for dry matter determinations, and the time taken in the statistical analysis. If we estimate 60 seconds as the time required per plot for each of these two last operations, then K_1 becomes

$$54.9 + 38 + 85 + 60 + 60 = 297.9$$

The time taken on the journey will affect both K_1 and K_2, but if the corrections made necessary by incorporating it into the expression are small, as they probably are and can be ignored, then the "optimum" size of plot, from equation (6) will be given by

$$x = \frac{b\ 297.9}{(1-b)34.0} \qquad .....(9)$$

In grassland experiments it would appear that the value of b is about 0.4 (this will be shown later), and substituting in equation (9),

$$x = 5.8 \text{ sq. yds.}$$

This is far too small to be of any practical use. "Optimum" plot sizes obtained by Fairfield Smith (1938) and Wassom and Kalton (1953) were 5 square feet and 26¼ square feet respectively. These again are far too small to be of any practical use in a field experiment, for carry-over effects from one plot to the next would be quite large, and the number of replications required to obtain reliable results would probably be prohibitive.

Once again the point of reliability of results has been raised. It is in fact the only valid criterion for determining technique in experimentation. Cost, particularly arbitrary cost factors such as the ones discussed above, should be completely ignored initially, and considered only in the final stage when it is to be decided whether the results it is hoped to obtain justify the expense. If the results are inconclusive then the "optimum" size of plot as calculated above, is, in no real sense of the word, "optimum".

No attempt is here made to give an alternative definition of "optimum" size of plot, but instead, a minimum size is suggested, together with the number of replications necessary for a selected degree of accuracy. In addition, a table is given showing the comparative sizes of plot required for the same degree of accuracy with fewer replications.

If the coefficient of variation of an agricultural experiment is high, the experimental technique is open to criticism. In general a coefficient of variation as low as 5 per cent is regarded as very good, and one of 10 per cent is regarded as satisfactory. The minimum size of plot will therefore be defined as that giving a coefficient of variation of 10 per cent. This may be criticised as being arbitrary, but in view of the objections already given against the use of cost factors, which are even more arbitrary, this criticism, although to a certain extent valid, is small in comparison.

The minimum size of plot, x_0 (in terms of the area of the basic unit plots), is found from equation (8). In this, the value of 10 per cent is substituted for C_{x_0}, and we have

$$x_0^a = \frac{C_1}{10}$$

$$\text{or } \log x_0 = \frac{\log C_1 - 1}{a} \qquad \text{.....(10)}$$

Having determined the size of plot, it is possible to calculate the number of replications necessary for a given degree of accuracy. If the difference between the mean yields of two treatments is a fraction, d, of the overall mean, then the value of Student's t is given by

$$t = \frac{dm_x}{s_x\sqrt{\frac{2}{r}}} \qquad \ldots\ldots(11)$$

where r = number of replications

m_x = mean yield of ultimate plots, x basic units in area

and s_x = standard deviation of plots x basic units in area.

Solving for r in this equation, we have

$$r = \frac{2t^2s_x^2}{d^2m_x^2} \qquad \ldots\ldots(12)$$

With the minimum size of plot, the coefficient of variation is 10 per cent, i.e. $s_x/m_x = 0.1$. If it is required to prove a 10 per cent difference between means (i.e. $d = 0.1$) significant at the 5 per cent level of probability (i.e. t is approximately equal to 2), the number of replications of the minimum size of plot necessary for this degree of accuracy is obtained by substitution and is found to be 8. This can, of course, include what is termed "hidden replication";

for example in a single replication of a 2^3 experiment, 8 different treatment combinations are laid down, but there is a fourfold replication of the main effects.

In general, this level of accuracy will be sufficient. In an exploratory trial, however, with a comparatively large number of treatments, a difference between means of 15 or 20 per cent only may be required for significance. Using these lower levels of accuracy, the number of replications necessary would be reduced to 4 and 2 respectively. On the other hand, if greater accuracy is required, and a difference of 5 per cent should be regarded as significant, 32 replications would be necessary.

Substituting the value for s_x/m_x obtained from equation (8) into equation (11), we have

$$t = d\,\frac{m_x}{s_x}\sqrt{\frac{r}{2}}$$

$$= d\,\frac{m_1}{s_1}\,x^{a}\sqrt{\frac{r}{2}}$$

where m_1 = mean yield per basic plot

and s_1 = standard deviation of the yield per basic plot,

this then gives

$$x^a = \frac{s_1}{m_1} \cdot \frac{t}{d} \cdot \sqrt{\frac{2}{r}}$$

If r_0 is the number of replications required for a given degree of accuracy, when the minimum size of plot is used, then, from this equation

$$\left\{\frac{x}{x_0}\right\}^a = \left\{\frac{s_1}{m_1} \cdot \frac{t}{d} \cdot \sqrt{\frac{2}{r}}\right\} \left\{\frac{m_1}{s_1} \cdot \frac{d}{t} \sqrt{\frac{r_0}{2}}\right\}$$

or $$\frac{x}{x_0} = \left\{\frac{r_0}{r}\right\}^{\frac{1}{2a}} \qquad \text{.....(13)}$$

Using the level of accuracy considered to be sufficient for most practical purposes, i.e. with r_0 = 6, Table I.2. gives the value of this multiplying factor for different numbers of replications and different values of a. When a is small this factor increases rapidly, and, in general, it would be better to use the minimum size of plot with 6 replications.

Table I.2. The ratio of the plot size required if fewer replications are used to that of the minimum plot size with 8 replications, for different values of a.

		a			
		.1	.2	.3	.4
REPLICATIONS	7	1.95	1.32	1.25	1.18
	6	4.21	2.05	1.61	1.43
	5	10.49	3.24	2.19	1.80
	4	32.00	5.66	3.19	2.38
	3	135.00	11.62	5.13	3.41
	2	1024.00	32.00	10.08	5.66

It is doubtful whether a ten-fold increase in plot size would be economical, and, as a general recommendation it would appear that 6 times the area of a plot necessary for a 10 per cent coefficient of variation should be the maximum size for use. This then fixes lower and upper limits for the practical size of a plot in experimentation. In general, however, when selecting a plot size for a given design, a plot about three times the minimum size would be sufficient provided r and s are not small.

There are several advantages in using this method over that proposed by Fairfield Smith. In the first place, the use of cost factors is avoided. This suffers from three important defects-

1) The estimates of the cost factors cannot be determined with any great accuracy;

2) Costs will vary in different years, in different countries, and with different methods. If a machine is used instead of labourers, the cost in time will presumably be much less, but should the capital expenditure on the machine be included?

3) Using cost as a criterion may give inconclusive results.

Secondly, a knowledge of the value of a is required to determine the "optimum" size of plot. A guess may be made at this value, from the results obtained elsewhere, but a small variation in a can produce a large variation in the size of the "optimum" plot. With the method suggested here, no exact knowledge of a is required. It should be possible to obtain a rough estimate of the size of plot necessary for a 10 per cent coefficient of variation from ordinary field experiments in the neighbourhood. If this is increased 3 or 4 times and 4 replications used, the experimental results, should, in general, be conclusive.

Having estimated the size of the minimum plot, a comparison of the labour and costs involved in using larger plots with a smaller number of replications and an examination of other practical details will enable the experimenter to determine the exact design for use.

From equations (8) and (12), we have

$$r = \frac{2t^2 s_1^2}{d^2 m_1^2 x^{a}}$$

From this it is obvious that as x increases the number of replications required for a given degree of accuracy will decrease. The area per treatment used in the experiment will be xn, or

$$\frac{2t^2 s_1^2}{d^2 m_1^2} x^{(1-2a)}$$

In practice a is always less than 0.5, so that for a given degree of accuracy, the area required will increase as the size of the experimental plot increases. Expressing this in a different form; for a given area, the value of t decreases as x increases, indicating that, in general, replication is more effective in reducing error than a change in plot size. This result has, of course, been noted in the practical results of uniformity trials by many workers.

4. The Experiments

4.1 A General Description

Uniformity trials were conducted at Muguga on the following forage and pasture crops:-

1) Oats in 1953 and 1954
2) Lucerne
3) Natural regeneration
4) Planted Kikuyu grass (Pennisetum clandestinum)
5) Star grass (Mixture of Cynodon dactylon and C. plectostachyum)
6) Rhodes grass (Chloris gayana).

With oats two trials were laid down, and only one harvest taken on each of the sites. With the remaining crops three harvests were taken on each experiment during the course of a year, except for natural regeneration, the first harvest of which was taken in June 1953 and the last in January 1955.

The majority of the earlier literature pointed to the fact that a long narrow plot was preferable to one more nearly square, and for this reason the basic plots were 1 x 6 square yards. This size, and the dimensions, were selected because Davies (1931) in Australia indicated that this was approximately the smallest size of plot giving a normal distribution of yield. In addition Stephens and Vinall (1928) had used basic plots of 40 ins. x 16½ ft. when dealing with green sorghum. This again approximated very closely to the size chosen for this series of uniformity trials. There was one exception to this general rule. In the second trial with oats, basic plots of 1 yard square were used because of certain indications obtained in the 1953 results.

In general a total area of just over half an acre was used in each experiment. The total number of basic plots was 432 arranged in a 24 x 18 layout, the dimensions being 24 yds. x 108 yds. In other words, the short sides of the basic plots were parallel with the short side of the total area, and the long sides of the basic plots parallel with the long side of the total area. Again there were certain exceptions to this general design, and these will be mentioned in the relevant sections later.

The plots were harvested by machine, which cut exactly to a width of 1 yard. The produce from each plot was separately weighed and sampled. Each sample was chaffed by machine and a subsample of about 2-3 lb. taken for dry matter determinations. From this subsample, 400 gms. was weighed, and put in an oven at 100°C until all moisture was removed. The dry matter percentage so determined for each plot was then used to convert the fresh weight into dry weight for statistical analysis.

Fresh weights were not used in the statistical analyses because variations in moisture content could be considerable. If there had been a heavy mist during the night prior to cutting, for example, the yield from plots harvested early in the morning would contain a considerable amount of external moisture. Later in the day, with a strong sun, most of this external moisture would have evaporated. This was particularly noticeable on one occasion with lucerne. It is obvious therefore that misleading results would have been obtained if fresh weights had been used. This point appears to have been overlooked by several earlier workers, who used fresh weights. With grain crops little or no error would be involved, but in any experiment where vegetative growth is also measured, errors could be considerable.

Even when the effects of different moisture contents are not immediately obvious, it is probably preferable to use dry weights instead of fresh weights. The results of one harvest - the first cut of natural regeneration - were statistically analysed with both fresh and dry weights. The weather had been uniform throughout the harvesting period and no immediately obvious effects were noticeable. The standard deviations of the basic plots were 5.67 and 1.37 for fresh and dry weights respectively, giving coefficients of variation of 30.4 and 26.9 per cent. The reduction in variation was caused by a significant relationship between fresh weight and percentage dry matter. When the fresh weight was comparatively high the percentage dry matter tended to be low; this would cause a levelling out in the dry-matter yield, thus reducing variation. It is obvious therefore that the use of dry weights is preferable.

One criticism may possibly be levelled against the use of dry weights. It may be objected that the estimation of percentage dry matter based on such a small sample will not be valid. If the method of sampling is poor this may be true, but any reasonable method of sampling, even though the more complicated methods are not used, should give a reliable estimate of the percentage dry matter. The variation in estimates of dry matter percentage to be expected was determined in two separate tests. In the first, a total of 1250 pounds fresh matter was thoroughly mixed and twenty-five 50 pound lots were extracted. From each lot a sample was taken for chaffing and a subsample for dry matter determinations. The mean percentage dry matter was 36, and the standard deviation obtained was 0.64, which is less than 2 per cent of the mean. In the second examination fifty-three lots of 25 pounds fresh weight were used in a similar manner. The mean percentage dry matter was 42 and the standard deviation was 1.5, which is less than 4 per cent of the mean. With large amounts of over 1200 pounds fresh weight it is gratifying to find such a small variation. With smaller amounts the material would probably be more homogeneous, and a lower variation would be obtained. There therefore appears to be little danger of any serious error in using one sample to determine dry matter percentage.

4.2 Oats

4.2.1 Size of Plot

The first experiment, in 1953, was on about ½ acre in the middle of a planted area to avoid border effects. The importance of these effects have been pointed out by McRostie and Hamilton (1927) McClelland (1929) and other workers. The layout was not quite the same as that used in the majority of this series of experiments in that only 288 basic plots were laid out in an area 24 yds. x 72 yds. The short side of this area was divided into 24 units of 1 yard and the long side into 12 (instead of 18 in the normal layout) units of 6 yards.

Day (1920) using wheat, and Immer (1932) using mangolds, reported that contour lines for weight ran parallel to the direction of the rows to a very pronounced degree. An examination by eye appeared to confirm that this was the case with the oats in 1953. It has already been pointed out, that in order to minimise the variation between plots, they should be long across the contour lines of fertility; it was therefore decided that the long side of the plot should lie across the drill rows.

In 1954 the basic plots were 1 yd. x 1 yd. in area, and a total of 576 plots were harvested in a square area of 24 x 24 square yards. Again border effects were avoided by locating the experimental area some distance from the edge of a planted field. Mercer and Hall (1911) had used a fixed number of rows to demarcate the plots, for they considered this important in order "to make the measured area one of crop rather than land". It will be shown that this precaution appears to be unnecessary, and the plots in these two experiments paid no regard to any deviations in the drill rows.

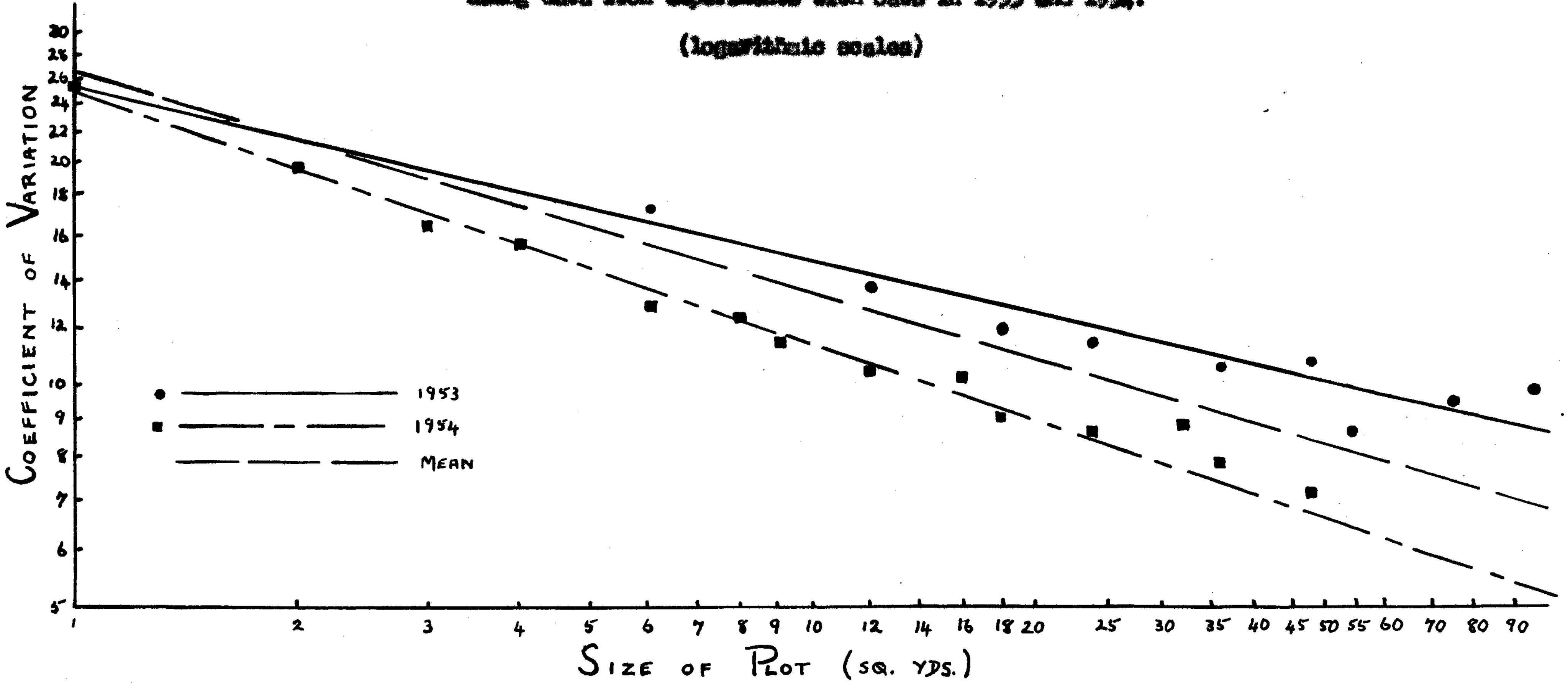
In 1953 the mean yield per unit plot was 5.68 pounds dry matter (or 0.95 pound per square yard), and the standard deviation obtained was 1.38 pound per plot, giving a coefficient of variation equal to 24.3 per cent. The distribution was first tested for normality, and it was found that g_1, a measure of skewness, was equal to -0.16 ($\pm$ 0.14), and g_2, a measure of the sharpness of the peak of the curve, was -0.11 (± 0.29). Theoretically, with a normal distribution, both these values should be zero, but neither is statistically significant (see Fisher (1948) for expressions for the g's and their variances). Good agreement is therefore indicated with the normal type of distribution. A χ^2-test on the actual and theoretical distributions again indicated no significant differences.

In 1954 the mean yield per basic plot of 1 square yard was 0.96 pounds dry matter, and the standard deviation obtained was 0.26 pounds per plot, giving a coefficient of variation equal to 27.2 per cent. The distribution of plot yields was again tested for normality. The distribution of the basic plot yields was significantly different from normal. The smallest plot giving a normal distribution was 6 square yards in area. The values of g_1 and g_2 obtained were -0.05 (± 0.25) and -0.35 (± 0.49) respectively for plots 1 yard along the rows and 6 yards across. These values are not statistically significant from zero.

Fig. I.3. shows the actual mean coefficients of variation for different sizes of plot, irrespective of

Fig. I.3. The relationship between coefficient of variation and plot size using data from experiments with oats in 1953 and 1954.

(logarithmic scales)



shape, on a logarithmic scale, together with the straight lines determined by the method suggested in section 3. Throughout, it is necessary first of all to demonstrate that the empirical linear logarithmic relationship still holds when the variation between rows and columns has been eliminated. This is immediately obvious in those experiments using oats, for there is good agreement between the actual values and those determined from the straight lines, and it will be seen that the same holds true when dealing with the other forage and pasture crops. The results from both years have been reduced to the same scale of plot size; the results for 1953 therefore start at a plot size of 6 square yards, and the two highest plot sizes, viz. 108 and 144 square yards, which were used in the calculation of the regression line, have been omitted from the figure.

The method of weighting suggested by Fairfield Smith was used in determining the regression coefficients. The equations obtained were

$$\log(C_x) = 1.400 - 0.23 \log(x) \qquad .....(14)$$

and $$\log(C_x) = 1.394 - 0.34 \log(x) \qquad .....(15)$$

in 1953 and 1954 respectively. In both cases the size of plot, x, is expressed in terms of square yards (and NOT in the number of basic units of 6 square yards in the case of 1953). From these regression equations, the expected coefficients of variation for plots of 1 square yard are 25.1 per cent and 24.8 per cent respectively.

It is rather surprising that these values and also the mean yields of dry matter (0.95 and 0.96 pounds per square yard) should be in such close agreement. In 1953 the oats were harvested 152 days after planting when they were just "piping", while in 1954 the crop was only 102 days old, and well "piped". In view of the large difference in actual age of the crops, and the more advanced development of the younger crop, it would appear that the agreement found was fortuitous.

The difference between the two equations lies in the different values obtained for the regression coefficient, a. It was explained in section 3 that eliminating rows and columns before determining the regression coefficient, should reduce the dependence of this parameter on soil heterogeneity and make it largely dependent on the crop alone. The two regression coefficients should therefore be more nearly equal. If, however, variation between rows and columns is not removed, the regression coefficients are 0.12 and 0.33. The difference in this case is much larger, and, therefore indicates the value of the method now suggested. It is obvious that all heterogeneity due to other factors has not been removed, but a large proportion has.

In 1953 the rains were poor, and only a limited amount of water was available for plant growth. Clements *et al* (1929) pointed out that the greatest competition between neighbouring plants is for underground water, and when this is scarce, competition will be severe. The longer the period during which the plants are growing the more marked will be the effects of competition, and this again will be effective in 1953. On the other hand, in 1954, the rains were ample for the needs of the crop, and effects of competition for water will not be marked. This, then, is the probable explanation of the higher variation in the first year when plots were increased in size. It would appear that the regression coefficient, a, had been influenced to a marked degree by limiting rainfall, although dependence on variations in soil fertility had been reduced. The effect of rainfall on variation in yield has been pointed out by Metzger (1935), who also found that when rainfall was adequate variability decreased.

The mean regression coefficient determined from the weighted sums of squares and products within years was -0.29. Each experiment was given the same weight, which involves a correction to the actual weights in one or other of the two years. The equation obtained was

$$\log(C_x) = 1.421 - 0.29 \log(x) \qquad .....(16)$$

As the trials were conducted in good and bad years, it was thought that this mean coefficient would probably be more representative of the general results likely to be obtained under East African conditions.

It is interesting to compare these results with those obtained by Stephens and Vinall (1928) for green sorghum. The general climatic conditions in which they were grown appear to be similar to those at Mugoga. The value of a, the regression coefficient (using the calculations already carried out by Fairfield Smith) was -0.21. This is somewhat lower than that in equation (16), but as differences between rows and columns were not eliminated such a difference is not unlikely. The coefficient of variation for the basic unit of 53 square feet with sorghum was 15.7 per cent. That calculated for oats from equation (16) would be 15.5 per cent. These are in very close agreement, but when the variation between rows and columns is eliminated in the case of sorghum it is possible that a lower coefficient of variation may be obtained. The reduction, however, should not be large.

Mean coefficients of variation have been plotted in Fig. I.3. irrespective of plot shape, because there was no effect of shape of plot on variation. This was also true of the results obtained with sorghum. In 1953 the basic plots lay across the rows of oats, but as this did not appear to have any effect in reducing variation it was decided that in 1954 square basic plots should be harvested in order to test the application of the results obtained by Day (1920) and Immer (1932). Again, however, there was no difference between the variation of plots long across the rows and that of plots long in the direction of the rows. Even when the variation between rows and columns was not eliminated there was no evidence of superiority of the long narrow plot. It does not therefore appear to be generally necessary in experimentation to orient long plots across the rows of planting. This point possibly needs testing more extensively, but if plots can be laid out parallel to the lines of planting, there may be a considerable saving of effort in some experiments; for example if fertilizer can be applied mechanically between the rows instead of by hand where short rows are involved.

The minimum size of plot, i.e. one giving a 10 per cent coefficient of variation, calculated from equation (16) is 27.6 square yards. This is an easily manageable unit, and even 3 or 4 times this area gives a practicable size of plot. The method proposed by Fairfield Smith to determine "optimum" size, would give

$$x = \frac{K_1}{K_2}$$

(putting $b = 2a = .6$ in equation (6))
If this were to give a value of about 28 square yards then K_1 should be about 95 per cent of the total cost per plot. In other words, from the practical point of view, that part of the total cost per plot depending on plot size, i.e. K_2, would be small enough to be ignored. In general, the cost of the machinery and other equipment as well as valuable time spent in discussing and designing the experiment is omitted from K_1; if it were included possibly more sensible sizes of plots would be suggested. Again, however, there is the problem of being unable to determine the cost in a more than arbitrary manner.

4.2.2 Relationship Between Yield and Number of Tillers per Plot

In 1954 the first 120 plots were examined and the number of tillers counted in order to determine the relationship between yield and number of tillers. Hodnett (1953) established a statistically significant relationship between yield and plant number with ground-nuts in basic plots of 7 links x 25 links. He does not, however, state whether the relationship was of practical value, but a comparison of the "fertility diagrams" of plant number and yield would indicate that it was not.

Hudson(1941) when dealing with wheat harvested in basic units of 6 inches of drill row established the fact that the yield per basic unit was not only affected by the number of plants within the unit but also by the number of plants in adjacent units. If a basic plot were surrounded by other plots with a large number of plants, the effect of the competition was to reduce the yield of the central unit. Similarly, when the central unit is surrounded by a low plant population, the plants in the central unit are able to utilise a larger proportion of the soil nutrients, and produce a comparatively higher yield. With basic plots only two rows wide, used in the ground-nut trial, competition between neighbouring plots will still be effective, and it is doubtful whether a simple linear relationship will hold.

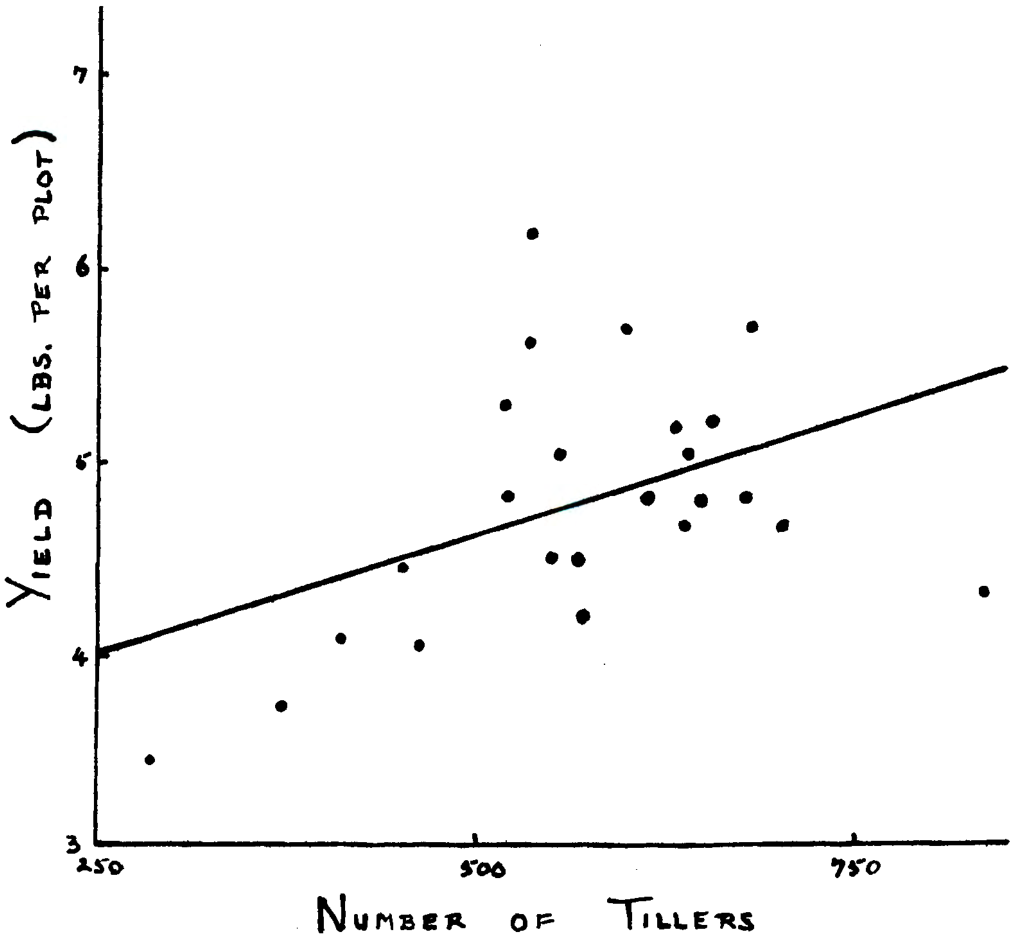
In the 1954 trial on oats with plots as small as 1 square yard, competition for soil nutrients must again be operating. In addition, the number of effective rows per plot varied from 3 to 6 because of the mechanical planting, so that competition within the plot will also be effective, and there are indications that the relationship is not linear but logarithmic. Plotting the points on a graph indicated that no relationship of practical use would be obtained. With plots 5 yards long along the rows, and 1 yard wide across the rows a significant relationship was obtained. Figure 1.4 shows the actual points together with the regression line

$$y = 3.37 + 0.0025n$$

where y is the yield per plot

and n the number of tillers per plot.

Fig. I.4. The relationship between yield and number of tillers per plot with oats in 1954 (Plot size = 5 yds x 1 yd.)



Even this significant regression, however, is not of much practical use, for the standard error of a plot yield after correcting for number of tillers is 0.60, giving a coefficient of variation of 12.5 per cent. When no correction is made for number of tillers, the coefficient of variation was 13.75 per cent (only 1.25 per cent higher).

These calculations would indicate that with small plots, at least, the variation in number of plants which occurs in practice does not greatly affect yield. The precaution of utilizing a given number of rows instead of a given area does not therefore appear to be necessary.

4.3 Lucerne

The design of the lucerne experiment was of the general basic type used in the remainder of the uniformity trials, having 432 basic plots of 6 square yards in area. The basic plots lay parallel to the planted rows. The seed was sown in rows 1 foot apart and in June 1954 the current growth was cut, but completely discarded because of defoliation by yellow spot disease. The first harvest to be weighed was taken on 11th and 12th August 1954. The mean yield per basic plot was 3.02 pounds dry matter, and the standard deviation was 1.12 pounds per plot (before eliminating the variation between rows and columns). The value of g_1 and g_2 to determine whether the distribution was sufficiently close to a normal distribution were calculated. Although the distribution was symmetrical (i.e. g_1 not significantly different from zero), there was a marked flattening of the curve (i.e. g_2 significantly negative). The smallest size of plot showing no significant departure from zero in the values of g_1 and g_2, was one of 36 square yards.

The second harvest to be weighed was cut on 12th and 13th October 1954. The mean yield was 3.14 pounds per basic plot and the standard deviation was 1.32. These results are in close agreement with those obtained in the previous harvest. Although the yields from the basic plots were symmetrically distributed, there was again a marked flattening, and the smallest size of plot giving values of g_1 and g_2 not significantly different from zero was again 36 square yards.

The third harvest was cut on 8th and 9th December 1954 after very poor short rains. The mean yield per basic plot was only 1.58 pounds dry matter, and the standard deviation obtained was 0.84 pounds per plot. Tests for normality were not made in this case, as it was felt, that although such information may be of some interest, its actual value was very limited.

The yields obtained from the three cuts were added together, to obtain the yields per basic plot over a period of about one year of growth. It has been suggested that totals over a period should be used when dealing with forage and pasture crops in section I.3, and not necessarily to harvest all plots on a given date, irrespective of stage development, and analyse each cut separately. The mean yield per basic plot was, of course, the sum of the means for the three separate cuts, i.e. 8.04 pounds, and the standard deviation obtained was 3.13 pounds.

If s_i denote the standard deviation of the i th cut, r_{ij} be the correlation coefficient between the i th and j th cuts, and s denote the standard deviation of the total, then

$$s^2 = s_1^2 + s_2^2 + s_3^2 + 2r_{12}s_1s_2 + 2r_{13}s_1s_3 + 2r_{23}s_2s_3$$

If there is little correlation between the yields obtained at the different times of cutting or if the correlation is negative, there will, in general, be a reduction in the coefficient of variation when the total is used. If the correlation is high and positive there will be little or no reduction. This can be easily seen by considering the special case when $s_1 = s_2 = s_3 = \sigma$ and $r_1 = r_2 = r_3 = r$. With no correlation

$$s^2 = 3\sigma^2$$

and the coefficient of variation will be 100 x $\sigma/m\sqrt{3}$, as compared with 100 x σ/m for each individual cut. When yields are perfectly correlated and $r_{ij} = 1$,

$$s^2 = 9\sigma^2$$

and the coefficient of variation will be 100 x σ/m, exactly the same as that for the individual cuts.

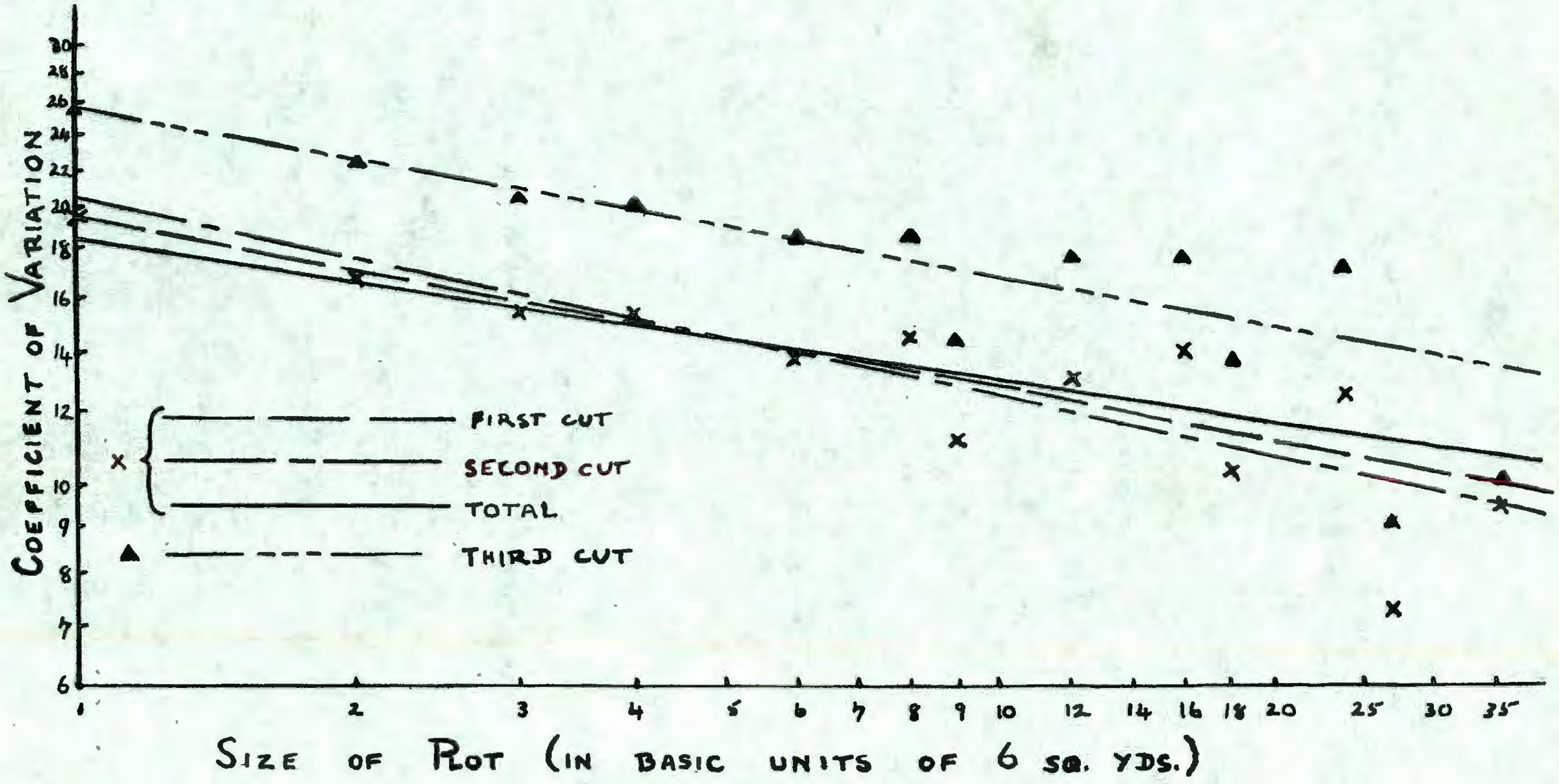
An estimate of the degree of association between yields may be obtained from the data given for the lucerne experiment. If $r_{ij} = r$ for all i and j,

$$s^2 = s_1^2 + s_2^2 + s_3^2 + 2r(s_1s_2 + s_1s_3 + s_2s_3) \quad ...(17)$$

Substituting in this expression those values given for the standard deviations of the total and the individual cuts, we find $r = 0.86$. As this represents the average correlation between plot yields in consecutive cuts as well as that between two cuts at some distance apart, this is indicative of a high association between cuts, and suggests that there will be virtually no reduction in the coefficient of variation when totals are considered. When the variation between rows and columns has been removed, the standard deviations obtained were 0.59, 0.65, and 0.48 for the individual cuts and 1.47 for the total. Substituting these values in equation (17), it is found that $r = 0.59$. Even though this is somewhat lower than that obtained before a large part of the soil heterogeneity had been removed, the reduction in coefficient of variation will not be very great when the number of cuts is as low as 3. The greatest reduction in the coefficient of variation will be obtained when two of the $r_{ij} = -1$, and the remainder $= +1$. In this case the reduction factor (i.e. the factor by which the coefficient of variation of an original cut must be multiplied to equalise it with that of the total) is $\frac{1}{3}$. When $r_{ij} = 0$, for all i and j, the reduction factor is $1/\sqrt{3}$, or about 0.6; and when r is as high as 0.6, the reduction factor is increased to almost 0.9, which presents very little improvement on an individual cut.

This is immediately obvious in Fig. 1.5. The results for the first and second cuts, and those for the total are very close together. So much so in fact that individual results are not given in these cases, but average points plotted for clarity, although

Fig. I.5. The relationship between coefficient of variation and plot size obtained with Lucerne (logarithmic scales)



the individual theoretical lines are given. Again it is obvious that the empirical linear law still holds. The equations expressing the relationship for the three individual cuts and the total, respectively, are

$$\log(C_x) = 1.286 - 0.18 \log(x)$$

$$\log(C_x) = 1.309 - 0.22 \log(x)$$

$$\log(C_x) = 1.408 - 0.18 \log(x)$$

and $$\log(C_x) = 1.262 - 0.15 \log(x)$$

Here the size of plot, x, is expressed in basic units of 6 square yards. The regression coefficients show good agreement, and there is no significant difference between those of the three individual cuts. It would appear that provided the yield is not abnormally low, as in the third cut after poor rains, the estimation of plot size can be made on a single cut, without involving any serious error.

The minimum sizes of plot estimated from the above equations are 35, 27, 180, and 57 respectively. As the results for the first and second cuts and the total are so close, a mean line was calculated from the within cuts sums of squares and products. The line obtained was

$$\log(C_x) = 1.236 - 0.18 \log(x)$$

The minimum size of plot calculated from this equation is 36.4 basic units, or about 220 square yards. This again is a manageable unit, but it is much larger than plots at present used in experiments with lucerne.

There was again no effect of shape of plot on variation.

4.4 Natural Regeneration

The design of the trial was of the general type having 432 basic plots of 6 square yards in area. The site was cleared in April 1952, and, in January 1953, all growth was cut back to ground level by panga (a large-bladed knife, very commonly used in East Africa). The first cut was taken in June 1953, and the second cut was taken on 19th and 20th May 1954, almost a year later. Before the third cut was taken in January 1955, part of the experiment was ploughed up by mistake, and had to be discarded. The third cut consisted of only 360 plots (i.e. 24 x 15) instead of the original 432 plots (i.e. 24 x 18). This smaller number of plots was also used when considering total produce.

The mean yields per basic plot for the three cuts were 5.09, 4.02 and 0.72 pounds dry matter respectively. The standard deviations obtained before removal of the variation between rows and columns were 1.57, 0.92 and 0.54, and those obtained after eliminating this variation were 1.18, 0.73 and 0.43. The mean yield of total produce per basic plot was 9.85 (which is not of course equal to the total of the three means due to the lower number of plots in the final cut). The standard deviations before and after removal of the variation between rows and columns were 2.28 and 1.86. Substituting these values in equation (17) the measures of the association of the yields at the different times of cutting were 0.44 and 0.36. These rather low values would suggest that there may be a reduction in variation when totals were used. This, however, was not attained as can be seen in Fig. I.6. The value of r obtained from substitution in equation (17), however, is equally dependent on the association between all cuts, and the expected reduction in variation is based on the assumption that the mean yields from all cuts are equal. The observed increase is therefore due to the very low yield obtained in the last harvest, and a higher association between the first two cuts, (an actual correlation coefficient of 0.51 was obtained). Fig. I.6 gives the actual coefficients of variation for different plot sizes (irrespective of shape), and also the calculated lines expressing the relationship between variation and plot size. Once again the empirical linear relationship appears to hold true. The lines, however, vary

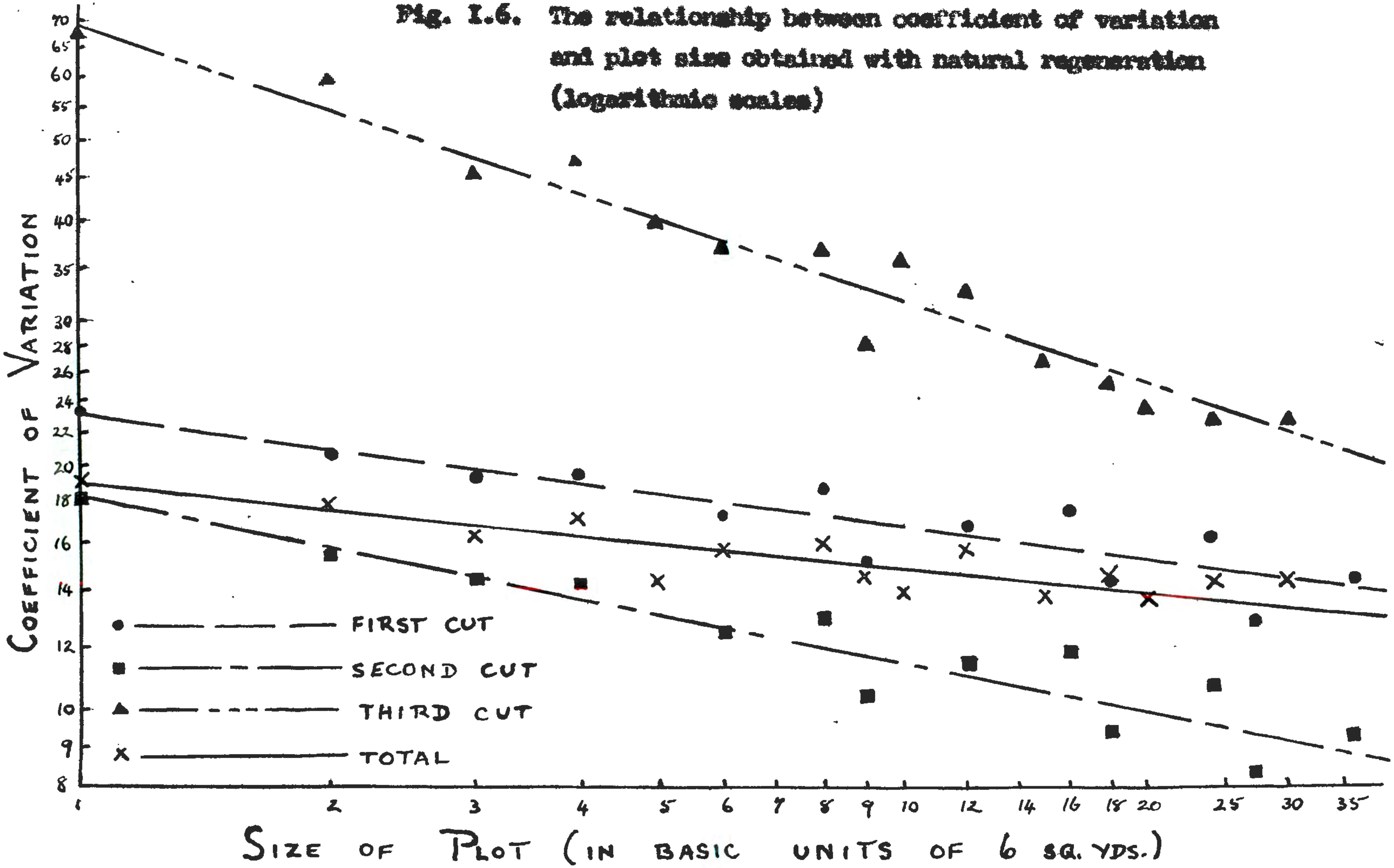


Fig. I.6. The relationship between coefficient of variation and plot size obtained with natural regeneration (logarithmic scales) considerably in slope; in other words, the value of the regression coefficient, a, is not constant. The equations obtained for the three cuts and the total respectively were

$$\log(C_x) = 1.360 - 0.14 \log(x)$$

$$\log(C_x) = 1.258 - 0.20 \log(x)$$

$$\log(C_x) = 1.839 - 0.34 \log(x)$$

and $$\log(C_x) = 1.275 - 0.10 \log(x)$$

It has been suggested that a should be largely dependent on the crop when variations between rows and columns have been removed, but these results do not appear to support this contention. The differences, however, may be due to differences in botanical composition of the herbage harvested. Unfortunately no botanical analyses were made on the plot, except initially before the first cut. Then the herbage was almost entirely couch grass (Digitaria scalarum), and with such a tenacious perennial weed, it is unlikely that the composition would alter materially between cuts. However, the area ploughed differed slightly from the remainder in its botanical composition, and for this reason, the regression equations for the first two cuts were re-calculated discarding the 72 plots. It has been pointed out that the removal of differences between rows and columns cannot remove all the differences in soil heterogeneity, and this may also be a contributory factor in the various regression coefficients. The two new equations for the first two cuts were

$$\log(C_x) = 1.374 - 0.11 \log(x)$$

and $$\log(C_x) = 1.244 - 0.14 \log(x)$$

If these two equations are substituted in place of the two original equations, there is then good general agreement between the regression coefficients, except for the third cut. In this case the value of a is considerably higher (significance greater than at the 0.1 per cent level of probability) than the remaining values. Too much reliability, however, should not be put on this estimate because of the low mean yield obtained. There was no significant difference between the values of 0.11 and 0.14 obtained for the first two cuts.

The minimum sizes of plot determined from the second set of equations are 2350, 51, 303, and 467 basic units of 6 square yards. Because of the general agreement between the equations for the first two cuts and the total a mean regression equation was determined. The equation was

$$\log(C_x) = 1.298 - 0.12 \log(x)$$

and the minimum size of plot calculated from this expression was 304 basic units.

4.5 Kikuyu Grass

The experiment using Kikuyu grass was of the same general design as the majority of the other uniformity trials conducted at Muguga. The grass was planted on 14th and 15th April 1953 by means of cuttings. No harvest was taken during the first year because the grass had not spread sufficiently to give a good cover. The first cut was taken in June 1954 towards the end of the long rains. The second cut was taken in October 1954, before the onset of the short rains, and the third cut was taken in February 1955, after very poor short rains. The mean yields per basic plot for the three cuts were 6.20 pounds, 2.95 pounds, and 0.97 pounds. These yields reflect the amount of water available for growth during the periods between the cuts. The standard deviations of the yield per basic plot, before and after eliminating the variation between rows and columns were 2.23 pounds, 2.23 pounds, and 1.00 pounds, and 1.65 pounds, 1.53 pounds, and 0.74 pounds.

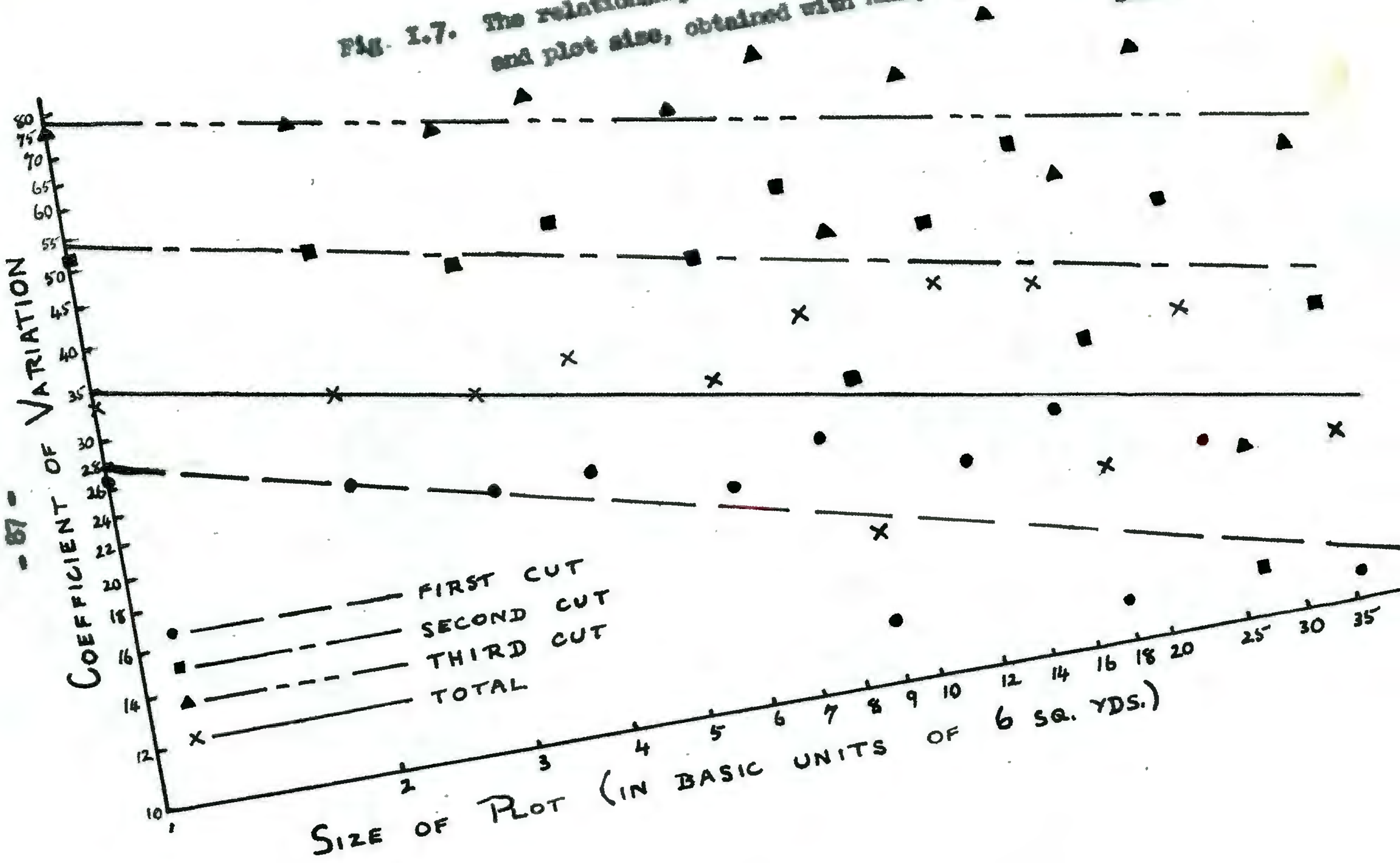
The values of g_1 and g_2 for the distribution of the yields from the basic plots in the first cut were 0.32 and 0.12. The latter did not differ significantly from zero but the former was significant at the 1 per cent level of probability. The positive value of g_1 indicates that the peak of the curve is at a point lower than the mean. The smallest plot for which the values of g_1 and g_2 did not differ significantly from zero was one 1yd. x 12yds. In the second cut, with the generally low yield, the zero limitation had an even more marked affect on the distribution, and a skew distribution was obtained. It is possible that this may have been a truncated normal distribution (see Hald, 1949), but this point was not further examined. The distribution of the yields of the basic plots in the third cut, when the mean yield was even lower, was also decidedly skew.

When the totals of the three cuts are considered the mean yield per basic plot is 10.12 pounds and the standard deviations before and after removal of the variation between rows and columns, are 4.92 pounds and 3.38 pounds. Equation (17) can now be used to determine the degree of association of the yields from the different cuts. The values of r obtained were 0.70 and 0.60 respectively. The value of r when variations between rows and columns have been removed is somewhat similar to that obtained for lucerne, so again the reduction in coefficient of variation when totals are considered should not be large.

In actual fact, the first cut gave a lower coefficient of variation than the total produce, as can be seen in Fig. I.7. This, however, is not surprising, due to the very much higher variation obtained in the second and third cuts. The lines in this figure are in general agreement with the plotted points giving the actual coefficients of variation, again indicating that the linear logarithmic relationship still holds. The differences from the lines, however, are greater than those obtained in the majority of the other experiments.

Shape of plot, itself, appeared to have little effect on the variation, but the differences from the straight line were due to different methods of combining the basic plots. This statement may appear contradictory, but the explanation is as follows. The effect of shape on coefficient of variation was tested by classifying the ultimate plots according to the ratio of length to width. Those having a ratio lower than 4.5

Fig. 1.7. The relationship between coefficient of variation and plot size, obtained with Kikuyu grass (logarithmic scales)



were examined in one group and those between 6 and 18 in another group. No real difference in the regression coefficient, a, was obtained. However, those plots, opposite sides of which consisted of the 6 yards basic plot length, gave very similar coefficients of variation irrespective of the number of basic plots in the ultimate plot; i.e. plots of 6 yds. x n yds., where the 6 yards always represents the length of a basic plot (and is not made of six 1 yard basic widths) and n is the number of basic widths (i.e. the number of basic plots in the ultimate plot), all gave similar coefficients of variation. Again with plots of 12 yards in length (i.e. composed of two basic units of 6 yards), although there was a reduction below plots of 6 yards in length, there was virtually no reduction as the width increased. This was true in the general case of a plot 6p yards in length and n yards in width; the coefficient of variation was virtually independent of n and merely decreased with increasing p. This would indicate that soil heterogeneity was affecting the coefficient of variation, but the majority of this has already been removed, by eliminating the differences between rows and columns. One is therefore forced to conclude that the association of herbage in close proximity irrespective of association of soil fertility is also quite high. In other words, the grass itself tends to be patchy in its growth, even though soil fertility may be fairly uniform.

This point is difficult to explain, and also very difficult to believe. It had originally been thought that Kikuyu grass should be one of the most uniform of the pasture crops dealt with for two reasons. In the first place it has been stated that the roots of Kikuyu grass can penetrate to considerable depths in the soil. Pereira (1953) records that the grass can reduce the first ten feet of soil to wilting point during the season. With such a large volume of soil nutrients available to the plants one would expect a uniform growth. Pereira also shows, however, that only about 10 per cent of the roots (by volume) penetrate below 5 feet, and only 3 per cent below 7 feet. In spite, therefore, of the roots being able to reduce the soil to wilting point to a depth of 10 feet, it is possible that only the roots in the top few feet can effectively utilise available water and nutrients. It therefore raises the point whether these roots are active. Secondly, the grass reproduces vegetatively by means of long runners. Any lateral movement of nutrients in these runners would tend to make growth more even; it would therefore appear that there is virtually no lateral movement.

When this experiment had indicated that the crop itself was extremely variable, it was noticed that in several cases, an extremely patchy growth was obtained with Kikuyu grass, while other grasses in similar habitats, did not exhibit such a marked difference in growth. This is by no means a carefully controlled comparison, but it does tend to support the somewhat surprising conclusion reached in this experiment. When Kikuyu grass is given water, growth is extremely rapid, and that part of the crop which starts growing, may maintain and increase its lead, due to the momentum set up in the movement of nutrients.

Controlled experiments, however, have been carried out on Kikuyu grass, and the results tend to support the conclusion reached here that the crop is very variable. A grazing trial (which will be described in greater detail in Part III) utilising Kikuyu grass was carried out at

Muguga. Two sizes of sample plot were used to estimate herbage; one was 7' x 1'4" and the other was 45' x 3'. The coefficients of variation obtained for estimates of the herbage prior to grazing were 30.7 per cent and 20.0 per cent respectively in the first trial. After a period of 15 days grazing by 11 steers the variation obtained was 35.1 per cent and 51.6 per cent. A second trial on the same area gave 33.4 per cent and 17.9 per cent for the coefficients of variation of the two estimates of the original herbage, and 37.3 per cent and 73.2 per cent for the final estimates after a period of 21 days by 4 steers. These figures indicate the large variation to be expected with Kikuyu grass.

The equations giving the relationship between coefficient of variation and plot size for the three individual cuts and the total in the uniformity trial are:-

$$\log(C_x) = 1.442 - 0.24 \log(x)$$

$$\log(C_x) = 1.727 - 0.19 \log(x)$$

$$\log(C_x) = 1.891 - 0.16 \log(x)$$

and $$\log(C_x) = 1.542 - 0.18 \log(x)$$

The regression coefficients are again in good agreement, and differences are not significant. The somewhat higher result from the first cut is associated with a higher standard error than either of the remaining cuts. The minimum sizes of plot estimated from these equations are 69.4, 7160, 256000, and 1100 basic units respectively. The second and third cuts can be omitted as the yields were low; the first cut then gives a minimum size of about 420 square yards, and the total a minimum size of about 6,600 square yards, i.e. about $1\frac{1}{2}$ acres. As the former is obtained from only one cut the result may be misleading, and one should rely more on the result obtained from the total. Here again, however, the result in this case may be misleading, because it is dangerous to extrapolate in estimated regression equations, particularly to such an extent. The results of this experiment are therefore inconclusive, but they demonstrate that extra care must be taken when dealing with this forage crop. A further experiment on another site is indicated, using a much larger size of basic plot in order to confirm the extreme variability of the crop, and in order to obtain a better estimate of plot size. If the results are confirmed, then any experiment using Kikuyu grass would require extremely large plots if any reliable conclusions are to be made, and may preclude the use of this crop in any small-plot experiments.

In the results given above for the grazing trial (to be discussed in more detail in Part III), it is possible to obtain estimates of the constants in the regression equation. The means of the coefficients of variation for the two sizes of plot used are 32.0 and 19.0 per cent for 1.037 and 15 square yards respectively. By substituting in equation 8,

$\log C_1 = 1.508$

and $a = 0.19$

$\therefore \log C_x = 1.508 - 0.19 \log_x$

The regression coefficient, a, is in very close agreement with those found from the uniformity trial. The size of plot giving a 10 per cent coefficient of variation would then be about 472 square yards, which is in reasonable agreement with that obtained from the first cut.

After grazing, however, the coefficient of variation of the larger sample plot is higher than that from the smaller plot. It is not possible therefore to utilise a relationship of the form given in equation (8). This demonstrates once again the unfortunate behaviour of the crop.

4.6 Star Grass

The design of the uniformity trial using star grass was of the same type as that used in the majority of the other uniformity trials conducted in this series. Cuttings were planted on 13th and 14th April 1953. As with Kikuyu grass no cuts were taken in the first year as the plants had not spread sufficiently to give a good cover. The first cut was taken on the 9th, 10th and 11th June 1954; the second on 4th and 5th October 1954, and the third on 24th and 25th January 1955. All these dates are very close to those on which the Kikuyu grass was planted and harvested. Unfortunately, the dry matter percentages of half the plots of the third cut could not be determined as the samples were destroyed by fire during drying. The results of the third cut are therefore restricted to only 216 plots (i.e. 12 x 18). It was of course necessary to utilise the same plots when considering totals, and the remainder were discarded.

The distribution of the yields from the basic plots in the first cut gave values of 0.20 and -0.57 for g_1 and g_2. The first of these is not significantly different from zero, but the second is, indicating a flat-topped curve. The smallest plot giving values of g_1 and g_2 which did not differ significantly from zero was one having dimensions 1 yd. x 18 yds. In the second cut both g_1 and g_2 were significantly positive, and the smallest plot giving a normal distribution had dimensions of 1 yd. x 36 yds. The normality of the distribution of the third cut and the totals was not tested.

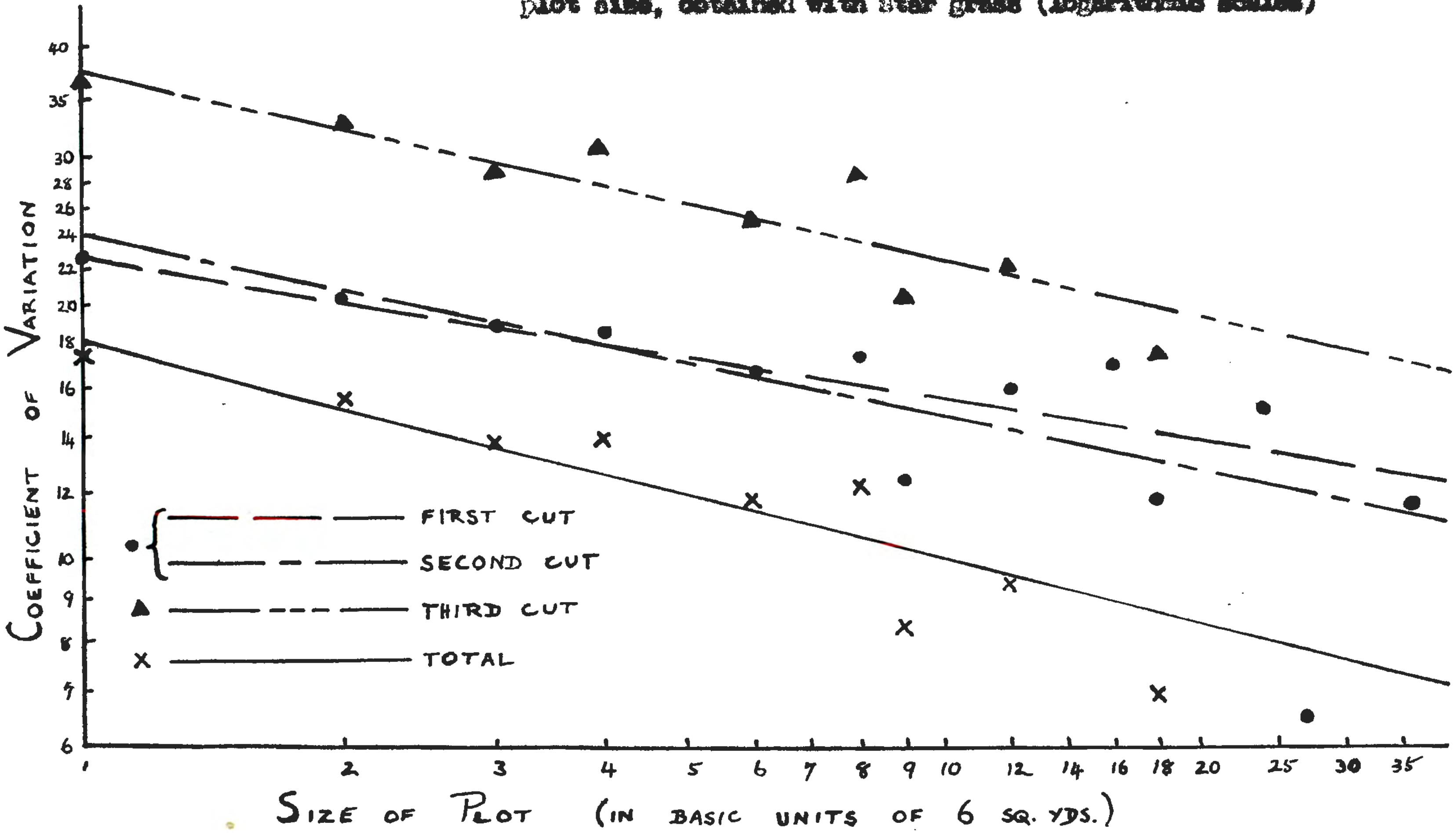
The mean yields obtained were 5.85 pounds, 4.15 pounds and 1.56 pounds dry matter per basic plot from each of the first three cuts, and the mean total yield was 11.77 pounds dry matter. The mean total is not the same as the total of the means in this case, due to the difference in the numbers of basic plots available for examination. The standard deviations obtained were 1.88 pounds, 1.30 pounds, 0.74 pounds and 3.58 pounds respectively, before the differences between rows and columns were eliminated. After removing these differences the standard deviations were reduced to 1.32 pounds, 0.96 pounds, 0.57 pounds and 2.04 pounds respectively. Using these values for the standard deviations of the individual cuts and the totals, and substituting in equation (17), the average correlation coefficients obtained were 0.73 and 0.23 before and after variation between rows and columns was removed. The low value of the latter would suggest that a certain amount of reduction in the coefficient of variation is to be expected. This can be seen in Fig. I.8.

Again the empirical linear relationship between $\log(C_x)$ and $\log(x)$ is demonstrated. The equations expressing this relationship are

$$\log(C_x) = 1.349 - 0.16 \log(x)$$
$$\log(C_x) = 1.379 - 0.21 \log(x)$$
$$\log(C_x) = 1.573 - 0.22 \log(x)$$
and $$\log(C_x) = 1.256 - 0.26 \log(x)$$

These four lines are shown in the figure but the actual coefficients of variation obtained with different plot sizes in the first and second cuts were so close that only the means are plotted. There is again no statistically significant difference between the regression coefficients of the three individual cuts. From these equations the estimated minimum sizes of plot are 146, 60, 351, and 9.8 respectively. There is therefore a decided improvement in this case, when totals are considered,

Fig. I.8. The relationship between coefficient of variation and plot size, obtained with Star grass (logarithmic scales)



and a plot of 60 square yards only is required. The third yield was small and therefore a minimum plot size higher than that generally necessary was obtained. There was a rather large difference between the sizes estimated from the first and second cuts, but as the actual coefficients of variation were close, an average line for the two cuts was determined. The equation of this line was

$$\log(C_x) = 1.364 - 0.19 \log(x)$$

and the minimum size of plot estimated from this equation was 87.7, or about 530 square yards.

Although actual plot shape appeared to have little effect on the coefficient of variation, a result somewhat similar to that obtained with Kikuyu grass was noticed. In other words when the size of plot was increased by adding on further basic plots to the length there was a reduction in variation, but when the size was increased by adding onto the width, very little reduction was obtained. In this case, however, the effect was not so marked as when Kikuyu grass was used.

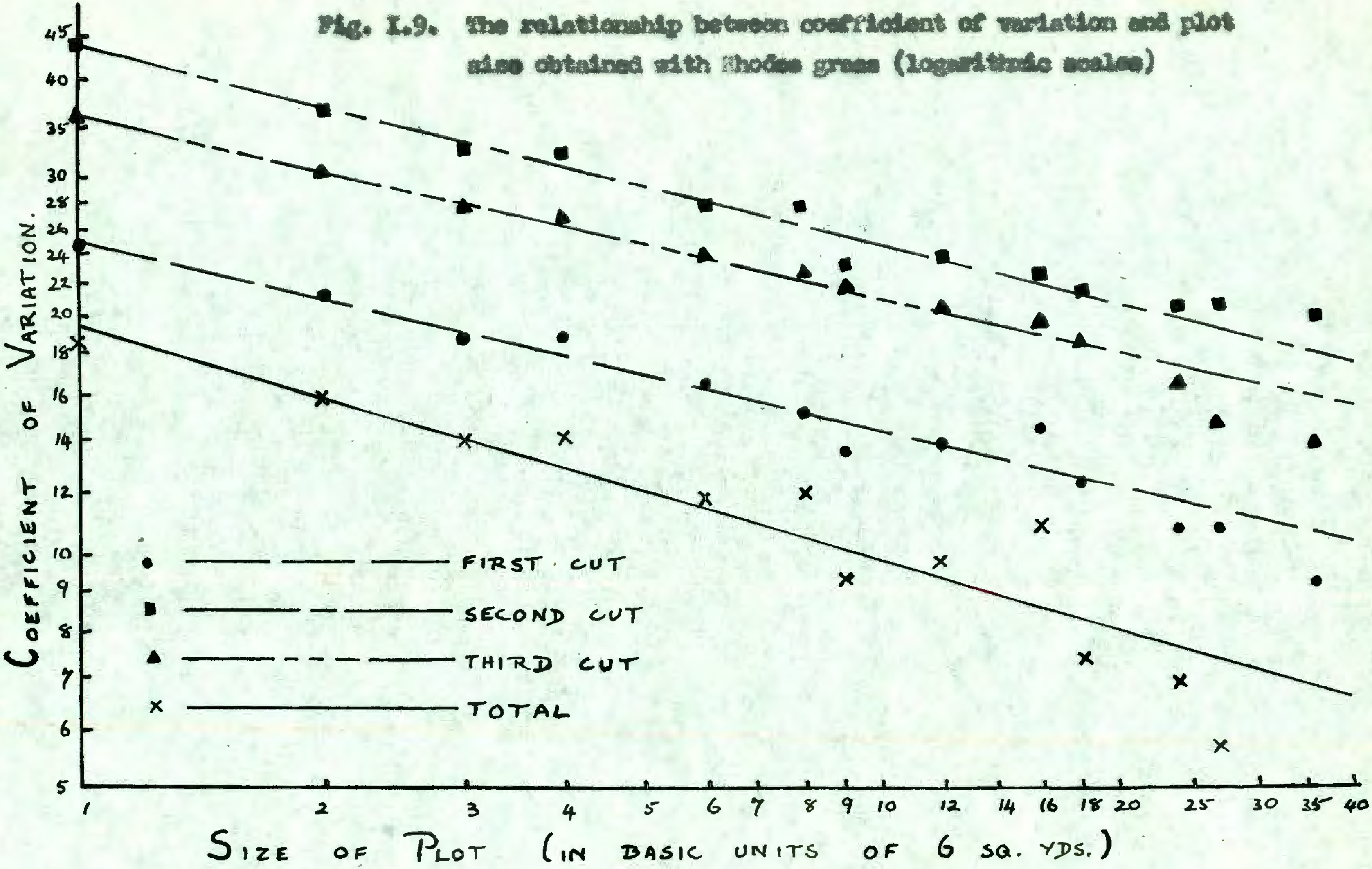
4.7 Rhodes Grass

The experiment using Rhodes grass was of the same general design as the majority of the other uniformity trials. Eight pounds of seed was sown broadcast over the area on 14th April 1954, and cuts were taken on 18th and 19th August 1954, after heavy long rains, on 25th and 26th October 1954, before the short rains were due, and on 12th and 14th January 1955, after very poor short rains. The distribution of the yields of the basic plots of the first cut did not differ significantly from a normal distribution. The values of g_1 and g_2 obtained were -0.16 and -0.34 respectively. The distribution of the yields in the second cut showed marked skewness for the value of g_1 was significantly positive indicating a peak in the distribution at a point lower than the mean. This was probably due to the zero limitation with such low yields, and possibly a truncated normal distribution (see Hald, 1949) would have given reasonable agreement. This point, however, was not tested because it was felt that this line of investigation would not be profitable. The smallest plot having a distribution not differing significantly from zero had dimensions 3 yds. x 18 yds.' The third cut again suggested that the zero limitations in yield may result in a truncated normal distribution.

The mean yields of dry matter per basic plot for the three cuts were 4.54 pounds, 0.85 pounds, and 1.09 pounds respectively. Before eliminating the variation between rows and columns, the standard deviations of the basic plots were 1.40 pounds, 0.47 pounds, and 0.71 pounds. When soil variation was eliminated the standard deviations were 1.11 pounds, 0.37 pounds, and 0.39 pounds. The mean yield per basic plot, when totals were used, was 6.47 pounds, and the standard deviations before and after eliminating differences between rows and columns were 1.78 pounds and 1.19 pounds respectively.

Using these values for the standard deviations of the individual cuts, the values of the average correlation coefficients estimated from equation (17) are 0.13, and -0.05, before and after eliminating variation between rows and columns. There is therefore little association between the yields of the different cuts, and some reduction in the coefficient of variation can be expected when totals are considered. This is shown in Fig. I.9.

Fig. 1.9. The relationship between coefficient of variation and plot size obtained with Rhodes grass (logarithmic scales)



Again the empirical linear relationship is demonstrated, and the equations expressing this relationship for the individual cuts and the total are

$$\log(C_x) = 1.594 - 0.24 \log(x)$$

$$\log(C_x) = 1.641 - 0.25 \log(x)$$

$$\log(C_x) = 1.554 - 0.23 \log(x)$$

and $$\log(C_x) = 1.284 - 0.30 \log(x)$$

The regression coefficients are extremely close, and there are of course no significant differences between those obtained for the three individual cuts. From these equations the estimated minimum sizes of plot are 42, 382, 241, and 9.1 basic units of 6 square yards. It is evident that the second and third cuts gave too low a yield to put any reliability on the estimates of plot size. If the first cut only is considered, the minimum size of plot would be about 255 square yards. In pasture research, however, the herbage is harvested over a comparatively long period, and single cuts in themselves do not mean a great deal. In determining the plot size for experimentation, therefore, the total should be considered, and this gives a minimum plot size of about 55 square yards.

Plot shape had no effect on variation.

4.8 General Discussion

The results from the different trials have been given in the preceding sections, but very little discussion on the different points was made. It is obvious that the empirical linear relationship between the logarithms of variation and plot size still holds when differences between rows and columns are eliminated. This is, of course, essential in the method. When these differences are eliminated it is not necessary to introduce the hypothetical infinite field of Fairfield Smith. Although this gives a nice mathematical exercise its practical use is extremely limited. In addition it is innocuous to introduce hypothetical concepts of this type into what is essentially an empirical discussion.

By eliminating the differences between rows and columns, a great deal of the variation due to soil heterogeneity will be removed, and the importance of shape of plot will be greatly reduced. In the trials reported here, shape has in fact had no effect on variation because of this. In general, of course, shape of plot will affect variation. Some earlier workers (see Section I.2.1) have found that the shape had little effect, but the majority have found that the long narrow plot is generally better. In some cases, however, the square plot has been less variable than the long plot, due to the orientation of the latter along the contour lines of soil fertility. If information exists on the fertility or water gradient on the experimental site, then in general it would be better to use long plots cutting across the contour lines. If no information is available, long narrow plots may, with an unfortunate orientation, give a larger variation than would have been obtained from plots more nearly square. In such circumstances it would then be a matter of which shape was most convenient.

Incidental information obtained from this investigation is that it is unnecessary to use square plots in a Latin square design. The shape, in fact, appears to be immaterial, and should depend entirely on what is most convenient from the practical point of view. This should, in any case, be apparent, but some workers appear to think that a square plot is necessary.

It has been suggested that the removal of the variation between rows and columns should make the regression coefficient, a, largely dependent upon the crop.

The improvement in agreement between the two values obtained has already been mentioned but further evidence is required. The general agreement between the three coefficients for individual cuts, and also that for the total of any one of the perennial type of forage crops is no criterion, for the soil remains the same throughout, and if a were entirely dependent on soil heterogeneity it would still remain constant.

Before the present method was used, largely eliminating soil differences, the regression coefficients for the two first cuts on all trials were determined. These are given in Table I.3. The table also shows the regression coefficients determined for the individual cuts after eliminating the differences between rows and columns. A weighted mean of these regression coefficients for each crop is given in the fourth column, and the last column gives the regression coefficient for the total produce. During the discussions on the individual experiments, it has been stated that where the yield is low, less reliance should be placed upon the results, because the coefficient of variation tends to increase when yields are small. The mean regression coefficients

Table I.3

The regression coefficients obtained in the equations expressing the relationship between the logarithms of coefficient of variation and plot size, before (row A) and after (row B) eliminating differences between rows and columns.

Crop		First	Second	Third	Mean value	Total produce
Oats	A	.32	.33	-	.22	-
	B	.23	.34	-	.28	-
Lucerne	A	.11	.11	-	.11	-
	B	.18	.21	.18	.19	.15
Natural regeneration	A	.23	.17	-	.20	-
	B	.11	.14	.34	.14	.10
Kikuyu Grass	A	.19	.15	-	.18	-
	B	.24	.19	.16	.22	.18
Star Grass	A	.13	.19	-	.15	-
	B	.16	.21	.22	.19	.26
Rhodes Grass	A	.21	.20	-	.21	-
	B	.24	.25	.23	.24	.30

have therefore been weighted according to the mean yield per basic plot.

The agreement between the coefficients of the individual cuts is not improved except in the case of oats. This, however, is not surprising because the soil remains the same in the other experiments. What is surprising, however, is that the mean regression coefficient appears in general to be independent of the type of forage crop for a single cut. It is reasonable to assume that the variation of different forage crops should behave in a similar manner, and these results tend to support the contention that, by eliminating rows and columns the regression coefficient, a, tends to become dependent mainly on the crop. There are two main exceptions to this general rule; the regression coefficient for oats is somewhat higher than the remainder and that for natural regeneration is somewhat lower. It is possible to give an explanation for this difference, however. It must be remembered that the higher the value of a, the more nearly random the distribution of plot yields on the ground. With low values of a there is more marked association between neighbouring plots (and NWF a more uniform distribution as suggested by Wasson and Kalton (1953)). With natural regeneration the development of plant communities will mean more association between neighbouring plots, and therefore the value of a should be lower. With oats, which were drilled, and grown only for a short period, there will be less time to develop association and the regression coefficient should be higher. Even with the three grasses which show good agreement between the values of the regression coefficient, the same type of effect is also noticeable; the seeded grass (Rhodes) gives a higher value for a than the two planted grasses.

Data given by Wasson and Kalton (1953) for bromegrass were also examined in order to obtain more confirmatory evidence. In 1950 two fields were sown, one broadcast and one in rows. Before eliminating differences between rows and columns, the regression coefficients obtained by Wasson and Kalton were .14 and .23 respectively. When soil variations were largely removed the coefficients became .24 and .28, which are in much closer agreement.

Although there is good agreement between the mean regression coefficients from individual cuts of the various forage crops, the coefficients obtained for the total produce vary considerably, from .30 with natural regeneration to .30 with Rhodes grass. With the former it would appear that plots with a high yield on one occasion tend to have high yields at other cuts, whereas with the latter a high yield on one occasion results in a comparatively lower yield from the same area at a subsequent harvest.

The minimum sizes of plot determined from the regression equations vary considerably, not only between the various crops, but also between the different harvests of a given crop. These latter differences are in general due to estimates obtained from poor yields, and more reliance should be placed on estimates from harvests giving high yields. In order to obtain a single estimate of the minimum size of plot for each type of forage crop, weighted mean regression equations were determined from the three individual cuts. The weights given to each of the individual expressions were equal to the mean yields per basic plot and the following equations were determined:-

Lucerne: $\log(C_x) = 1.324 - 0.19 \log(x)$

Natural Regeneration: $\log(C_x) = 1.354 - 0.14 \log(x)$

Kikuyu Grass: $\log(C_x) = 1.568 - 0.22 \log(x)$

Star Grass: $\log(C_x) = 1.390 - 0.19 \log(x)$

Rhodes Grass: $\log(C_x) = 1.453 - 0.24 \log(x)$

Plots giving a 10 per cent coefficient of variation calculated from these equations will be approximately 310, 2030, 2290, 680, and 460 square yards, respectively.

The plots required for natural regeneration and Kikuyu grass are excessive, and because of the extrapolation required, no great reliance should be placed upon them. The indication is, however, that very large plots are required for any conclusive results to be obtained in experimentation. This is a serious handicap, particularly as Kikuyu grass is at present regarded as one of the best types of pasture in East Africa. It is causing some anxiety at the moment (Dougall, 1953 and 1954) due to a comparatively rapid fall-off in production, possibly caused by a matting of the stoloniferous growth near the soil surface. From the results of the uniformity trial, it would appear that any conclusive evidence in support of some particular cultivation method will be difficult to obtain. It is not surprising, therefore, that little is known about the cultivation of this grass for obtaining the best results.

The plot sizes required for the remaining three forage crops are not so excessive, and can be utilised without serious practical difficulties, although the scope of an experiment will be somewhat limited. The regression equations for these crops show very good general agreement, and the mean regression equation was found to be

$$\log(C_x) = 1.389 - 0.21 \log(x)$$

This gives a minimum plot size of about 430 square yards. As this covers several types of forage crop, it would appear that this size could be given as a general recommendation for work in East Africa.

Using the minimum size of plot, it has been shown in Section I.3 that 8 replications are required to prove a difference of 10 per cent significant. In a 2^3 experiment, this would mean a total of 64 plots covering an area of about 5¾ acres. In general this would probably be too large to use in practice. Similarly, in a 3^3

experiment one replication would require 27 plots on about 2¼ acres. Eight replications would then cover 18 acres, which is far too large an area to be practicable. It has been pointed out however, that the 8 replications required can include "hidden" replications. In a single replication of the 2^3 experiment the main factors will be replicated 4 times. In order to obtain sufficient degrees of freedom for a reliable estimate of error, however, 3 replications are required, which means that the main factors will be replicated 12 times, and the terms for determining the two-factor interactions will be replicated 6 times. This degree of replication should therefore be sufficient. In a single replication of the 3^3 experiment, the main factors will be replicated 9 times, and there will be a 3-fold replication of treatments required for the two-factor interactions. In a preliminary trial therefore one complete replication is probably all that is necessary, but if more precise information is required on interactions, two or three will be necessary. The use of the minimum plot size, and the stipulation that 8 replications are required, will not therefore greatly affect the design in practice.

The results from the uniformity trials suggest that, in general, an examination of a single harvest will not be very profitable, particularly if yields are poor. Yields over a year, or a longer period, will be more meaningful, and should, in general, be used. This would suggest that estimates based on the total of the three cuts should be used. Again natural regeneration and Kikuyu grass give excessive sizes. The results obtained with Lucerne suggest a plot size very little different from that determined from the mean regression equation for individual cuts. With Star and Rhodes a considerable reduction to about 60 square yards is suggested. In early experiments, however, it may be unwise to go as low as this in case inconclusive results are obtained.

5. General Application

It has been stated that the results obtained by the method suggested can be applied in general to experiments designed for other sites, because of the dependence of the regression coefficient mainly upon the crop. If it is largely dependent also upon the soil heterogeneity of the site chosen for the uniformity trial, the results, although indicating what is liable to occur, cannot be applied in practice in other areas.

It has been shown that the mean value for a obtained in these trials is 0.21, and general agreement between the different pasture crops was good. The mean of the two values obtained by Wassom and Kalton (1953) for bromegrass was 0.26 after variation between rows and columns was removed. The value obtained by Fairfield Smith (1938) from data of Davies (1931) on natural grassland in Australia was .19 and from data of Stephens and Vinall (1928) for sorghum was .21. These values do not differ greatly from the mean for the Muguga Experiments. It seems safe therefore to take this value as generally applicable to forage crops in East Africa, and there may be little serious error involved if it were assumed to be generally applicable. It is not therefore necessary to have any precise knowledge of the value of a in the area to be used for the experiment. The general equation representing the relationship between variation and plot size in pasture experiments then becomes

$$\log(C_x) = A - 0.21 \log(x) \qquad \ldots\ldots(18)$$

If no data are available for the determination of A from experiments in the neighbourhood, then the general minimum plot size of about 430 square yards calculated in the previous section can be used. If data are available for the determination of A, then this value can be substituted in the equation, and a better estimate of the minimum plot size should be obtained for any given area. An experiment on the selected crop in the area can be used to obtain the coefficient of variation for the size of plot used, and by substituting in equation (18), a value of A will be determined. For example, a fertiliser experiment conducted at Muguga on star grass used plots of 36' x 17' (or 68 square yards). The yield from one cut was 53.02 pounds dry matter per plot and the standard deviation obtained was 8.58 pounds, or 16.18 per cent of the mean. Taking the plot used as the basic size of plot (for any alteration in the scale of units of area will not affect the value of the regression coefficient) and substituting in equation (18)

$$A = 1.209$$

For a coefficient of variation of 10 per cent therefore,

$$1 = 1.209 - 0.21 \log(x)$$

i.e. $\log(x) = 0.995$

or $x = 9.89$

This gives an area of approximately 670 square yards (i.e. 9.89 x 68).

A further experiment, also on star grass, used plots of varying sizes. Plots 4⅓' x 7' (or 3.37 square yards) gave a coefficient of variation of 18.84 per cent. Again taking the plot used as the unit plot and substituting in equation (18),

$$A = 1.275$$

For a coefficient of 10 per cent

$$\log(x) = \frac{0.275}{0.21} = 1.3095$$

or $x = 20.4$

This gives an area of approximately 70 square yards. The sizes of other plots used in the experiment were 91' x 7', 13' x 7', and 6½' x 7' (or 70.78, 10.11 and 5.056 square yards). The coefficients of variation for these different sizes were 12.11, 18.76, and 15.91 respectively. By substituting in equation (18) and the further necessary calculations, the sizes of plots giving a 10 per cent coefficient of variation were, respectively, about 180, 200, and 50 square yards. The mean of these four estimates is 125 square yards.

The first experiment gave a minimum size somewhat higher than that already determined from the uniformity trials, and the second experiment gave one somewhat lower. The mean of the two estimates, however, is 400 square yards which is in close agreement with the 430 square yards calculated in the previous section. These results suggest that estimates of A from only one experiment in the vicinity may not be sufficiently accurate and that as many as possible should be used.

PART II

BOTANICAL ANALYSIS

II.A. Introduction

The importance of determining the botanical composition of herbage available for grazing has been recognised for some time and a great many techniques have been developed during the past 40 or 50 years, particularly since 1930. The vast amount of literature on the subject precludes any comprehensive review of past work in this report, but three writers (West, 1937, Goodall, 1952 (a) and (b), and Brown, 1954) have given lengthy and detailed reviews, which, read in conjunction, give a balanced and almost complete summary of the work to date. It is therefore fortunately unnecessary to give any but the briefest of reviews here, and reference will be made only to those papers having a direct bearing on the argument.

The many different techniques which have been used to determine the composition of pasture may be broadly classified under three main heads: numbers of individuals, area covered, and weight. Brown (1954) in her discussion of the subject gives in addition a fourth classification - "frequency" of occurrence, first suggested by Raunkiaer (1909). In this connection "frequency" is limited to a conception of either absence or presence and takes no note of the actual numbers of individuals present in the sampling unit. It is an unfortunate use of the word and has led to a certain amount of confusion, for it can be used with an entirely different, yet more precise, meaning, when discussing numbers of individuals. When used in this special sense the word will be used in quotes.

This fourth method of estimating botanical composition, however, can be regarded as a sub-division of the first class suggested above, for it is in effect the simplest method of counting. It is much quicker than counting all individuals present, and at the same time, ignores any difficulties in defining an individual. For these reasons, it is not surprising that it was one of the first methods used in botanical analysis. In addition to these advantages, the method is also objective, and Brown (1954) suggests that "frequency" readings may be converted to an estimation of the numbers of individuals, and give the exact information required for critical studies of vegetation changes. Such a conversion, however, would be possible only if the population were dependent on one parameter. With two parameters, two sets of sample units must be counted - those containing no individuals of the species, and those containing one individual as well as noting the total number of sample units used. It would then be possible to determine the population parameters, if a sufficiently large sample were taken. With a small sample the inherent random variation in numbers of sample units in the various classes may give completely misleading information. Such a conversion, will, in any case, depend on an exact knowledge of the distribution, and so far no proposed distribution can claim to have universal application. Any such conversion therefore would not necessarily lead to any improvement, and, in fact, cannot lead to any improvement, for any body of data contains a definite limited amount of information, and no mathematical manipulation of the data can increase that amount of information. This suggestion by Brown is an example of the very common fallacy that statistical methods can improve the quality of the data collected.

Goodall (1952 (a)) attacked the use of "frequency" on similar lines. He pointed out that "the main errors in Raunkiaer's early work stem from the assumption that "frequency" can in any way provide a measure of the quantity of a species present. The "frequency" of a species depends not only on its quantity but also on its distribution". Because of this dependence on density and distribution, "frequencies" are in no sense additive, and consequently present grave difficulties in any statistical analysis or interpretation. Goodall states that "it has been suggested that "frequency" is not a characteristic of vegetation at all". This is probably going a little too far, for it does reflect certain characteristics such as density, distribution, and size of individuals (as well as the size of quadrat used), but it combines them into such a complex measure, that it is not possible to use "frequency" as a description of the vegetation. Goodall states that Raunkiaer realised the inadequacy of "frequency" in his later papers; it is a pity that later workers have not also realised its serious shortcomings.

As a fore-runner of present techniques it is interesting from the historical point of view, but it is time that it died a natural death. Penfound (1945) ably sums up what should be the present attitude to this method: "apparently 'frequency' is the most artificial and the least important of the three concepts of frequency, density and cover. It has been used entirely too often and for too many purposes in the past two decades and should be employed much less in the future".

In the following sections, a consideration of "frequency" is therefore ignored, and attention is directed to numbers of individuals and area covered. At present ecologists have accepted none of the proposed distributions for numbers of individuals, but it is hoped that the one suggested here, which appears to fit a number of distributions will be accepted. As regards area covered, the method proposed by Levy and Madden (1933) is critically examined, and a comparison with other methods made.

II.B. Number of Individuals

1. Historical

A great deal of time is consumed in counting numbers of individuals in each sampling unit, and this is the main criticism of the method, for it cannot readily be used for extensive surveys or when large numbers of experimental plots are involved. In order to overcome this difficulty the "frequency" method already referred to was used, and other methods described by West (1937) and Brown (1954) have also been tested. These in general depend on eye estimates of the density which are recorded in percentages, or degree of presence, by such terms as abundant, common, and rare. In the first place such measurements are subjective, and therefore suffer from all the disadvantages of such measurements, which are now too well known to require repetition here. In addition, the use of qualitative descriptions makes any exact knowledge of the vegetation extremely difficult. It is of course possible to give some arbitrary system of scoring to the qualitative classes. It is claimed (Acocks, 1953) that this has been done with some success in a recent survey in South Africa. The survey covered the whole of the Union and is almost exclusively a descriptive survey. Acocks suggests that "the arbitrary numerical values of the habitat symbols can be modified to suit individual lists". If this is done any quantitative comparison is meaningless. The qualitative classes in this survey were based on the distance apart of individuals. This method has been suggested by various other workers (Cottam and Curtis (1949), Numata (1954), Hopkins and Skellam (1954) and Clark and Evans (1954)) as a quantitative measure of plant distributions. The method appears to be tedious and does not give such valuable information as counting. Hopkins and Skellam have attempted to improve the value of the data by determining theoretical distributions for these linear measurements. Unfortunately, however, they base their arguments on the random distribution of a plant population. Where distances are actually measured some quantitative value may be attached to habitat symbols to clarify their meaning, but in the survey carried out by Acocks the distances were estimated by eye, which again throws some doubt on the validity of attaching any quantitative measurement to the classes.

As early as 1898 Pound and Clements stated that although it may appear "a simple matter to pick out the several species and to classify them in the several grades of abundance with reasonable accuracy ... to ensure accurate or even approximately accurate results, it is necessary to resort to some method of actual count". Subjective estimations are still used, unfortunately, but in view of the serious criticisms which may be levelled against them, they will not be considered in this report.

In dealing with the density of individuals, besides knowing the mean of the population it is necessary to have some idea of the appropriate distribution before any accurate comparison between different samples can be made. It was briefly mentioned in Section II.A. that there is as yet no recognised form of distribution applicable to vegetation studies.

Originally it was thought that the distribution of individuals of species should be at random, resulting in a Poisson curve, and several workers based their methods on this assumption. In one of the first critical analyses of plant counts, Blackman (1935) attempted to fit the Poisson curve to a number of distributions, and other workers followed his example. In the majority of cases, however, it was found that significant differences existed between the expected and actual frequencies. (Frequency is here used in its more precise sense and no quotes are used). In nearly every case the plants tended to be grouped together and the community was termed "over-dispersed". (This again is a confusing term and "aggregated" will be used instead). When plants are grouped in this manner, compared with the Poisson distribution, there will be an excess of quadrats containing no individuals and an excess containing a comparatively large number, while the middle class frequencies will consequently be reduced. In spite of the general indication that a species is not distributed at random, workers (Greig-Smith (1952) and Cottam et al (1953)) still discuss methods of sampling random populations. Because of the obvious difficulty in obtaining a random distribution in practice, both of the references given used artificially made up communities in the laboratory. As the methods cannot be applied in practice, hypothetical discussions of this type appear at best, to be a waste of time and, in addition may be very misleading.

When it was recognised that most species were not randomly distributed, attention was directed to the degree of non-randomness, and various methods for measuring this have been suggested. The ratio of the variance to the mean was used by Blackman (1942) as the "coefficient of dispersion". For a Poisson distribution the variance and the mean are equal, so that a coefficient of dispersion of unity indicated randomness. Archibald (1950) used this measure and obtained large differences for the same species on the same area with different quadrat sizes. Fracker and Brischle (1944) used a distribution factor $d = (m - n)/n^2$, where m is the actual mean number of individuals per quadrat, and n the estimated mean number of individuals which should be present on an area having the recorded number of zeros, if the distribution were random. Other workers have used m/n as a "factor of heterogeneity". Numata (1955) proposed yet a further measurement which he termed the "coefficient of homogeneity", and Moore (1953) suggested the use of $\phi = 2n_0n_2/n_1^2$, where n_i is the number of quadrats containing i individuals. David and Moore (1955) later suggested an "index of dispersion" defined by $I = s^2N/m$ where s^2 is the calculated variance, m the mean, and N the number of observations.

There is little point in discussing the various merits of these proposed methods for measuring departure from randomness, for it is clear that the view of the earlier ecologists that the individuals in natural vegetation are randomly distributed must be completely discarded. It is not sufficient merely to know that there is a significant departure from a random distribution. This has occurred so often that the point no longer requires testing. It is necessary now to direct attention to the actual form such departures take. Any given value of one of the suggested measurements could cover a great variety of distributions, and widely different values could represent the same type of distribution. This is immediately obvious for example with the method proposed by Moore. If there are three distributions in which the number of quadrats with no individuals, one individual, and two individuals are respectively 24, 5, 4, and 20, 6, 2, and 4, 5, 24, then the values of ϕ obtained will be 7.68, 2.22 and 7.68 respectively. In this case the first two distributions are similar but give widely different values for ϕ, while the last distribution is completely different, but gives a value for ϕ identical with the first.

It is obvious therefore that these coefficients, suggested by the various workers cannot be used for any comparison purposes, and this, after all, is the main reason for collecting data. If, however, a knowledge of the form of distribution is available, comparisons can be made.

One of the first serious attempts to break away from the assumption of a random distribution was made by Archibald as late as 1948. She found that in some cases where the Poisson series did not fit a set of data, that the contagious distribution described by Neyman (1939), gave satisfactory agreement. This distribution was originally determined to describe the distribution of insects and bacteria and was successfully used by Beal (1940) when examining the distribution of insect larvae. It is characterised by the possibility of several modes, but unless such a set of modes is demonstrated one may reasonably feel reluctant to use the series. It is, however, a reasonable suggestion as a beginning, but Archibald found several species which did not conform to this contagious type of distribution. It had also been used earlier by Fracker and Brischle (1944), who found that the agreement with the actual data was not good.

In view of the unsatisfactory results obtained by the use of Neyman's contagious distribution, a further non-random distribution was developed by Thomas (1949) with special reference to plant ecology. It was assumed that groups of plants were distributed at random, and that the number of plants in each group was 1 plus an observation from another Poisson series with a different mean. This series was found to fit some distributions very satisfactorily, and was used by Archibald in a later paper (1950). No evidence, however, was given to suggest that the form of distribution of numbers of individuals in groups was of the type suggested, so that there is also some reluctance to use this series. A further objection to the series arises in the discussion by Archibald (1950). Two sizes of quadrat were tested on the same area, and in most cases the larger size of quadrat did not contain a larger number of groups, with the number of individuals per group remaining fairly constant, as one would expect. It was suggested that the discrepancy arose because groups included within themselves smaller groups. This argument, if continued, reverts to the individual, and little progress appears to have been made.

2. The Negative Binomial Distribution

These two attempts to determine the form of plant distributions have not proved successful, and obviously another approach is required. The first assumption to be made is that individuals are not randomly distributed. This needs no further evidence for support. In other words the plants are aggregated together in some way to form groups. The "group" may consist of a single individual or a larger number of individuals, but cannot of course contain no individuals. This led Thomas (1949) to make the rather artificial assumption that the number of individuals in a group would be 1 plus an observation from a Poisson series. Such an artificial assumption, however, is not necessary. In cases where the null class cannot be observed, as in this example where groups of no individuals cannot be recorded, it has been found that many ecological populations follow the logarithmic distribution first given by Fisher et al. (1943). In this distribution, the probability of obtaining n individuals in a group is

$$\alpha \frac{x^n}{n}$$

where $\alpha = -\dfrac{1}{\log_e(1 - x)}$

If, in addition, it is assumed, as Thomas did, that the number of groups per quadrat follows the Poisson series with a mean m, then the probability of obtaining p groups in a quadrat is

$$e^{-m}\frac{m^p}{p!}$$

By combining these two distributions it is possible to show (Quenouille, 1949) that the resultant distribution is the negative binomial, with a probability of obtaining s individuals in the quadrat given by

$$P_s = (1 - x)^{\alpha m}\frac{(\alpha m + s - 1)!}{(\alpha m - 1)!s!}x^s$$

which is the general term in the expansion of

$$y^{\alpha m}(1 - x)^{-\alpha m}$$

where $y = 1 - x$

The second assumption possibly needs further consideration before it can be accepted. When the first individuals of the species were introduced into the area from outside, the seeds travelled some distance, and it may be assumed that they were scattered at random. Subsequently colonies have been built up around these initial parent plants. It does not therefore seem unreasonable to assume a Poisson distribution for these colonies.

Goodall (1952 (a)) has criticised Thomas' assumption on the basis that it means a whole group must be included in a quadrat. The same criticism could be made against the present argument, if it is considered a valid criticism. To be strictly correct it would be necessary to demarcate all groups and determine the centre of each. Only groups having centres within the quadrat would then be considered. This task however is impossible, but it is obvious that not all groups having centres within the quadrat will be entirely included in the quadrat, and other groups having centres outside will be partially included. It is obvious that these two types of groups will have a compensatory action.

If this argument fails to convince the more critical ecologist, it is possible to develop the negative binomial series from another set of considerations. If small areas of ground are considered, it is possible that the distribution within each of these small areas is at random.

This seems a reasonable assumption provided the areas are small enough. Areas near to the parent plant will have a high population so that the mean of the Poisson distribution will be high, and areas at some distance from the parent will have a low population resulting in a low parameter for the distribution. There is thus a series of random distributions scattered over the whole area with varying means. It is obvious that the mean of a Poisson series cannot be negative, and it is reasonable to assume that the asymmetrical Pearson Type III distribution should give a fair approximation to the distribution curve of the means. If these two types of distribution are combined, the resultant distribution of numbers of individuals per quadrat is again the negative binomial.

Anscombe (1950) has pointed out yet a third method by which the negative binomial can be developed. In this it was assumed that the progenitors were released with a uniform distribution in time and that the birth- and death-rates per individual were constants.

It is not here claimed that any one of these three methods is the correct one, but all appear to be possible and do not suffer from the objections raised against the other contagious distributions which have been utilised. The weakest point, as David and Moore (1953) suggest, is the use of the Pearson Type III distribution. They, however, produce no arguments against its use, and there appear to be none immediately obvious, for its use has led to no large discrepancies between theoretical and observed distributions in the majority of cases. On the other hand, of course, the arguments in its favour are by no means conclusive, but are sufficiently plausible.

3. Use of the Negative Binomial Distribution

3.1 The Material

In order to test the applicability of the negative binomial distribution to plant populations, data collected by Steiger (1930) in an extensive study of the botanical composition of what he termed high and low prairie were used. In each of the two localities 40 quadrats each of 1 square meter were examined, and the number of individuals of several species were counted. With grasses and sedges the basal area occupied by the different species in each quadrat was also measured. The data he collected have already been used by Clapham (1936) in an attempt to fit the Poisson series to the number of individuals per quadrat. From the two localities a total of 44 distributions were examined; 24 of these were for non-grassy species and the remaining 20 were for grasses and sedges. Of the former only three showed no great divergence from the Poisson series, and only one of the latter. This is an excellent example of the general lack of randomness in natural vegetation.

The negative binomial is defined by two parameters, the mean, m, and the exponent, k.

The expected frequency of zeros is given by

$$p_0 = \left\{1 + \frac{m}{k}\right\}^{-k}$$

and the probability of observing any positive count r is given by

$$p_r = p_0 \binom{k + r - 1}{r} \left\{\frac{m}{m + k}\right\}^r$$

the variance of the distribution is

$$m + \frac{m^2}{k}$$

The estimation of k by the method of maximum likelihood or minimum χ^2 is very tedious, but Anscombe (1949, 1950) has given several methods whereby k can be estimated. One of the methods suggested is to equate the calculated variance for the sample to the population variance. This is Method I in Anscombe's notation, and the estimate of k obtained is

$$k = \frac{m^2}{s^2 - m}$$

where s^2 is the sample variance.

Method II suggested by Anscombe is to equate the actual number of zeros obtained to the expected number. In other words k is chosen by successive approximation to satisfy the equation

$$n_0 = N\left\{1 + \frac{m}{k}\right\}^{-k}$$

where n_0 is the number of quadrats containing no individuals and N is the total number of quadrats. The remaining methods suggested by Anscombe are more difficult to apply and are not as generally useful as the two given here. These only have been used in the following calculations.

3.2 Non-grassy Species

The twenty-four distributions were examined, and the values of the exponents were found by one of the above methods. In his paper Anscombe gives curves showing the efficiency of the two methods for various values of k and m. Roughly speaking, Method I is used when k is greater than unity, and Method II when k is less than unity. The χ^2 test was applied to each of the distributions, and only *Antennaria campestris* on low prairie showed any large disagreement between the actual results and the expected numbers calculated on the basis of the negative binomial series. The disparity is caused by a number of quadrats with 16 to 19 plants in each. This rise would suggest that a distribution with more than one mode would be more suitable. The double Poisson series and Neyman's contagious series were therefore fitted to the distribution; but the theoretical expectations were in neither case close to the actual results observed. There is therefore no added advantage in these two distributions even in this case. In view of the general agreement of the actual distributions with the distributions calculated using the negative binomial series, and as the same species on high prairie showed no great divergence from the negative binomial, it does not seem very likely that this exception represents a true deviation from the negative binomial. It is more probably due to the difficulty of counting individuals of a species in which individual plants form colonies by vegetative means.

There is some theoretical evidence (Kendall, 1948) to show that the value of k, the exponent, depends on the intrinsic power of the species to reproduce itself, and is independent of the external condition of the environment. The value of k in the two communities should therefore, on theoretical grounds, be equal for each species.

This point has not been tested with actual data before, and it was felt that it would be instructive to determine values of the exponents for the combined distributions of several species in both communities. Of the 24 distributions there were 7 pairs of species in high and low prairie (excluding the pair of Antennaria campestris). A combined value of k was then determined for each of the 7 species by using the total number of zeros found in the two communities.

Table II.1 gives the value of the exponent for each distribution, together with the combined value for the two communities. Method II was used in all cases except for *Amorpha canescens* and *Psoralea floribunda*, both on high prairie. In these cases it was found from the graphs given by Anscombe that Method I should be more efficient.

Table II.1. *Values of the exponent of the negative binomial distribution for various non-grassy species in high and low prairie*

Species	A	Am	C	E	H	P	S
Low Prairie	1.18	0.32	0.88	1.78	0.77	0.16	0.44
High Prairie	2.30^{+}	0.30	0.77	0.69	0.40	1.62^{+}	0.24
Combined	1.43	0.30	0.81	1.00	0.47	0.52	0.31

A	Amorpha canescens	H	Helianthus scaberrimus
Am	Aster multiflorus	P	Psoralea floribunda
C	Cathartolinium sulcatum	S	Solidago glaberrima
E	Erigiron ramosus		

$^{+}$The values of k in these two cases were determined by Method I; the remainder were by Method II (see text).

The large-sample variance of k for Method I is

$$V(k) = \frac{2k(k + 1)}{Nx^2}$$

and for Method II it is

$$V(k) = \frac{(1 - X)^k - 1 - kX}{N\left[-\log_e(1 - X) - X\right]^2}$$

where N is the number of quadrats examined (in this case N = 40) and

$$X = \frac{m}{m + k}$$

Using these formulae, the variance of k for each species in the two different communities was found. By an application of the t-test, Psoralea floribunda showed a difference between the two values which was significant at the 5 per cent probability level; the remainder showed no great disagreement.

Table II.2 gives the actual and the expected numbers of quadrats in the various classes for the different species in the two areas. The expected numbers for Psoralea floribunda were calculated using the two different values of k for the two communities, but those for the remainder were calculated using the mean value of

Table II.2. The actual (column (i)) and expected (column (ii)) distribution for non-grassy species in high and low prairie

	Number of individuals	Number of quadrats in the different groups													
		A		Am		C		E		H		P*		S	
		(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
LOW PRAIRIE	0	6	5.0	20	20.4	15	15.4	12	14.7	7	11.2	33	33.0	12	15.4
	1	6	5.5	6	5.5	10	8.6	7	9.3	6	4.9	3	3.7	2	4.5
	2	3	5.1	2	3.2	3	5.4	10	5.9	2	3.4	4	1.5	4	2.8
	3	6	4.5	1	2.2	5	3.9	5	3.7	6	2.6	0	0.8	3	2.1
	4	6	3.8	2	1.6	1	2.3	4	2.4	1	2.1	0	0.4	2	1.6
	5	4	3.2	2	1.2	2	1.5	1	1.5	3	1.7	0	0.2	1	1.4
	6	1	2.6	1	1.0	2	1.0	1	0.9	0	1.5	0	0.2	3	1.2
	7	1	2.1	1	0.8	1	0.7	0	0.6	3	1.3	0	0.1	0	1.0
	8	2	1.7	2	0.6	0	0.5	0	0.4	2	1.1	0	0.1	3	0.9
	9	0	1.4	0	0.5	1	0.3	0	0.2	3	1.0	0	0.0	0	0.8
	10	2	1.1	1	0.4	0	0.2	0	0.2	0	0.9	0	0.0	1	0.7
	11	0	0.9	0	0.4	0	0.1	0	0.1	0	0.8	0	0.0	1	0.6
	12	0	0.7	0	0.3	0	0.1	0	0.1	1	0.7	0	0.0	2	0.6
	Over 12	3	2.4	2	1.9	0	0.4	0	0.0	6	6.8	0	0.0	6	6.4

*Individual values of the exponent, k, used for the two communities

Table II.2. <u>The actual (column (i)) and expected (column (ii)) distribution for</u>
(CONTINUED) <u>non-grassy species in high and low prairie</u>

	Number of individuals	Number of quadrats in the different groups													
		A		Am		C		E		H		F*		S	
		(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
HIGH PRAIRIE	0	0	2.1	24	24.0	18	17.9	15	12.6	20	18.9	6	8.2	21	18.8
	1	5	2.6	5	5.9	10	9.1	5	8.6	6	7.1	12	8.3	4	5.3
	2	3	2.8	4	3.1	6	5.2	7	5.9	5	4.2	7	6.8	4	3.2
	3	2	2.8	1	2.0	3	3.1	3	4.0	4	2.7	3	5.1	1	2.2
	4	1	2.7	1	1.3	1	1.8	4	2.8	0	1.9	4	3.7	4	1.7
	5	2	2.5	3	0.9	0	1.1	1	1.9	1	1.3	3	2.6	0	1.3
	6	4	2.4	0	0.7	1	0.7	2	1.3	0	1.0	1	1.8	1	1.1
	7	2	2.2	0	0.5	0	0.4	0	0.9	1	0.7	1	1.2	0	0.9
	8	0	2.0	0	0.4	0	0.2	0	0.6	1	0.5	2	0.8	1	0.7
	9	3	1.8	1	0.3	0	0.2	0	0.4	0	0.4	0	0.5	0	0.6
	10	0	1.7	1	0.2	0	0.1	1	0.3	0	0.3	0	0.3	1	0.5
	11	2	1.5	0	0.1	0	0.1	0	0.2	0	0.2	0	0.2	0	0.4
	12	3	1.4	0	0.1	0	0.0	0	0.1	0	0.2	1	0.2	1	0.4
	Over 12	13	11.5	0	0.5	1	0.1	0	0.4	2	0.6	0	0.3	2	2.9

*Individual values of the exponent, k, used for the two communities k found from the total number of zeros in both areas. The appropriate mean number of individuals for the different species was used in each case. The application of the χ^2 test for goodness of fit showed no large deviations from the expected numbers. These results tend to support the theory that the value of k does not depend on the community associations.

In her paper comparing Neyman's series with the Poisson series Archibald demonstrated that the distribution of several species in which there was a significant divergence from a random distribution could be fitted reasonably well by Neyman's contagious series. These species, however, also showed no great divergence from the negative binomial series, and, in addition, the distribution of Salicornia stricta, which did not follow Neyman's series, gave good agreement with the negative binomial series.

3.3 Grasses and Sedges

In view of the general applicability of the negative binomial series shown by the above examples it was thought that, in spite of the difficulty of segregation into separate individuals, the distributions given by Steiger should be tested on this basis. Table II.3 gives the values of the exponents found for six species on both high and low prairie, together with the mean values obtained for the composite distribution in the two areas. The distribution of Eragrostis pectinacea on low prairie gave a variance smaller than the mean, so that the value of k is indeterminate. Although the Poisson series fits the distribution quite well, the use of the mean value of the exponent, i.e 0.50, gives a distribution which also is not significantly different from the actual distribution obtained.

The large-sample variances of the various exponents were calculated using the formulae given above. The t-test showed that the differences between the two exponents for Koeleria cristata and Andropogon furcatus were significant. The remaining four species showed no significant differences between the two exponents, and

Table II.4 shows the actual and the expected number of plants in the various classes calculated using the mean value of k in these cases. The table also shows the actual and the expected numbers of plants for the other two species, but the appropriate values of k were used. The χ^2 test indicated no great differences between the actual and the expected numbers.

Table II.3. Values of the exponent of the negative binomial distribution for six species of grass in high and low prairie

Species	Af	As	El	Ep	K	St
Low Prairie	8.50^{+}	0.76	0.17	?	0.60	0.90
High Prairie	1.74^{+}	1.70^{+}	0.07	0.31	2.88^{+}	0.55
Combined	2.88^{+}	1.37^{+}	0.11	0.50	0.70	0.64

Af	Andropogon furcatus	Ep	Eragrostis pectinacea
As	Andropogon scoparius	K	Koeleria cristata
El	Elymus canadensis	St	Stipa spartea

$^{+}$The values for k in these cases were determined by Method I; the remainder were by Method II (see text).

4. Methods of Comparison

Having determined the general form of a distribution which appears to fit a large number of examples sufficiently well, it is possible to go one step further to the methods which can be used in a comparison of different localities. The distribution is most decidedly skew, so that an analysis of variance using plant counts will not give a valid estimate of the importance of the difference between two communities. Anscombe (1949) has shown, however, that after the use of a transformation it is possible to apply the analysis of variance technique. This depends on the assumption that the exponent, k, remains constant under the different conditions. The data examined here would suggest that this is not unlikely.

As an example of the method of analysis the data for Aster multiflorus are considered. In his paper Steiger gives no indication of the location of the 40 quadrats in each of the two localities and all have been grouped together in the above analyses. For an estimate of error in the analysis of variance, however, it is necessary to have replicate samples. For purposes of illustration these have been made up by grouping the quadrats into four series containing ten quadrats, in each locality. Every fourth quadrat was selected from Steiga's tables to give the four random samples in each locality. The distributions obtained were as follows:-

Low Prairie: Sample 1 0^5, 1, 2, 4, 5, 13
2 0^5, 1, 3, 4, 8, 10
3 0^6, 1^2, 7, 18
4 0^4, 1^2, 2, 3, 6, 8

High Prairie: Sample 1 0^6, 1, 2, 3, 5
2 0^6, 1^2, 2, 9
3 0^5, 1, 2, 5^2, 10
4 0^7, 1, 2, 4

The notation used is self-explanatory; Sample 3 on low prairie, for example, had six quadrats with no individuals, two with one individual, one with 7 and one with 18 individuals.

The totals of each sample are now used. The combined exponent, k, for the individual quadrats was found to be 0.30 (Table II.1); therefore, the exponent for the totals of 10 quadrats will be ten times this value, i.e. 3.0. Anscombe suggests two transformations but the simplest one for use is

$$y = \log_{10}(r + \tfrac{1}{2}k)$$

where r is the total number of individuals in the sample. For example in Sample 1 from low prairie the total is 25, hence

$$y = \log_{10}(25 + 1.5)$$
$$= 1.42$$

The eight values obtained by using this transformation are

Low Prairie:	1.42	1.44	1.45	1.40
High Prairie:	1.10	1.16	1.40	0.93

and the analysis of variance becomes

Variation	d.f.	s.s.	m.s.
Total	7	0.2718	
Localities	1	0.1568	
Error	6	0.1150	0.0192

The variance ratio for testing the difference between localities is then 8.17, and for significance at the 5% level of probability, the ratio should be greater than 5.99. This therefore indicates a significant difference between the two populations on high and low prairie. This method can of course be readily extended to the full analysis of a properly replicated field trial.

David and Moore (1953) have suggested various criteria for determining the difference between two populations, which they claim are independent of the actual form of the distribution of plant populations. In the first place, however, their methods suffer from the limitation imposed by having only two populations for comparison. It is therefore ungainly if used for comparing treatments in a replicated experiment.

The comparison they suggest for means is the test used for normal populations. With the undisputed skew distributions which are actually found, the validity of such a test is open to question. A further criterion suggested is

$$w = -\tfrac{1}{2}\log_e \left\{ \frac{s_1^2}{m_1} \times \frac{m_2}{s_2^2} \right\}$$

where m and s^2 indicate mean and variance. This is then the logarithm of the ratios of the two coefficients of dispersion (c.f. Blackman, 1942). When the populations follow the negative binomial distribution this reduces to

$$w = -\tfrac{1}{2}\log_e \frac{k + m_1}{k + m_2}$$

provided $k_1 = k_2 = k$

It is therefore a criterion for testing the difference between means.

A third criterion they suggest is based on the measure suggested by Moore (1953). They overcome to a certain extent the criticism of this method which was made in Section II B. 1, by stating that it should be used only when half or more than half of the total frequency is included in the first three classes. In a negative binomial distribution

$$p_1 = p_o k \left\{ \frac{m}{m + k} \right\}$$

and $$p_2 = p_o \frac{(k + 1)k}{2} \left\{ \frac{m}{m + k} \right\}^2$$

$$\therefore \phi = \frac{p_o p_2}{p_1^2}$$

$$= \frac{(k + 1)k}{2k^2}$$

$$= \tfrac{1}{2} \left\{ 1 + \frac{1}{k} \right\}$$

this seems to be a highly questionable procedure.

5. Size of Quadrat

A great deal has been written on the most suitable size of quadrat for use in this type of ecological study, for it has been shown that different sizes of quadrat can alter the values of the proposed coefficients for measuring the degree of association in a given community.

With a negative binomial distribution an increase in the size of quadrat will increase both m and k, so that the proposed new method of dealing with plant communities does not differ from the earlier suggestions in this respect. There is, however, a very large difference in the two concepts. Earlier writers have given the impression, and in some cases expressly stated, that the various coefficients are some absolute measure of the population under study. In this case the two parameters do of course define the population, and are therefore absolute values in that sense, but they obviously must vary according to the size of quadrat in use. This, however, is no disadvantage, and, provided the same size of quadrat is used, different populations can be readily compared.

It is difficult to decide what criterion should be used for defining a "minimum" or "optimum" quadrat area, but fortunately such a definition is not necessary. The simplest procedure is to determine the size of quadrat most suitable for the type of the vegetation to be studied, and most easily manageable from the point of view of the experimenter. This will to a certain extent be a matter for subjective choice by the individual, but it will usually be based on what the other workers have used, in order to facilitate a direct comparison. Having chosen the most suitable size, it is then merely a matter of determining the requisite number for a given degree of accuracy. This is a sound procedure and obviates the difficulty of defining and determining an "optimum" size.

6. Exceptions to the General Rule

The data collected by Steiger (1930) which have been used to show the general applicability of the negative binomial distribution, did in fact contain several examples which did not follow the general rule.

The six examples given in Section II.B.3.3 will first of all be considered. Four of the species had exponents which did not differ significantly in the two localities. These were *Eragrostis pectinacea*, which grows as distinct individuals, *Elymus canadensis* and *Stipa spartea* which are tufted varieties of grass making the identification of individuals comparatively simple, and *Andropogon scoparius*, for which the individuals although densely packed, can without great difficulty be separated. The remaining two species *A. furcatus* and *Koeleria cristata* gave different values of the exponent on the two localities. *A. furcatus* is similar to *A. scoparius* in that dense communities are found, and *Koeleria cristata* is more or less tufted in its growing habit, so that again individuals should not be too difficult to distinguish.

The first three species for which it is comparatively simple to distinguish individuals all gave similar exponents in the two localities, and one of the last three species, where individuals are somewhat more difficult to separate, also gave similar exponents in the two localities. It is possible that the failure of the remaining two species to agree with this general indication was due to the greater difficulty of separating individuals, which would result in a mis-count on some of the quadrats.

This suggestion is further strengthened by the fact that three other species, *Carex meadii*, *Bouteloua curtipendula*, and *Poa pratensis*, which were enumerated on both high and low prairie, showed significant differences between the actual distributions and the expected distributions calculated on the basis of a negative binomial series. All of these species exhibit a creeping type of growth, so that individuals are extremely difficult to distinguish. In such circumstances it is obvious that mis-counts will be made.

In other words, although the negative binomial appears to be suitable for the majority of cases, there are certain exceptions which do not follow the general rule. The reason for this, however, appears to be entirely due to practical difficulties and cannot, with justification, be raised as a criticism of the statistical method. With species of this type it is often impossible to determine whether connections occur without digging, and even then roots may be broken. Certain definitions of an "individual" have been made by various writers, but these are in general arbitrary and bear no real relation to the actual biological status. Faced with this serious difficulty of being unable to distinguish individuals reasonably easily, some other form of estimating the importance of the various species must be employed. We therefore turn to a consideration of the second method suggested in Section II.A - area covered.

II.C. Area Covered

1. Historical

Many workers consider that the use of area is the best method in experimentation for determining succession and trends, and for comparing different management and cultural treatments, and a great many methods have been evolved. The measurement may be either leaf-spread (crown cover) or basal area (ground cover). There are arguments in favour of both practices. Crown cover is useful in soil conservation studies, for it gives an estimate of the ground projected against the direct beating of the rain, and, in addition, is a useful estimate of the relative productivity of a given species. It is of no use for comparing the estimated productivity of two different species, for a tall upright species will cover less area than a short creeping species, although the actual productivity may be much greater. This is put forward as one of the disadvantages of the method, but provided this point is recognised and the data are not used for this comparison, it is not likely to lead to any serious error. The method used must depend on the information required. If, for example, an estimate of the amount of blight on potatoes after various treatments is required, one cannot use the yield as a measure. It will to a certain extent reflect the amount of disease present, but some measurement of the actual disease, such as number of plants infected, is required. Yield, however, is not therefore criticised, and considered useless.

Basal area is in general more favoured than crown cover for it is regarded as reflecting definite changes in the pasture, whereas crown cover is largely dependent on the immediately preceding climatic conditions, and management. Basal area is therefore more useful in determining trends over a period of time. Whether crown cover or basal area is used, the method of determining can be the same. Brown (1954) has given several methods for the determination of area, but unfortunately she makes conflicting statements side by side, and the experimental worker is given no indication of the better methods for use. As a great many pasture experiments are carried on for a comparatively long period, there is an appreciable possibility that the person recording the data changes over the period. It is important therefore that the method used should eliminate as far as possible the personal element in experimentation, and should consist of purely objective assessment. Both West (1937) and Brown (1954) give descriptions of the various methods which have been employed, but nearly all of these suffer from this serious disadvantage, and will not be considered further. In order to obtain an objective assessment of area some actual numerical data must be collected.

It has been claimed (Brown, 1954) that "charting is the detailed, accurate representation of the position and the area occupied by species in a sample plot". It is, however, difficult to substantiate such a claim. In theory this should be the case, but in practice there is still the danger of a personal error in the actual mapping. If coordinates are actually measured or a pantograph used, this difficulty may be largely overcome, but the method then becomes very tedious and time consuming, and its use extremely limited. Photography is another possible method of overcoming the difficulty provided the reproduction does not suffer from curvature at the edges, and it is generally quicker.

Having prepared the map by a purely objective method it is still necessary to measure the area; in other words, the map merely transfers the problem from the field to the laboratory, with the increased likelihood of error in the extra stage required. It is therefore open to question whether the charting of quadrats is a method of any real value. The argument against the use of charting quadrats is further strengthened by the fact that large variations exist between quadrats in close proximity, due to the inherent variability in natural communities. Precise knowledge of the herbage on a very small fraction of the area therefore gives very little indication of the herbage on the whole area, and the method appears completely unnecessary.

This attack on the charting of quadrat samples in pasture is not intended as an attack on charting itself. It has already been stated that the method used must depend on the information required. The quadrat, by implication, is a small sample area selected from the total. Any knowledge of the quadrat, even if this knowledge is precise in every detail, cannot be immediately applied to the whole area; several samples are required in order to obtain an estimate of the community as a whole. This therefore precludes the use of the methods which require a considerable time for the enumeration of one small sample area. Charting methods, however, are extremely useful when it is necessary to distinguish comparatively large differences, and details are unimportant.

It is necessary now to consider what numerical data is required in order to estimate area. The first and most obvious measurement is the actual area itself. This, however, is rather difficult in a natural community, and most of the methods suggested (c.f. Brown, 1954) approximate the actual irregularly shaped areas to either rectangles or circles. This may introduce a certain amount of personal error. A simpler method would be the use of the line transect, where only linear measurements are taken, and the percentage area covered is determined without the necessity of measurements of actual area. Bauer (1943) stated that the method is more accurate than the use of quadrats. On the other hand Edwards (quoted by Brown, 1954) found that the method gave no indication of the visually obvious change over a number of years, when examining the effect of fire treatment on grasslands in Kenya. It was thought that this was probably due to the fact that several different observers recorded the data, which would suggest that the method is not as free from personal error as would at first appear. Parker and Savage (1944) found in fact that there were statistically significant differences between different observers. The difference between lines, however, was so much higher that it is doubtful, from their results, whether such differences although statistically significant, would, in fact, lead to any serious error in practice. The main disadvantages of the method are the time required for enumerating a single line sample, and the difficulties encountered in more dense vegetation when it is difficult to determine the limits of plants. In this case the personal factor is liable to become important.

Having progressed from the area to the line it is natural to consider the possibility of going one stage further, to the point. Point quadrats for determining vegetation composition have been used by many workers during the past 20 years and a review of the literature has been made by Goodall (1952 (b)). In this paper he makes the following statements (p. 3):-

"In view of the inadequacy of fundamental knowledge concerning a method of some popularity, with much to commend it from both theoretical and practical view points, a detailed study of certain aspects of it were considered worth while. No comparisons with dry-matter analyses were made; it was considered that point-quadrat analysis should be judged on its own merits as an ecological technique, and that its value was independent of agreement with other ecological techniques which, *from the ecological point of view*, had no greater *prima facie* validity".

It was with this same idea in mind that the investigations reported here were carried out. Goodall criticised the method on several minor details which will be mentioned later, but concluded " ..... it remains one of the most trustworthy methods available to the ecologist, and one of the most nearly objective". The majority of workers who have tested the method appear to be in general agreement with this statement.

2. The Point Quadrat Method

The first detailed description of the method was given by Levy and Madden (1933). They used a frame in which ten pins were arranged to move freely in a vertical plane at distances of 2 inches apart. The apparatus is placed at a number of randomly selected points in the area, and the number of contacts made by the pins is recorded on each occasion. If crown cover is required the species touching the pin is recorded, and if basal area is required only direct hits on points or emergence from the ground are recorded.

In the first case, the percentage each species contributes to crown cover can be determined. As it is possible for several different species to touch the same pin, the sum of the percentages for each species plus bare ground may exceed 100. This, however, is immaterial when dealing with an individual species for purposes of comparison between two areas. When considering total crown cover it is necessary, of course to regard the contact of one pin by several different species as one hit only. This does not arise when considering basal area. With these data at one's disposal it is possible to determine other quantities, such as the percentage cover each species is contributing to the total cover, suggested by Levy, and use them for comparison purposes. Such measurements, however, are not generally necessary, and do not increase the amount of information already available, and in addition may be misleading. It is therefore, in general, better to deal only with the original measurements of crown cover and basal area. This should not be regarded as a rule never to be broken, for in some circumstances, valid arguments can be put forward for their use, but this will only be in comparatively few cases.

3. Distribution of Percentage Area

It has already been stated that in order to be able to deal effectively with a set of data it is necessary not only to know the mean but also the form of distribution.

First of all it might be as well to consider the distribution of percentage area when measured as such in sample quadrats. In his examination Steiga (1930) prepared maps of the 80 quadrats used, and it is therefore possible to determine the percentage basal area for each species. The distribution will vary between the limits of 0 and 100 per cent, or by a slight change of scale from 0 to 1, when fractional area is considered instead of percentage. It may be expected therefore that the distribution of the fractional area occupied will follow the Pearson Type I curve. This is defined by two parameters p and q, and the ordinate of the curve is given by the equation

$y = Ax^{p}(1 - x)^{q}$ where A is a constant

$$= \frac{1}{B(p + 1, q + 1)}$$

$$= \frac{\Gamma(p + q + 2)}{\Gamma(p + 1)\Gamma(q + 1)}$$

and x is the fractional area covered by the species ($x = 0$ when none of the species is present, and $x = 1$ when the whole area is covered by that species). The mean of this distribution is

$$\bar{x} = \frac{p + q}{p + q + 2}$$

and the variance is

$$\sigma^2 = \frac{(p + 1)(q + 1)}{(p + q + 2)^2 (p + q + 3)}$$

The mean value of x^2 is given by

$$\overline{x^2} = \frac{(p + 1)(q + 1)}{(p + q + 2)(p + q + 3)}$$

and this together with the formula for the mean was used to determine the values of p and q. Three species were tested in this manner: *Carex meadii*, a creeping sedge; *Poa pratensis*, a creeping grass; and *Andropogon furcatus*, which has already been shown in Section II.B.3.3 to follow a negative binomial series when considering the distribution of numbers of plants. Table II.5 gives the values of p and q calculated from the above expressions, and Fig. II.1 gives diagrams of the theoretical curves using these values, together with histograms of the actual numbers obtained.

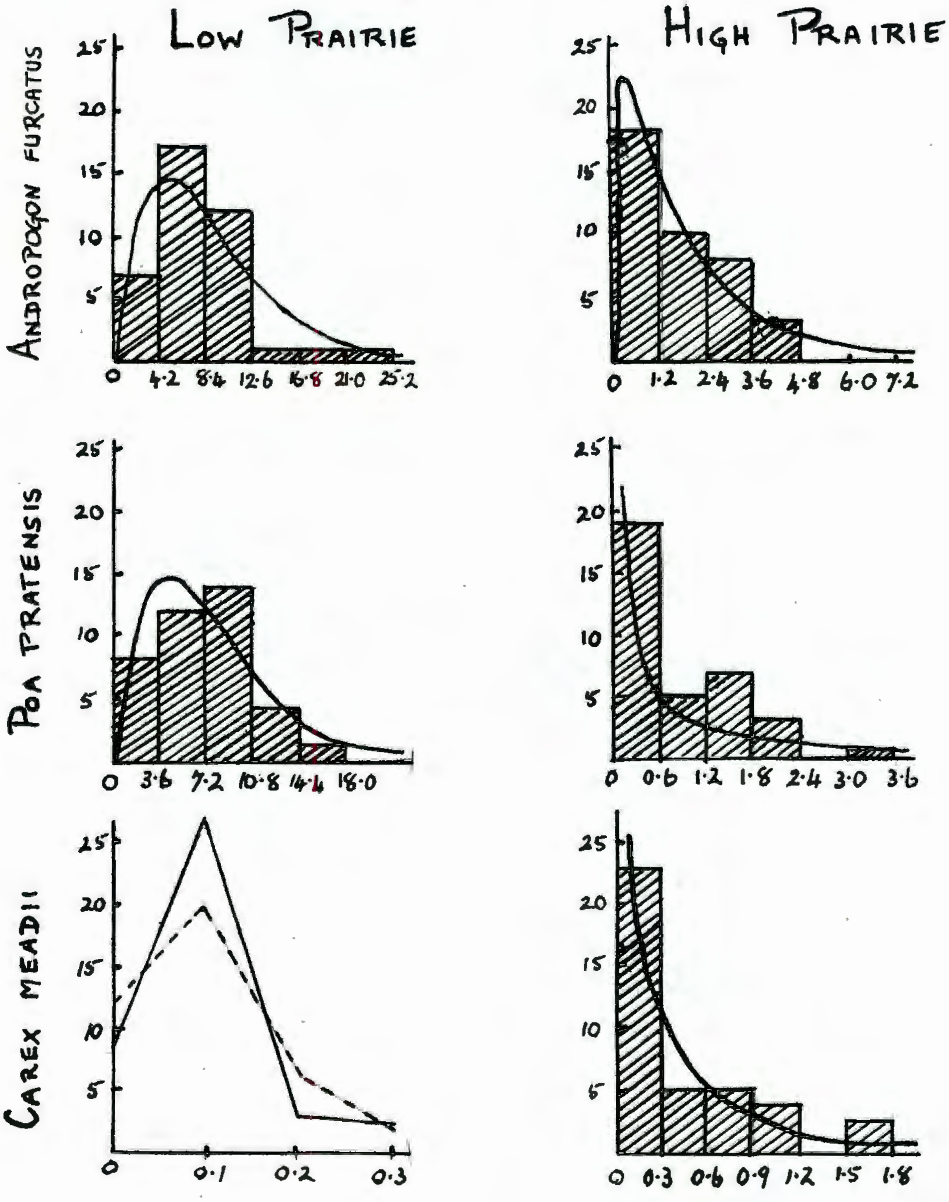
Table II.5. <u>Values of the constants p and q for the Pearson Type I curve, governing the distribution of the fractional area covered by two grasses and one sedge</u>

	Andropogon furcatus		Poa pratensis		Carex meadii	
	p	q	p	q	p	q
Low Prairie	1.1	21.7	1.3	28.3	0.7	1663
High Prairie	0.1	63.9	-0.8	9.6	-0.3	167

The values of p and q will of course depend upon the mean fractional area occupied. If this is large p will be large compared with q; if it is small q will be large compared with p. This can be clearly seen in Table II.5 and Fig. II.1. The values will also depend on the amount of aggregation; when this is high p and q will more nearly approach each other in value, resulting in a higher variance. When the aggregation is not so marked the values of p and q will be more widely apart. This can be seen by reference to <u>Andropogon furcatus</u>. On low prairie the values of p and q are not so widely different as those on high prairie, and reference to Table II.4 shows that the variation in plant number is much greater on low prairie,

Fig. II.1. Theoretical Curves (Pearson Type I), and Histograms of the actual distributions of the percentage area covered by two grasses and one sedge. (Percentage area on horizontal axis; number of quadrats on vertical axis.)

The figure for *Carex meadii* on Low Prairie shows the actual numbers of quadrats with 0, 0.1, 0.2, and 0.3 percentage cover (continuous line) and the expected numbers (broken line), assuming 0 to 0.05 be registered as zero; from 0.05 to 0.15 be registered as 0.1, etc.



giving 8 quadrats with more than 140 individuals.

The expected numbers of quadrats in the various groups were calculated from the formulae for an integral given by K.Pearson in Part II of his "Tables for Statisticians and Biometricians". Weddle's approximation to the integral over a given range is

$$\frac{3h}{10}(y_0 + 5y_1 + y_2 + 6y_3 + y_4 + 5y_5 + y_6)$$

where y_0, ... y_6 are the ordinates equally spaced over the range, at a distance h apart. This formula was used in all cases except for the initial range in the two cases where p was negative. In this case the method of continued fractions, also given by Pearson, was used. Application of the χ^2 test showed that there was no evidence of any large discrepancies between the expected numbers calculated by these methods and the actual numbers observed.

The areas given by Steiga for *C. meadii* on low prairie appear to have been rounded off to the nearest decimal place. As the species covered only a very small area in each quadrat, the data no longer resemble a continuous curve. It was better in this case therefore to plot the expected number of quadrats in which the area covered by the species was 0, 0.1, 0.2, ... square decimetres. It was assumed that when the area was between 0 and 0.05 a zero area would be recorded; between 0.05 and 0.15 then a 0.1 area would be recorded, etc. This will tend to overestimate the number of recorded zeros, and underestimate the number of plots recorded with an area of 0.1 square decimetre, but even so, the agreement is reasonably good and χ^2 is not significant at the 5 per cent probability level. The suggested Pearson Type I distribution therefore appears to be a reasonable approximation to the actual distribution of basal area.

The distribution of fractional area when the data are collected by means of the point-quadrat apparatus will of course follow a discontinuous type of distribution. If the series of points are distributed entirely at random on the area under investigation the distribution will follow the binomial series, irrespective of the actual distribution of basal area within a quadrat.

This can be seen if one considers the extreme case where one section of the area is a solid mat of vegetation and the remainder completely bare. If points are scattered entirely at random, the probability of landing on the occupied ground will be p, where p is the fraction of the area occupied. In any type of stratified sampling, however, such as distributing the points at random in fixed quadrats or areas the probability of contact will be zero or unity for those quadrats entirely in one of the sections, and some value between where quadrats extend over both sections. This will, of course, not lead to a binomial distribution for the whole area. With the apparatus generally used for point quadrat determinations the ten points are not independent, and under certain circumstances this may lead to large differences from the binomial distribution. Blackman (1935) suggested that the use of single points distributed at random may lead to more accurate results than sets of points, and this suggestion has been tested by Goodall (1952 (b)). He claimed that the use of single points was far superior to the use of frames because they gave a smaller variation.

The normal distribution has been fitted to basal area distributions by West (1937) and to cover distributions by Stewart and Keller (1936). Both found that when the quadrat was large enough, the actual distributions obtained did not differ significantly from normal. This is not surprising because the binomial distribution tends to normality in the limit. This was shown by de Moivre as early as 1733.

4. Distribution of Points in Sampling

Before dealing with the theoretical aspect, the practical disadvantages of randomising single points will first of all be considered. At one stage in his discussion Goodall (p. 22) states that for one of the species examined 423 single points will give a variance equivalent to 2,000 points in frames. This appears to be a considerable saving of effort. It must be realised, however, that 2,000 points on frames reduces to 200 positions of the frame. A large amount of the time taken in this method of sampling is utilised in walking from one sample point to another. This time is therefore more than doubled if single points are used. The time taken in actual recording is relatively small, and the recording of ten points in one position will not take ten times as long as the recording of one point. It is doubtful therefore whether there is in fact any actual saving.

The second disadvantage of the use of a single point is that there is the danger of personal bias in its actual location. Goodall recognised the seriousness of this criticism and in order to overcome it suggested the use of a frame carrying a number of pins, and the selection at random of one of them. This seems a reasonable solution of the difficulty, but if one has gone to the trouble of making a frame, why not use it to its full capacity? This, of course, presupposes the frame is not as bad as Goodell would have one believe. The frame or rather, a modification of it, has been tested under varying conditions, and, as the following results will show, has proved very effective.

5. The Experiments

5.1. Experiment I. Kitale

5.1.1. General Description

The first experiment was conducted at Kitale, which is in a large maize-growing area of Kenya, at about 6,000 ft. above sea-level, with an average rainfall of some 40 inches per annum. The plot was approximately 1 acre in area, and had not previously been grazed, so that the vegetation was tall. The samples were taken at the beginning of March, 1955, before the long rains, and the vegetation consisted mainly of dead stem and leaf, which was to be burnt for experimental purposes.

Two types of apparatus, both being slight modifications of the apparatus used by Levy and Madden (1933), were employed in this investigation to test their relative efficiency, in determining basal area. The first was simply a stout length of wood through which ten 6 in. nails had been driven 2 in. apart. This was similar to the apparatus used by West (1937). The second was basically the same as the original used in New Zealand, except that the pins were 4 in. apart instead of 2 in. These two types of apparatus will be denoted by "nails" and "pins" respectively in the following discussion.

From the practical point of view the nails were easier to carry about in the field, but with the difficult type of vegetation the shorter projections of the nails made it rather difficult to clear away the well-knit cover when recording the number of hits. In doing this it was also very difficult to prevent the board from moving slightly, so that the sample obtained was not the strictly random one that it should have been. In a pasture of young growth these practical difficulties are unlikely to arise.

The framework for the pins was, of course, somewhat larger and more cumbersome for carrying about the plot. Its size, however, was also an advantage, for when placed on the ground in the sampling position it did not move when the long dead grass was cleared and two hands were left free for the operation. The apparatus used in this investigation suffered from two minor defects; the pins showed some slight tendency to bend, and the ends were not pointed, so that sometimes when the pins dropped they would slide off the roots onto bare earth. This, however, did not happen very often, and where there was some doubt the pins were drawn up and allowed to fall again, and the point of first contact, not the resting position, was recorded. These are defects of the apparatus and not of the method.

It was unfortunate that the pasture was not at a younger stage of growth. As it was, in the clearing process, the grass would sometimes break off at the root which could then give rise to some doubt in the identification of the species with only the root remaining. It was also unfortunate that it was impossible for one person to complete the identification of all species during the sampling. With the difficult type of vegetation and the resulting errors in identification, it is to be expected that there will be differences between investigators in the estimation of basal area of certain species. Such differences between investigators should not occur when the total ground cover of all species is considered, and this has been used for the critical test of the merits of the two types of apparatus. In addition, one species Loudetia, has been used for further illustration, as it is one of the easier plants to identify.

The plot was roughly rectangular in shape, and fencing poles had just been erected around the perimeter. One investigator, A, took two transects parallel to the long sides of the rectangle for obtaining the samples using the pins. At every pace the apparatus was placed immediately in front of the investigator and at right angles to the path of the transect. The number of hits and the species involved were then noted. This was done 140 times, thus obtaining 1,400 points. The transects used for nails were at right angles to the long sides of the rectangle, and a further investigator, B, identified the species. In this case 600 points were obtained.

It was then thought that one pace between samples was not sufficient, for in obtaining samples covering the whole area a very large number would be involved. The number of paces between samples was therefore increased to 6. Investigator C was then asked to take over the sampling. The transects chosen were demarcated by the fencing-poles along the long sides of the rectangle. A zigzag pattern covering the whole area was obtained by using alternate posts on each side for pins and a similar pattern using the remaining posts was obtained for nails. In this way 1,090 points were obtained for pins and 1,470 for nails.

5.1.2. Total Basal Area

The results obtained by the three investigators are given in Table II.6. Investigator A took two transects with pins, and the results are given separately in the table. The data were transformed into degrees for statistical analysis, and the difference between the means of the two transects was tested. The transects of investigator C were grouped together in two classes by taking alternate transects of the zigzag pattern, and the data were again transformed into degrees for analysis. The means and the variances on the transformed scale are given in Table II.7. In all three cases (i.e. the two transects of A using pins and the two groups of transects of C using pins and nails) there was no difference between the two means. There was also no difference between the means obtained by the three investigators, and there was very good agreement between the two methods of obtaining samples.

The main differences to be observed lie in the amount of variation. Investigators A and B, in taking samples only one pace apart, covered a much smaller area than that covered by C. This smaller area should then be more

Table II.6 <u>Ground Cover</u>

	NAILS					PINS					
Investigator	B	O		Total	Expected	A		C		Total	Expected
Number of hits		I	II			I	II	I	II		
0	5	12	5	22	19.7	3	4	7	4	18	21.5
1	18	26	15	59	52.2	14	18	13	13	58	54.9
2	20	24	14	58	62.3	11	23	15	11	60	63.1
3	11	12	11	34	44.1	8	10	17	17	52	42.9
4	2	8	9	19	20.5	3	5	3	3	14	19.2
5	3	3	4	10	6.5	1		2	4	7	5.9
6	0	3	1	4	1.4						1.5
7	1			1	0.3						
Over 7											
Total	60	88	59	207	207	40	60	57	52	209	209
Mean No. of hits	2.02	1.99	2.34	2.10	2.10	1.92	1.90	2.03	2.27	2.03	2.03

Table II.7 Ground Cover

The means and variances of samples - data transformed into degrees

	NAILS				PINS				
	B	C		All Samples	A		C		All Samples
		I	II		I	II	I	II	
Mean	24.98	24.05	27.07	25.18	24.46	24.52	24.74	26.80	25.14
Variance	123.97	163.58	144.90	146.86	103.70	84.92	134.35	120.64	110.80

homogeneous in character than the whole plot, which should result in the variance being smaller. Table II. 7 shows that this was true for both nails and pins; the differences, however, were not statistically significant. It is nevertheless interesting to note that the differences which did exist were in the direction to be expected.

When binomial data are transformed into degrees the approximate variance on the new scale is 821/n, where n is the number of objects involved at each sampling (e.g. see Bartlett, 1947). In this case there are 10 points recorded at each sampling, so that n = 10 and the approximate variance of the new scale would be 82.1. All variances, except for transect II of investigator A using pins, are considerably higher than this, which should indicate a large degree of heterogeneity. This approximation to a constant variance, however, is misleading when n, the number of points used, is small, and p, the probability of obtaining a hit on a plant, is not near to 0.5. This is clearly shown in Table II.8, which gives the actual values for the variance on the transformed scale for different values of n and p. The table of variances is symmetrical about the value p = 0.5; in other words

Table II.8 The variance and the mean, on the transformed scale, of the binomial distribution for various values of p, the probability of obtaining a hit on a plant, and n, the number of points used

n \ p	.5	.4	.3	.2	.1	Approximate variance
	VARIANCE					
5	253.4	261.4	279.0	282.8	222.3	164.2
10	94.0	96.6	106.4	125.9	136.0	82.1
20	43.4	43.5	44.5	48.8	63.5	41.0
30	28.2	28.4	28.7	29.9	36.7	27.4
	MEAN					
5	45.0	37.9	30.4	22.0	12.0	
10	45.0	38.8	32.2	24.6	14.7	
20	45.0	39.1	32.8	25.9	16.6	
30	45.0	39.1	33.0	26.2	17.6	

the values for $p = 0.6$ are the same as those for $p = 0.4$. The second half of Table II.8 gives the means obtained on the transformed scale. It should be noted that these are not necessarily equal to the transform of the mean on the original scale.

The percentage basal area is approximately 20 per cent, so, from Table II.8, the variance on the transformed scale should be about 126. Reference to Table II.7 will show that this figure is generally in closer agreement with the results actually obtained. The variances obtained from the samples taken by investigator A are much lower than this expected value. The reason would probably lie in the close proximity of the samples; with the small area covered, the basal area would tend to be more homogeneous, with more readings around the mean, resulting in a smaller variance.

Further reference to Table II.7 will show that the variance obtained with nails is higher than that obtained with pins. Bartlett's test for the homogeneity of variances was used, and it was found that the difference was significant at the 5 per cent probability level. This result was not unexpected, and may be explained by the association existing between points in close proximity.

With nails, the points are only 2 in. apart, so that the ground covered at each sampling is small. With plants having a single stem this would not matter, but with grasses, and also other plants, having a crown covering a fairly large area of ground, the distribution will not be random. There will be a definite association so that when one hit is obtained there is a likelihood of obtaining more. In other words, with association, the extremeties in the distribution will have a greater frequency than values near the mean. This can be quite clearly seen in Table II.6. With pins the actual numbers obtained are alternately higher and lower than the expected numbers, but with nails the extremes of 0, 1, 5, 6 and 7 classes have actual frequencies greater than expectation, while the middle classes of 2, 3 and 4 have lower actual frequencies. With the greater number of readings at some distance from the mean, the variance will be increased. This means that a greater number of samples will be required if nails are used, to obtain the same degree of accuracy.

Pins show very good agreement with the expected binomial distribution. Where such agreement occurs the number of samples required to obtain a given accuracy may be determined from Table II.8. If it is desired that the variance of the mean should be v per cent of the mean, then the formula for N, the number of samples required, is

$$N = \left\{\frac{100\sigma}{vm}\right\}^2$$

where m is the mean on the transformed scale and σ^2 is the variance on the transformed scale. For example, if the basal area is 20 per cent then the mean and variance on the transformed scale will be approximately 24.6 and 125.9. With v = 5 per cent the number of samples required is approximately 83, i.e. 830 points.

5.1.3 Basal Area of Loudetia

The frequency of hits on Loudetia are given in Table II.9. Here the expected difference between investigators is shown very strongly with pins; but with nails somewhat similar results were obtained. The two sets of transects of investigator C using both pins and nails again showed no significant difference, and the two transects of investigator A using pins showed good agreement. The means and variances on the transformed scale are given in Table II.10. It was further found that investigators A and B obtained comparable results for nails and pins, and investigator C obtained comparable results by the two methods.

The results so far, apart from the difference between investigators A and C using pins, are the same as those obtained with basal area. The similarity is carried further in the comparison of the actual distribution and the variances obtained. There was no statistically significant difference between the variances obtained by the two investigators in either nails or pins, but again those obtained by A and B, where the samples were only one pace apart, were on the average slightly lower than those

Table II.9 Distribution of Louistia

	NAILS					PINS					
Investigator	B	C		Total	Expected	A		C		Total	Expected
Number of hits		I	II			I	II	I	II		
0	23	43	27	93	79.5	13	15	29	24	81	84.0
1	20	24	12	56	80.1	17	33	19	29	88	80.3
2	12	16	12	40	38.3	9	8	5	6	28	34.2
3	3	4	6	13	10.6	1	4	4	2	11	8.8
4	1	1	1	3	2.1				1	1	1.9
5	1		0	1	0.2						0.2
6			1	1							
Over 6											
Total	60	88	59	207		40	60	57	52	209	
Mean No. of hits	1.03	0.82	1.08	0.96		0.95	1.02	0.72	0.79	0.87	

Table II.10 Loudetia

Means and variances on the transformed scale

	NAILS			PINS			
	B	C		A		C	
		I	II	I	II	I	II
Mean	14.5	11.8	14.0	14.6	15.9	10.8	11.8
Variance	160.8	190.8	200.5	119.4	105.6	138.1	140.2

obtained by C. The difference between the two types of apparatus again proved significant at the 5 per cent level of probability.

The higher variance with nails is again due to the excess of readings at the extremes of the range - in this case mainly in the zero class. This can be clearly seen in Table II.9. The distributions obtained with pins by both investigators showed no large disagreement with the theoretical expectations. From Table II.8 it will be seen that the expected variance, when the mean is approximately 10 per cent, is 136.0, which is very close to the values obtained by C, and somewhat higher than those obtained by A. The number of samples required to obtain a given accuracy can again be calculated using the formula already given for basal area. With v again being 5 per cent, the number of samples required will be approximately

$$N = \left\{\frac{100}{5 \times 14.7}\right\}^2 \times 136.0$$

$$= 252$$

where 14.7 is the mean on the transformed scale when the mean basal area is 10 per cent. Or alternatively, if the number of samples considered necessary to obtain the basal area with a given accuracy is used, the accuracy of the determination in this case can be measured. From the above formula it will be found that

$$V = \frac{100\sigma}{m\sqrt{N}}$$

$$= \frac{100}{14.7}\sqrt{\frac{136.0}{83}}$$

$$= 8.7 \text{ per cent}$$

The small increase in accuracy by using 252 samples instead of 83 is probably not sufficient to warrant a large increase in the number of samples taken.

These formulae cannot be used without prior knowledge of the percentage cover. It is therefore necessary to take a few samples, possibly 20 would be sufficient, to determine the approximate basal area. Using Table II.8 it should then be possible to determine how many more samples are required, correcting at a later stage when more samples have been taken, if this is considered necessary.

5.1.4 General Conclusions

When considering the method used per se, reference to total basal area is as efficient as reference to basal area of a selected species. In order to restrict the work necessary, therefore, total basal area only will be taken into account.

The above results indicate that, although a 2 in. spacing between points may be sufficient for pastures in England and New Zealand, a wider spacing would be more efficient with the very heterogeneous herbage met on pastures in Kenya. The spacing between the points appears to be the main difference between the two types of apparatus, when dealing with basal area. If crown cover, however, is considered the shorter projections of the nails would be a great disadvantage. Goodall (1952 (b)) used a frame with the pins 2 in. apart on pasture in Australia, and obtained results similar to, but probably more extreme than, those given above. He concluded therefore that it is much better to use individual points scattered at random. It has already been pointed out that this will not necessarily decrease the work required, but, on the contrary will probably increase it.

A far simpler solution to the problem appears to be the increase of the distance between points. Corby (quoted by Brown (1954)) appears to have reached the same conclusion in Rhodesia. There is of course a practical limit to the distance which can be used, for the apparatus would become completely unwieldy. This could be overcome by reducing the number of points in the frame.

The number of points required has received little attention. It would appear that the selection of ten points originally was not based on any theoretical consideration, but it was merely a convenient number for converting to percentages. The use of a single point from theoretical reasons is possibly preferable, but on practical ground must be rejected. The use of any number of points smaller than ten will suffer from the disadvantage of increasing the time required, but will not be subject to the same amount of personal bias. In addition Table II.8 shows that with a smaller number of points, the use of the angular transformation will not reduce the variance to a constant independent of the mean, which may lead to discrepancies when comparing several communities. By combining several such frames together it is possible to build up samples of any number of points, which would make the use of the angular transformation more efficient. This, however, would reduce the number of samples in a community for a given amount of time, and provided the ten points in a frame do not lead to distributions widely different from expectation there is no advantage. Where the use of ten points in a frame does not lead to the binomial distribution and an increase in the spacing between points will make the apparatus too cumbersome, this second alternative could be tested.

An increased number of points must at once be dismissed for use in Kenya, because of the increased distance required between pins. In more uniform herbage the possibility could be investigated.

5.2 Experiment II. Muguga and Athi Plains

Muguga is situated at about 7,000 ft. and receives an average rainfall of some 30 - 35 in. Although it is not in a ranching area, this rainfall is typical of some of the large ranching areas of Kenya (c.f. map produced by Robinson and Glover published in a report on farming in the Kenya Highlands (Troup, 1953)). Two areas of approximately half an acre were examined at the beginning of February, 1954. The types of apparatus used were similar to the "nails" in the previous experiment. The object was to test the relative efficiency of three spacings between points - 2 in., 4 in., and 6in. The results are given in Table II.11.

The agreement between the actual and the expected distributions is good for the two wider spacings on both sites, and is also good for the 2 in. spacing on site 2. There is, however, a significant difference between the actual and the expected distributions using the 2 in. spacing on site 1. This is again due to the association existing between points lying so close together, and is due to the excess of sixes and zeros. It will be noticed that even on site 2 where agreement is good there is a

Table II.11. The actual (column (i)) and expected (column (ii)) distributions of the total basal area measured on two sites at Muguga by frames in which the points were 2 in., 4 in., and 6 in. apart

No. of hits	SITE 1						SITE 2					
	2 in.		4 in.		6 in.		2 in.		4 in.		6 in.	
	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	4	1.8	1	0.6	-	0.8	4	2.4	2	2.2	1	1.3
1	6	8.0	3	3.8	3	4.4	7	9.6	9	9.1	5	6.3
2	20	15.8	8	10.2	11	11.1	22	17.3	21	16.9	14	13.9
3	14	18.4	17	16.3	22	16.9	15	18.6	15	18.6	18	18.3
4	12	14.1	20	17.1	14	16.8	9	13.1	10	13.5	21	15.8
5	4	7.4	13	12.2	13	11.4	10	6.3	10	6.7	7	9.3
6	9	2.7	6	6.1	4	5.4	2	2.1	2	2.3	3	3.8
7	-	0.7	1	2.1	2	1.8	1	0.5	1	0.6	1	1.1
8	-	0.1	-	0.5	-	0.4	-	0.1	-	0.1	-	0.2
9	-	-	-	0.1	-	-	-	-	-	-	-	-
10	-	-	-	-	-	-	-	-	-	-	-	-
Total	69	69.0	69	69.0	69	69.0	70	70.0	70	70.0	70	70.0
Mean per cent cover	30.4		37.4		36.2		28.7		29.3		33.0	
Variance on transformed scale	151.3		87.9		73.3		137.2		114.1		85.8	

tendency for an excess of zeros with this narrower spacing.

Even though the expected distribution is not obtained with the narrower spacing, there should be agreement between the mean percentages of basal area. This was obtained on site 2, but with the narrower spacing on site 1, the mean basal area was significantly lower than that obtained by using either the 4 in. or 6 in. spacing. This is possibly because the number of samples was insufficient to obtain a reliable estimate of the mean. The rest of the results, however, indicate that the method gives objective figures, which can be used for comparison purposes, without discrepancies arising from differences from the expected distributions.

It will also be noticed that the narrower spacing on both sites has the larger variance on the transformed scale. On site 1 the variation obtained by using the 2 in. spacing is significantly higher than that obtained by using either of the other spacings. On site 2 the difference between the 2 in. and 6 in. spacings is not quite significant at the 5 per cent level of probability; using Bartlett's test the value obtained was 3.76, and

3.84 is required for significance.

These results give further confirmation that the 2 in. spacing is insufficient, but there is little advantage in increasing the spacing beyond 4 in. on this herbage. It had been hoped that the basal area would be smaller than that obtained at Kitale, in order to test more diverse conditions. A preliminary examination by eye had suggested that this might be the case, but the results proved otherwise.

It was therefore decided to examine herbage on the Athi plains, where the rainfall is much less reliable than that in Kitale or Muguga, and where it may therefore be expected that the basal area of the herbage will be smaller. The plots were examined in February, 1954 before the rains, and the herbage consisted in the main of small isolated tufts. On each site two samples were taken using each of the three frames, in order to test the reproducibility of the results.

Table II.12 gives the results obtained. It is obvious from an examination of this table that all distributions agreed with expectation, and that there

Table II.12. The actual (col (i)) and expected (col (ii)) distributions of basal area obtained by using 2 in., 4 in., and 6 in. spacing on two sites on the Athi Plains

No. of hits	SITE 1												Mean
	2 in.				4 in.				6 in.				
	Sample 1		Sample 2		Sample 1		Sample 2		Sample 1		Sample 2		
	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(ii)
0	10	14.8	21	15.3	16	14.0	17	19.0	11	13.2	17	15.3	15.2
1	33	27.2	21	27.5	24	26.7	30	29.4	25	26.0	29	27.5	27.4
2	23	22.5	22	22.3	21	22.8	24	20.4	31	23.2	17	22.3	22.3
3	11	11.0	10	10.7	15	11.6	7	8.4	8	12.2	11	10.7	10.8
4	3	3.5	4	3.4	3	3.9	2	2.3	4	4.2	4	3.4	3.4
5	-	0.8	1	0.7	1	0.9	-	0.4	1	1.0	2	0.7	0.7
6	-	0.1	1	0.1	-	0.1	-	0.1	-	0.2	-	0.1	0.1
7	-	-	-	-	-	-	-	-	-	-	-	-	-
8	-	-	-	-	-	-	-	-	-	-	-	-	-
9	-	-	-	-	-	-	-	-	-	-	-	-	-
10	-	-	-	-	-	-	-	-	-	-	-	-	-
Total	80	79.9	80	80.0	80	80.0	80	80.0	80	80.0	80	80.0	79.9
Mean per cent basal area	15.5		15.2		16.0		13.4		16.5		15.2		15.3
Variance on transformed scale	101.0		179.5		147.8		123.5		114.2		155.2		
Mean variance	140.2				135.6				134.7				143.5

Table II.12. (CONTINUED) The actual (col (i)) and expected (col (ii)) distributions of basal area obtained by using 2 in., 4 in., and 6 in. spacing on two sites on the Athi Plains

No. of hits	SITE 2												Mean
	2 in.				4 in.				6 in.				
	Sample 1		Sample 2		Sample 1		Sample 2		Sample 1		Sample 2		
	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(ii)
0	13	13.8	19	18.0	14	13.4	14	15.5	13	12.2	14	12.4	14.1
1	30	26.5	30	28.9	24	26.2	29	27.7	26	25.3	23	25.4	26.7
2	17	22.9	16	21.0	24	23.1	24	22.2	22	23.5	21	23.4	22.8
3	15	11.8	12	9.0	13	12.0	9	10.5	13	12.9	18	12.8	11.5
4	5	3.9	2	2.5	5	4.1	3	3.3	3	4.7	3	4.6	3.8
5	-	0.9	1	0.5	-	1.0	1	0.7	2	1.2	1	1.1	0.9
6	-	0.1	-	0.1	-	0.2	-	0.1	1	0.2	-	0.2	0.1
7	-	-	-	-	-	-	-	-	-	-	-	-	-
8	-	-	-	-	-	-	-	-	-	-	-	-	-
9	-	-	-	-	-	-	-	-	-	-	-	-	-
10	-	-	-	-	-	-	-	-	-	-	-	-	-
Total	80	79.9	80	80.0	80	80.0	80	80.0	80	80.0	80	79.9	79.9
Mean per cent basal area	16.1		13.9		16.4		15.1		17.1		17.0		15.9
Variance on transformed scale	131.1		148.1		135.1		126.6		145.6		141.6		
Mean variance	139.6				130.8				143.6				138.0

were no differences between the three types of apparatus, in either variation or mean basal area.

The variances obtained on the Athi Plains are very close to the variance to be expected from Table II.8. This is not surprising because of the good agreement between the actual and expected distributions in all cases. In the Muguga experiment the individual variances differ somewhat from expectation, but the mean variance for site 1 is 104.2 and that for site 2 is 112.4, which are in close agreement with the expected variance.

5.3 Experiment III, Makaveti

5.3.1 Ground Cover

A further experiment was carried out at Makaveti. The areas chosen were four half-acre plots of an experiment designed to test the feasibility of establishing grass on eroded soil in the Machakos District of Kenya. The experiment has been described by Pereira and Beckley (1952). The plots chosen for examination were planted with *Panicum coloratum*. This species in general forms large clumps, leaving considerable patches of bar ground. During the several years in which the experiment has been operating, there has been virtually no incursion of other species. Only in the fourth and fifth years has there been a very limited invasion of couch grass (*Digitaria abyssinica*), but this can be disregarded from the practical point of view.

The type of herbage tested in this trial of the point quadrat method represents the extreme, and under natural conditions it is unlikely that such an extreme would occur. If the method gives useful results under these definitely unusual conditions, it is reasonable to assume that the method can be generally used without leading to any serious errors.

Each plot was examined by two recorders, A and B, using all three types of apparatus. With each type of apparatus each recorder covered a zigzag path over the plot, dropping the frame at distances of 5 or 6 paces immediately in front, similar to the methods used on other sites. The two recorders took entirely independent samples, and did not each record the frame when in position, in the manner used by Goodall (1952 (b)). By this method two entirely independent estimates would be obtained, and in addition to testing the difference between observers, would also test the reproducibility of results. It is not necessary to separate these two problems, because in practice they are inter-dependent.

The distributions obtained for basal area are given in Tables II.13, II.14 and II.15. The first of these shows the results using the 2 in. spacing between points. Under less extreme conditions it has already been found that errors arise due to association between neighbouring points, and this is clearly demonstrated in the table. In all cases there is an excess of frames securing no hits and of frames securing a large number of hits. The

Table II.13. The actual (col.(i)) and expected (col.(ii)) distributions of basal area of *Panicum coloratum* on 4 plots at Makaveti, using 2 in. spacing between points, obtained by two recorders

Plot No.	13				14				47				48			
Observer	A		B		A		B		A		B		A		B	
No. of hits	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	6	0.4	6	1.7	7	1.1	7	1.7	17	2.2	10	1.2	20	4.7	12	1.7
1	1	2.4	11	6.8	7	5.2	13	6.8	6	8.1	8	5.5	5	12.6	4	6.8
2	8	6.8	8	12.3	4	11.0	4	12.3	6	13.3	9	11.1	9	15.0	11	12.3
3	8	11.3	8	13.3	9	13.8	9	13.3	4	13.0	4	13.3	4	10.7	6	13.3
4	9	12.5	7	9.4	12	11.3	4	9.4	5	8.3	5	10.5	4	5.0	5	9.4
5	5	9.4	4	4.6	7	6.3	3	4.6	2	3.7	2	5.6	1	1.6	4	4.6
6	5	4.9	2	1.5	2	2.5	6	1.5	4	1.1	5	2.1	3	0.4	3	1.5
7	5	1.8	1	0.4	3	0.7	3	0.4	1	0.2	1	0.5	1	-	1	0.4
8	2	0.4	2	-	1	0.1	1	-	3	-	5	0.1	3	-	1	-
9	-	0.1	-	-	-	-	-	-	1	-	-	-	-	-	3	-
10	1	-	1	-	-	-	-	-	1	-	1	-	-	-	-	-
Total	50	50.0	50	50.0	52	52.0	50	50.0	50	49.9	50	49.9	50	50.0	50	50.0
Mean per cent basal area	38.6		28.8		31.9		28.8		26.8		31.0		21.0		28.8	
Variance on transformed scale	347.2		325.7		273.2		307.2		559.6		472.3		427.9		430.0	

distributions tend to have two peaks, one in the zero class, and one in the class corresponding with the peak of the expected binomial distribution. The effect of association is also shown in the high variances obtained after transforming to degrees. Higher variances were obtained on plots 47 and 48, and it can be seen from the tables that this corresponds with a greater excess of zeros and larger numbers of hits. In all cases the χ^2 test indicated a significant difference between the expected and the actual distributions.

A t-test was used to test the difference between the mean percentages of basal cover obtained by the two recorders. In no case was the difference significant. Bartlett's test for the differences between variances was also used to compare the results obtained by the two recorders. The differences were not significant. The results therefore indicate that the method does not greatly suffer from the effects of personal bias.

Table II.14 shows the results obtained using a 4 in. spacing between points in the frame. Possibly the most obvious difference between this and the proceeding table is the general reduction in the variances. They are

Table II.14. The actual (col.(i)) and expected (col. (ii)) distributions of basal area of Panicum coloratum on 4 plots at Makaveti, using 4 in. spacing between points, obtained by two recorders

Plot No.	13				14				47				48			
Observer	A		B		A		B		A		B		A		B	
No. of hits	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	1	0.6	2	0.4	5	0.9	6	1.9	11	3.2	6	1.3	13	4.6	9	3.2
1	6	3.5	3	2.6	4	4.6	7	7.3	9	10.2	5	5.6	9	12.4	8	10.2
2	5	8.8	8	7.1	9	10.2	8	12.7	5	14.4	8	11.2	7	15.0	9	14.4
3	15	12.9	6	11.6	7	13.4	8	13.2	10	12.1	9	13.3	11	10.8	11	12.1
4	12	12.5	11	12.4	11	11.4	15	9.0	9	6.7	11	10.4	5	5.1	7	6.7
5	7	8.3	11	9.2	11	6.7	4	4.2	2	2.5	5	5.5	3	1.6	4	2.5
6	2	3.8	9	4.7	3	2.8	1	1.4	3	0.7	5	2.1	1	0.4	1	0.7
7	3	1.2	-	1.6	-	0.8	1	0.3	1	0.1	1	0.9	-	0.1	1	0.1
8	1	0.3	-	0.4	1	0.1	-	-	-	-	-	0.1	-	-	-	-
9	-	-	-	-	-	-	-	-	-	-	-	-	1	-	-	-
10	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total	52	51.9	50	50.0	51	50.9	50	50.0	50	49.9	50	50.0	50	50.0	50	49.9
Mean per cent basal area	35.6		38.0		32.9		28.0		24.0		30.8		21.2		24.0	
Variance on transformed scale	135.1		146.9		211.0		200.6		276.6		226.4		269.7		231.5	

still higher than the expected variances, indicating that the effects of association are still in evidence, but the reduction indicates that these effects are not so marked. Again plots 47 and 48 give higher variances than plots 13 and 14. In plot 13 the expected and actual distributions did not differ significantly when the χ^2 test was applied. In plot 14, adjacent classes were grouped in order to obtain an expected number greater than 5, so that the value of χ^2 should not be inflated. When this is done the differences are not quite significant. In this case, however, it would appear that this may not be a fair test in view of the obvious excess of zeros in the distributions of both recorders. In plots 47 and 48, when adjacent classes are grouped for the χ^2 test, the distributions obtained by B are not significantly different from expectation. There is, however, again the obvious excess of extreme classes, and when these are grouped χ^2 becomes significant. The distributions obtained by A were significantly different from expectation in both plots.

Means and variances were again tested for significance between recorders on the individual plots. There was no evidence of a real difference.

Table II.15. The actual (col. (i)) and expected (col. (ii)) distributions of basal area of *Panicum coloratum* on 4 plots at Makaveti, using 6 in. spacing between points, obtained by two recorders

Plot No.	13				14				47				48			
Observer	A		B		A		B		A		B		A		B	
No. of hits	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	2	0.8	1	0.4	4	1.9	2	0.9	9	2.9	4	2.0	9	3.7	6	3.4
1	3	4.1	1	2.6	6	7.3	3	4.3	6	9.5	7	7.7	8	10.9	12	10.5
2	10	9.5	9	7.2	10	12.7	7	9.8	8	14.2	13	13.0	12	14.7	12	14.6
3	12	12.9	12	11.7	16	13.2	17	13.2	15	12.4	8	13.1	9	11.7	8	12.0
4	10	11.5	11	12.4	7	9.0	13	11.7	7	7.2	13	8.7	8	6.1	6	6.5
5	9	7.1	8	9.0	4	4.2	5	7.0	2	2.8	2	3.9	2	2.2	2	2.4
6	3	3.0	6	4.6	3	1.4	2	3.0	2	0.8	3	1.2	1	0.6	4	0.6
7	1	0.9	2	1.6	-	0.3	2	0.8	1	0.2	-	0.3	-	0.1	-	0.1
8	-	0.2	-	0.4	-	-	-	0.2	-	-	-	-	1	-	-	-
9	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
10	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Total	50	50.0	50	49.9	50	50.0	51	50.9	50	50.0	50	49.9	50	50.0	50	50.1
Mean per cent basal area	33.8		37.8		28.0		33.5		24.8		27.4		23.0		23.6	
Variance on transformed scale	133.2		109.2		152.5		119.8		230.5		156.5		227.4		191.3	

These results indicate that the 4 in. spacing is superior to the 2 in. spacing, because lower variances were obtained and because the differences between expected and actual distributions were not as marked as with the 2 in. spacing. It would appear, however, that it is still possible for further improvement.

The last table (Table II.15) gives the results obtained by using a 6 in. spacing. There is once again a general fall in the variances obtained, indicating a closer approximation to the binomial distribution. It was in fact found that when the χ^2 test was applied, none of the distributions was significantly different from expectation. In spite of the rather obvious excess of zeros on plots 47 and 48, the remaining classes were all close enough to expectation to reduce the total χ^2 below significance. It would appear, however, that an even wider spacing may be advisable for these two plots in order to be completely certain that a binomial distribution is obtained. It was pointed out initially, however, that these four plots represent extremes of distribution which are unlikely to be found in general application of the point quadrat. A wider spacing in general does not therefore appear necessary.

In all cases plots 47 and 48 had higher variances than plots 13 and 14, and in general showed larger discrepancies between expected and actual values. It was thought that this may possibly be due to the size of clumps - larger clumps indicating more association, and therefore giving wider differences from expectation. In a transect running along the middle of each plot, ten clumps were selected at random and their diameters at the base measured. The means obtained were 17.7, 17.9, 15.4 and 15.0 respectively, for plots 13, 14, 47 and 48. The differences were not statistically significant, but the indication is that it is <u>not</u> the larger clumps which cause the greater discrepancies.

The two pairs of plots were especially chosen because there were previous indications that one pair (13 and 14) had a comparatively high cover, and the other pair (47 and 48) had a comparatively low cover. This is borne out by the actual means from the six sets of results from each plot, which give basal areas of 35.4, 30.5, 27.5, and 23.6 per cent respectively. It appears therefore that the greater the cover the more efficient the method, and that the spacing to use between points depends not on the diameter of clumps but on the distance between clumps.

Summarising the results from all trials it is apparent that the 2 in. spacing is definitely too small for use on herbage such as that found in East Africa, but that a 4 in. spacing will in general be sufficient. For more variable herbage a 6 in. spacing is recommended.

Table II.16 shows the mean percentages obtained by the three types of apparatus, and by the two recorders. There is obviously no bias from the three methods in determining percentage basal area. In other words, a valid estimate is obtained by all methods. There is also no difference between the two observers, indicating the absence of personal bias in the results. This does not agree with the results obtained by Goodall (1952 (b)) and Corty (quoted by Brown (1954)). There is, however, a possible explanation for this.

In his first measurement of plot 13, which was obtained using a 6 in. spacing, observer A obtained a mean basal area of only 27.6 per cent, which is significantly lower than the result obtained by observer B. This would possibly indicate

Table II.16. The mean percentages of basal area obtained by two observers on 4 plots at [illegible]

Plot No.	13			14			47			48		
Observer	A	B	Mean	A	B	Mean	A	B	Mean	A	B	Mean
Spacing												
2 in.	38.6	28.8	33.7	31.9	28.8	30.3	26.8	31.0	28.9	21.0	28.8	24.9
4 in.	35.6	38.0	36.8	32.9	28.0	30.4	24.0	30.8	27.4	21.2	24.0	22.6
6 in.	33.8	37.8	35.8	28.0	33.5	30.7	24.8	27.4	26.1	23.0	23.6	23.3
Mean	36.0	34.9	35.4	30.9	30.1	30.5	25.2	29.7	27.5	21.7	25.5	23.6

a personal bias, but it was discovered that the two observers were measuring different things; it is not surprising therefore that different results were obtained. In some cases with the larger clumps, the centre was covered by a very thin layer of earth over a dense mat of roots. Observer A was recording this as bare earth, whereas Observer B was recording it as a hit. When this difference in recording was rectified, the results obtained by the two recorders were comparable. This stresses the importance of defining exactly what is to be recorded, and it is possible that insufficient definition may have caused the differences found by Goodall and Corby.

53.2 Crown Cover

When each frame was placed in position in addition to the basal area the crown cover was also recorded. Any direct hit on the root (i.e. basal area) was included together with any nails which were in contact with a leaf. The results of this investigation are given in Tables II.17, II.18, and II.19. It is obvious that the main general conclusion reached when discussing basal area, also applies to crown cover; the wider the space between points in a frame the smaller the variance and therefore the more efficient the apparatus.

With the 2 in. spacing all distributions are significantly different from expectation except that obtained by observer B on Plot 14. With 4 in. spacing, plots 47 and 48 gave distributions showing disagreement with the binomial distribution but those obtained on plots 13 and 14 showed reasonable agreement. When the spacing is increased to 6 in. in spite of the general decrease in variance, the expected improvement in the agreement between the actual and expected distributions is not attained. There were discrepancies in the distributions obtained by observer A on plots 47 and 48 and by observer B on plot 13, and when

Table II.17. The actual (col. (i)) and expected (col. (ii)) distributions of Crown cover of *Panicum coloratum* on 4 plots at Makaveti, using 2 in. spacing between points, obtained by two recorders

Plot No.	13				14				47				48			
Recorders	A		B		A		B		A		B		A		B	
No. of hits	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	-	-	-	-	-	-	-	-	3	-	1	-	3	-	3	-
1	-	-	-	-	-	-	1	-	1	-	1	-	-	-	2	-
2	-	-	-	-	4	0.1	-	-	-	-	1	-	3	0.1	1	0.2
3	1	-	1	0.2	3	0.6	-	0.2	1	0.3	1	0.2	3	0.6	-	1.1
4	-	-	3	0.7	1	2.3	3	1.1	2	1.2	-	1.0	1	2.3	2	3.6
5	2	0.3	6	2.5	5	6.0	2	3.7	5	3.9	4	3.4	3	5.9	9	7.9
6	3	1.6	8	6.7	5	11.0	9	8.3	8	8.7	10	8.0	7	10.7	8	12.0
7	5	5.7	5	12.1	9	13.8	13	13.0	3	13.1	8	12.9	7	13.3	4	12.5
8	7	13.2	4	14.4	14	11.4	6	13.3	4	13.0	5	13.6	3	10.8	6	8.6
9	12	18.0	10	10.2	3	5.6	10	8.1	5	7.7	3	8.5	7	5.2	8	3.5
10	20	11.1	13	3.2	8	1.2	6	2 2	18	2.0	16	2.4	13	1.1	7	0.6
Total	50	49.9	50	49.9	52	52.0	50	49.9	50	49.9	50	50.0	50	50.0	50	50.0
Mean per cent crown cover	86.0		76.0		68.8		73.2		72.6		73.8		68.4		64.6	
Variance on transformed scale	252.8		325.1		321.2		227.0		659.8		471.7		627.1		516.6	

Table II.18. The actual (col. (i)) and expected (col. (ii)) distribution of crown cover of Panicum coloratum on 4 plots at Makaveti, using 4 in. spacing between points, obtained by two recorders

Plot No.	13				14				47				48			
Recorders	A		B		A		B		A		B		A		B	
No. of hits	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	-	-	-	-	-	-	-	-	-	-	-	-	2	-	2	-
1	-	-	-	-	-	-	-	-	2	-	-	-	1	-	1	0.1
2	-	-	-	-	1	0.1	1	0.2	1	-	3	-	1	0.1	6	0.6
3	-	-	-	-	2	0.5	-	0.4	2	0.2	4	0.3	-	0.8	4	2.2
4	-	0.1	-	-	2	2.0	3	1.8	1	1.1	2	1.4	5	2.7	3	5.7
5	1	0.4	1	0.3	3	5.6	3	5.0	5	3.6	3	4.4	7	6.7	2	10.1
6	3	2.1	2	1.4	9	10.5	9	9.9	6	8.2	6	9.2	6	11.3	4	12.5
7	7	6.8	8	5.3	13	13.6	15	13.5	5	13.0	2	13.3	4	13.1	9	10.7
8	13	14.4	6	12.8	13	11.6	10	11.8	8	13.4	13	12.5	10	10.0	8	5.9
9	14	18.1	18	18.4	6	5.8	7	6.2	7	8.2	7	7.0	7	4.5	9	2.0
10	14	10.2	15	11.9	2	1.3	2	1.4	13	2.3	10	1.8	7	0.9	2	0.3
Total	52	52.1	50	50.1	51	51.0	50	49.9	50	50.0	50	49.9	50	50.1	50	50.1
Mean per cent crown cover	85.0		86.6		69.4		70.2		73.4		71.6		67.0		59.8	
Variance on transformed scale	178.8		179.1		148.8		136.2		415.2		373.1		433.9		407.8	

Table II.19. The actual (col. (i)) and expected (col. (ii)) distributions of green cover of *Panicum coloratum* on 4 plots at Makaveti, using 6 in. spacing between points, obtained by two recorders

Plot No.	13				14				47				48			
Recorders	A		B		A		B		A		B		A		B	
No. of hits	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
0	-	-	-	-	-	-	-	-	1	-	-	-	1	-	-	-
1	-	-	-	-	-	-	-	-	-	-	1	-	1	-	1	0.1
2	-	-	-	-	-	0.2	-	-	3	0.1	-	0.1	2	0.1	2	0.5
3	-	-	-	-	2	1.1	-	0.2	-	0.4	2	0.6	4	0.7	2	2.0
4	-	0.1	-	0.2	4	3.4	3	1.1	6	1.8	3	2.2	2	2.7	6	5.3
5	2	0.7	4	1.0	8	7.7	3	3.6	5	5.0	7	5.8	2	6.6	10	9.8
6	4	3.0	2	3.6	11	11.9	6	8.3	3	9.9	7	10.6	7	11.2	10	12.5
7	9	8.2	10	9.0	11	12.6	12	13.2	9	13.3	8	13.3	8	13.1	5	11.0
8	11	14.7	15	15.0	6	8.8	17	13.7	9	11.8	12	10.9	10	10.1	7	6.4
9	11	15.7	8	14.8	8	3.6	8	8.5	7	6.2	5	5.3	7	4.6	3	2.2
10	13	7.6	13	6.5	-	0.7	2	2.4	9	2.4	5	1.2	6	0.9	4	0.3
Total	50	50.0	50	50.1	50	50.0	51	51.0	50	49.9	50	50.0	50	50.0	50	50.1
Mean per cent green cover	82.8		81.6		65.0		73.5		70.2		68.6		67.2		60.6	
Variance on transformed scale	203.4		216.9		114.0		113.5		402.2		241.0		377.1		256.0	

extremes are grouped disagreement is shown in the remaining distributions on plots 13, 47 and 48.

The data again indicate the reproducibility of results and general lack of personal bias. This can also be seen in Table II.20. This table also shows that there is no bias in the results obtained by the three types of apparatus. The means obtained by the two observers for each individual plot were also tested for significant differences. On plot 13 with the 2 in. spacing Observer A obtained a mean significantly higher than Observer B, and on plot 14 using the 6 in. spacing Observer B obtained the higher figure. None of the other differences was significant. Differences between the two variances on individual plots were also tested, and none proved significant.

This extensive series of tests of the method under widely different conditions indicates the objective nature of the method. Provided precise instructions are given on what exactly is to be recorded it is apparent that the method tends to eliminate personal error, which is extremely important in experiments carried on over a long period.

Table II.20. The mean percentage of crown cover obtained by two observers on 4 plots at Makaveti

Plot No.	13			14			47			48		
Observer	A	B	Mean	A	B	Mean	A	B	Mean	A	B	Mean
Spacing												
2 in.	86.0	76.0	81.0	68.8	73.2	71.0	72.6	73.8	73.2	68.4	64.6	66.5
4 in.	85.0	86.6	85.8	69.4	70.2	69.8	73.4	71.6	72.5	67.0	59.8	63.4
6 in.	82.8	81.6	82.1	65.0	73.5	69.3	70.2	68.6	69.4	67.2	60.6	63.9
Mean	84.6	81.4	83.0	67.7	72.3	70.0	72.1	71.3	71.7	67.5	61.7	64.6

5.3.3 Comparison with Square Meter Quadrats

The square frame used for estimating percentage crown cover was divided into 100 decimeter squares by cross wires, and the crown cover in each of these small squares was estimated by eye. Each square which was half or more than half covered was regarded as one per cent cover; squares less than half covered were regarded as bare. Cover was considered to be provided by growing grass, by grass projecting from nearby clumps, in adjacent squares, and by root-mats bare of foliage. This is in conformity with the criteria used for determining a hit by the point quadrat method.

The same two observers recorded each quadrat when in position. The comparison between the results obtained will therefore be a measure of the difference between observers and will not be a measure of the reproducibility of results. In order to measure the reproducibility of results, a second set of readings was taken on plot 14. Whenever there was a break in recording, even the short period taken for lunch, it was found necessary to record the first quadrat together, before proceeding to independent estimates. If this were not done wide discrepancies occurred. This first quadrat has been ignored in all comparisons. The results are given in Table II.21. The last column of this table gives further values for comparison. This was obtained by taking the mean of the three estimates from three entirely different recorders on five quadrats in each of the plots. This for convenience will be called the estimate by observer C.

Good agreement is obtained between the two recorders for the first sample on plot 14, and on the remaining three plots. In addition the estimates by the point quadrat method, and by observer C on plots 13, 14 and 48, show generally good agreement with the two estimates by A and B. On plot 47, however, although the point quadrat estimate is in fairly good agreement with the new estimate that obtained by C is somewhat low. This may be because the number of samples is not sufficient, or because of personal bias affecting the results. The means, however, indicate that there is no persistent bias.

In the second sample on plot 14, there is a significant difference between the two observers, in spite of the precaution of recording the first quadrat together. This is immediately obvious in the individual estimates

Table II.21. Estimates of crown cover using square meter quadrats

Plot No.	No. of quadrats in sample	Observer A: Mean per cent crown cover	Observer A: Variance on transformed scale	Observer B: Mean per cent crown cover	Observer B: Variance on transformed scale	Mean per cent crown cover obtained from all point quadrats	Cover estimated by C
13	12	79.5	52.4	80.7	43.9	83.0	84.4
14(i)	11	74.9	55.0	74.5	74.8	70.0	75.4
(ii)	30	76.3	47.8	69.7	34.3		
47	11	61.7	113.0	65.6	87.1	71.7	47.4
48	11	50.9	159.3	52.6	147.9	64.6	55.8
Mean		68.7		68.6		72.3	65.6

of each quadrat; in every case except two, B's estimate is lower than that of A. This is further evidence, if more is required, of the danger of using estimates only instead of using objective methods for obtaining data. In spite of this, surprisingly good agreement between the two methods was obtained in general.

The variances on the transformed scale are all higher than the approximate variance of 8.2, which should be approached when the number of squares is increased. This of course is not unexpected, but in general the variances are not substantially smaller than those obtained using the frames of ten nails. It would be necessary then to utilise almost the same number of quadrats as frames if the same accuracy is required. With the longer time necessary for recording the quadrat this is a distinct disadvantage.

The point quadrat method has also been compared with the percentage area method for determining basal area by West (1937). An apparatus similar to the nails used in this investigation was used, but instead of placing it in a predetermined position, it was thrown, and where it came to rest it was turned over so that the 10 points came into contact with the ground. It is now recognised that throwing hoops and other apparatus of this kind does not produce a random sample. When thrown the nails of this apparatus would catch on the tougher and larger plants, and a sample biased in favour of these plants would be obtained. West found, for example, that the bare bulbous stem bases of *Digitaria tricholaenoides* secured more hits than would be expected from a comparison with the results obtained by the percentage area method. In addition to the bias introduced by this method of sampling, a non-random distribution resulting from the points being only 2 in. apart would also be obtained. It is not surprising, therefore, that the results obtained by West from the two methods should show some disagreement, but it is wrong to conclude that the point quadrat method of sampling gives inaccurate results, for the comparison is not a valid one in his case.

A further criticism of the method has been made by Goodall (1952 (b)). He showed that the diameter of the pin used in the apparatus affects the results obtained, and he claimed that the method would therefore tend to over-estimate the actual cover present, and that dimensionless points should be used to obtain a true estimate. One of the objects of determining cover is to measure the soil exposed to erosion by rain. Drops of rain cannot be regarded as having no dimensions, and from this point of view Goodall's suggested method would tend to underestimate cover. This point, however, is academic and is not worth further consideration. The main point is that results must be comparable, and whether, for some purposes, the estimate of cover obtained is regarded as too high, or, for others, too low, is immaterial. In order to obtain comparable results it is necessary therefore to use pins (or nails) of the same diameter. This is all that is necessary; there is no need to insist on pins being as fine as possible. If very fine pins are used they will probably suffer from the defect of flexibility and bend when put in position.

PART III

PROBLEMS RAISED BY THE INCLUSION OF ANIMALS IN EXPERIMENTS

1. The Accuracy of Animal Weights

1.1 Introduction

In pasture research the effects of different treatments are often measured by pasture yield and composition. This is one very important aspect of the problem, but the eventual effect on the grazing animal is equally important and, in general, it is far more difficult to determine, for many more practical difficulties in the experimental procedure occur, and there is a substantial increase in costs. The effects of various pasture treatments on the grazing animal are often measured by live-weight increase, but before any reliance can be placed on such a measurement, the variation to be expected, and various factors influencing this variation must be examined.

In 1910 Wood and Stratton gave a brief discussion on variation in live-weight increase, but the examination was not very detailed. They found that the probable error (0.6745 x standard deviation) of live-weight increase on a number of animals was about 14% of the mean. The earliest detailed study of the problem was made by Lush and Black (1927), which was followed in 1928 (Lush *et al*) by a further examination, with corrections to the methods and formulae used in the earlier report. At that time it would appear that several workers weighed animals on 3 consecutive days and used the average of the 3 days' weights as the actual live-weight. These papers were in support of this practice and it was stated that there was a much smaller variation if this were done. Age and breed of cattle were also examined and it was stated that younger cattle appeared to have a lower variation, but that breed had little effect.

The method of using the mean of 3 consecutive days' weights appears to have been accepted for several years, but in 1942 Baker and Guilbert suggested that the merits of the method were not quite as great as had been claimed by Lush and his co-workers. On the assumption of a linear increase in weight throughout two periods of 30 consecutive days' weighings it was found that there was a significant auto-correlation between deviations from the straight line increase. Taking into account this correlation it was pointed out that the variation to be attached to the mean of 3 consecutive days' weights would be increased, and using the figures obtained in their the American Society of Animal Production and the American Society of Range Management (1952) does not seem to consider the problem completely settled. Some workers still seem to be convinced that better results will be obtained by using average weights. Before proceeding further therefore, it appears necessary to examine the various attacks on the method in more detail.

1.2 Average Weights Versus Single Day Weights

In their early paper Lush *et al* (1928) state that there are two principal causes of the fluctuation in live-weight from day to day. The first consists of comparatively large changes in the average of a group, in which all, or nearly all, of the cattle gain or lose weight together. Such changes were termed "fills" or "shrinks" in accordance with the market usage of the terms. The causes of these changes are often easily recognisable in environmental factors, such as changes in temperature, shipment to market, etc. The second consists of fluctuations made by individual animals, which are independent of the daily trend of the group. This inherent variation, which cannot be overcome by standardising external conditions is caused by normal differences in the animals' behaviour before weighing. It has been shown for example (Woodman *et al*) that the amount of herbage consumed varies from day to day, and in addition, the amount of water taken in, the time of evacuation of the faeces, and other individual habits will vary. It was with this inherent variation that Lush *et al* were primarily concerned. They found that the average standard deviation was 10.0 lbs. with a range from 4.8 lbs. with calves of 10 months old, having an average weight of 398 lbs. to 17.7 lbs. with steers of 2½ years old, weighing 915 lbs. on the average.

The results obtained by Bean (1946, 1948), Baker et al (1947) and Patterson (1947) appear to give conclusive evidence that the recommendation made by Lush and his co-workers was wrong. These workers, however, were considering a completely different type of variation; it is not surprising therefore that their conclusions are different. The paper by Patterson gives a clear description of the method employed. In addition to the two sources of error mentioned by Lush et al, he includes for consideration the variation between animals. This, together with the second type of variation discussed by Lush et al - the inherent type of variation - were examined. He states that it is possible by three consecutive weighings to effect a considerable reduction in the variation due to inherent fluctuations of the individual animal, which is in agreement with the results given by Lush et al. In experiments, however, differences between groups of animals are tested and Patterson considered that variation between animals must be taken into consideration. As this considerably larger than the inherent variation in weight of a single animal it is obvious that when the two types of variation are combined, the "considerable reduction" in inherent variation will amount to a very small percentage reduction in the combined total variation. Bean and Baker et al do not give such a clear description of the methods they have employed, and following the paper by Lush et al, the contradicting conclusions rather confused the problem. Like Patterson, however, they have used the combined variation from these two sources of error.

The problem here then is not to consider whether the two opposing sets of data are right or wrong, which would appear to be the case on first inspection, but to consider whether it is really necessary to include variation between animals. If it is, then obviously a single weighing is all that is necessary. If, on the other hand, variation between animals can be ignored or reduced sufficiently to a magnitude comparable with the residual variation, then the use of a 3-days' average will be better.

The paper by Baker and Guilbert (1942) is concerned with residual variation and ignores the variation between animals. If this is done, considerable reduction in the residual error is not disproved, but it is shown that the amount by which it is reduced is not as great as would be expected if a day's weights were independent of those on the preceding and following days. The main problem therefore is still to consider whether variation between animals must be taken into account.

The answer to the question is not as obvious as the opponents of the use of averages appear to think; it will depend upon the type of problem under investigation.

If, for example, an experiment is testing various pastures, or various methods of feeding, and different groups of animals are used for each treatment, it is necessary to take into consideration the variation between animals. In this case, therefore it would appear to be pointless to weigh on three consecutive days. There is still, however, the question as to whether this variation between animals can be reduced. Yates (1934) has shown that by using the analysis of covariance to correct for initial weight of animals it is possible to obtain a considerable reduction in variation between animals. Table III.1 shows that it is possible, with different animals and under different conditions, to reduce the variation between animals to a magnitude similar to the residual error variation by correcting for initial weight. Yates also suggests that it may be possible that a further reduction may be obtained by using average weights. Some reduction was in fact obtained when averages were used in his experiment, but he does not examine the question in detail.

If the correlation coefficient between the final experimental weights and the initial weights is denoted by r then the variance between animals after correcting for the initial weight will be $(1 - r^2)$ times the uncorrected variance. Patterson (1947) showed that the variation obtained from a single day's weighing will be $s_a^2 + s_r^2$ where s_a^2 represents the variance between animals and s_r^2 represents the residual variance. When animals are weighed on d consecutive days the variance of the average weights will be $s_a^2 + s_r^2/d$. The corrected variance in the two cases will therefore be $(s_a^2 + s_r^2)(1 - r^2)$ and $(s_a^2 + s_r^2/d)(1 - r_o^2)$, where r_o is

Table III.1. Variation for different types of animals, together with correlation coefficients between initial and final weights under different conditions

Type of animals	No. of animals in groups	Mean weight (lb.)		Period (days)	Correlation Coefficients					C.V. between animals		Residual C.V. of final weights
		Initial	Final		1st day	2nd day	3rd day	Mean	Using averages	Before[x]	After[x]	
Pigs[+]	8	90	149	92	.670	.659	.681	.670	.688	9.00	6.95	1.01
		98	149	56	.857	.879	.898	.878	.879	9.00	4.64	
Pigs[+]	9	37	164	98	.731	.682	.691	.701	.731	9.38	6.85	1.35
		64	164	69	.831	.743	.813	.796	.797	9.38	6.06	
Pigs[+]	9	48	169	85	.300	.456	.481	.412	.415	6.56	6.38	1.90
		87	169	57	.788	.902	.769	.820	.906	6.56	2.97	
Cattle	10	865	1002	63	.986	.987	.983	.985	.991	9.86	1.41	0.76
		865	953	28	.981	.992	.988	.987	.996	10.10	0.92	1.47
Cattle	10	955	889	28	.980	.955	.971	.969	.973	12.89	3.17	1.56
Cattle	8	785	933	190	.845	.816	.860	.840	.842	15.45	9.02	0.76
Cattle	21	800	948	190	.853	.866	.893	.870	.873	11.93	6.30	0.72
Sheep	8	59	66	62	.976	.965	.974	.972	.980	13.13	2.79	1.29
Sheep	11	11.2	20.1	98	-.102	-.092	-.049	-.081	-.081	20.05	-	1.06
Chickens[+]	10	2.10	4.29	77	.736	.807	.757	.760	.778	17.66	14.44	2.90

[+] Pigs were weighed on alternate days; chickens once per week; other animals on three consecutive days.

[x] Before and after correcting for initial weight differences.

the coefficient of correlation between the final average weights and the initial average weights. As S_r^2 is small in comparison with S_a^2, the advantage of using averages will be mainly dependent on the relative values of r and r_a. In general, r_a will be greater than r because the random variation from the "true" weight will be smaller when averages are used. If there is a considerable increase in the correlation coefficient, then it would be worth while using averages. It is difficult to determine the exact form the change will take, but as both coefficients are estimates of the true correlation coefficient, ρ, it is unlikely that the increase will be very great. Table III.1 shows the correlation coefficients obtained for single day weights and average weights of three days. In all cases the coefficient obtained using averages is slightly higher than the mean of the three single-day coefficients. The increase, however, is extremely small and will make little difference to the corrected variation between animals. In this case, when two groups of animals are used, it is therefore unnecessary to weigh on more than one occasion to determine live-weight increases for comparison.

There are occasions, however, when using the average of several days' weighings will be an advantage. When comparing different techniques in weighing, such as time of day, under shelter, etc., or when comparing the accuracy of different assistants, comparisons between groups of cattle do not enter into consideration, and it is essential to weigh on different days. In addition, some experiments are designed to compare the output of a pasture as measured by live-weight increase, and as measured by herbage clippings from small randomly scattered plots. For accuracy in such cases it is necessary to weigh on more than one day. According to Bean (1948) this may give a biased result, but no valid evidence is given for this conclusion, and the danger of bias need not enter into consideration.

In examining the reduction in variation to be expected from the use of averages it is necessary now to return to the paper by Baker and Guilbert. They showed that there was a definite correlation coefficient between the deviations of live-weights from a linear increase. Lush and Black (1927) appear to have utilised a correlation coefficient along the same lines as they suggested, but no correction for correlation was made in the later paper by Lush *et al* (1928).

In order to test the general applicability of the correlation between daily weights the live-weights of several animals were examined. Six mature sheep, which were more or less stable in live-weight, were weighed on 10 consecutive days. No trend in live-weight during the period was found; there is therefore no need to make any allowance for trend. There was no significant auto-correlation for any of the individual animals, nor was the overall correlation significant. It was felt that although no linear trend was evident that possibly differences in daily weights may be masking a significant auto-correlation. Daily weights were therefore corrected for differences between days, but again no significant correlations were obtained.

A group of nineteen cattle were also utilised for a further examination, and were weighed on 11 consecutive days. For each animal a multiple regression of one day's weight on time and the previous day's weight was found. Linear trend was significant in 13 cases, and of these only one showed a further significant auto-correlation between daily weights. Two of the remaining six which showed no significant daily trend gave significant correlations between consecutive daily weights. In other words, only three out of 19 animals gave a significant auto-correlation when daily trend was eliminated.

It is therefore apparent that significant auto-correlations are not always obtained. The periods involved here are short, and with a longer period similar to that used by Baker and Guilbert it is possible that the correlation may have become significant. Its value, however, would be small, and the effect it would have on reducing accuracy similarly small. It does not therefore appear to be of very great practical importance, and can be ignored when only three days average weights are being considered. If weights are taken on more than three days and averages used, then it may be important to take correlation into account. It is however, not advisable to weigh on more than this somewhat arbitrary number of three days. Lush *et al* point out that cost and labour may preclude weighing on more days. In addition the reduction in variation will be proportional only to the square root of the number of weighings and not to the actual number involved. If correlation is important the reduction in variation will be even smaller. One further factor also influences the efficiency of using a number of days for weighing. If an animal is weighed over a comparatively long period, it may have had time to gain or lose weight between the first and last days on which it is weighed; the average will not then be an estimate of its final weight. In investigations made here it would appear that it is inadvisable to weigh on more than 4 or 5 consecutive days if this effect is to be avoided.

1.3 Other Methods of Reducing Variation

As averages are not efficient in reducing variation in experiments utilising different groups of animals other methods of reducing variation between groups of animals must be examined. One obvious method is to increase the number of animals in each group, thereby decreasing the variance of the mean, but there is a very definite, rather low, limit to which numbers can be increased in practice, because of increases in area of land required, and increased cost of the experiment. Another method by which variation between groups can be lowered, is to allot similar animals to each of the groups. This will increase the variation within a group, but will decrease the variation between groups. ("Group" is here used in the normal sense when group-feeding trials are carried out, but it can also be replaced by the term "treatment", when single animals are used for each replication.)

In some experiments, workers have gone to a lot of trouble to obtain "balanced" groups of animals for experimental purposes, in the belief that the variation will thereby be considerably reduced. One extreme case of choosing animals as nearly alike as possible is quoted by Wood and Stratton (1910). During a preliminary period of 10 months, four steers in a group had shown an almost identical increase in live-weight, their ages were similar, past history identical, and their actual live-weights all within 10%. When they were put on a fattening ration, however, they began to diverge, and after three months the variation in live-weight increase was similar to other groups of animals, which had not been so carefully selected. This admittedly is only a very small number of animals on which to base any definite conclusions, but it would appear that any complicated system of "balancing" groups is unnecessary. In 1933, Dunlop showed that age, sex and condition and previous growth rate had no effect on the rate of live-weight increase of pigs, and he also concluded that the basis on which animals were allotted to groups to ensure homogeneity between groups was fallacious.

On the other hand, of course, it would be useless to include animals widely divergent, and some simple method of selection is desirable. The simplest method is to select <u>all</u> of the experimental animals to be of similar age, weight and condition, and allot them at random to the different treatments. The analysis of covariance will enable the effects of individual differences in initial weight to be eliminated, without any account of them being taken in the original design. This then gives a greater freedom of experimental design for other purposes. If animals of nearly equal weight are grouped together and one or more from each group allocated to each treatment, a further set of restrictions is introduced into the design, and as the number of treatments increases, the groups must be increased in size and the method becomes progressively less efficient.

Another method whereby some workers consider that the variation between animals will be reduced is by considering live-weight increase instead of actual final live-weight. Patterson (1947) considers the two cases separately, and points out that the variance of a gain in weight will be equal to

$$\sigma_1^2 + \sigma_2^2 - 2\rho\sigma_1\sigma_2$$

where σ_1^2, and σ_2^2 are the variances of the actual initial and final weights respectively and ρ is the correlation between the two weights. In a large number of experiments the values of σ_1 and σ_2 will be similar, so that this formula can be reduced approximately to

$$2\,\sigma^2(1 - \rho).$$

As ρ increases therefore, the variance of gains in live-weight will be reduced and, in general, the variance will be smaller than σ^2, for in practice ρ will be greater than 0.5. This is then the possible justification for using live-weight gains instead of final live-weights.

It is not necessarily true, however, that ρ will be greater than 0.5, as shown in Table III.1. In cases where the experimental period is long, the correlation between initial and final weights will decrease, because all animals do not react in the same way to all sets of conditions. When the period is extensive the variation in external conditions will increase, and the effects of differences in reaction will also increase. It is also possible that if very young animals are put onto the experiment, disease and other factors will not have allowed the potentially good animals to show their superiority before the experiment begins. This is shown in one of the sets of data for pigs. The result obtained from these data would tend to support the suggestion made by Dunlop (1933) that pigs under 70 lbs. live-weight should not be used for experimental purposes. The other two sets of data, however, do not support this. The use of live-weight gain, therefore, will not always result in a decrease in error.

Even if ρ is greater than 0.5, and reduction in variation is obtained, it is still not advisable to use live-weight gains, for if the analysis of covariance is used to eliminate initial differences in live-weight, then using live-weight gain becomes identical with using final weights. This can easily be seen from the following considerations:-

Let x_i denote the initial weight of the i th animal
y_i denote the final weight of the i th animal
and g_i $(= y_i - x_i)$ denote the gain of the i th animal

$$Sx^2 = \text{Sum of } (x_i - \bar{x})^2$$

$$Sxy = \text{Sum of } (x_i - \bar{x})(y_i - \bar{y})$$

Then the residual sum of squares for gains, after correcting for initial weight differences will be

$$Sz^2 - \frac{(Sxz)^2}{Sx^2}$$

$$= S(y-x)^2 - \frac{\left[Sx(y-x) \right]^2}{Sx^2}$$

$$= Sy^2 - 2Sxy + 2Sx^2 - \frac{\left[(Sxy)^2 - 2Sx^2Sxy + (Sx^2)^2 \right]}{Sx^2}$$

$$= Sy^2 - \frac{(Sxy)^2}{Sx^2}$$

which is the residual sum of squares for final weights, after correcting for initial weight differences.

The use of live-weight gains in comparisons therefore presents no advantage in reducing variation and because of the necessity for subtracting initially gives further opportunity for arithmetical errors - a distinct disadvantage. It is therefore advisable to use final weights only, for comparison and utilise the method of analysis of covariance.

1.4 Recording Weights

The next problem is to consider the accuracy with which weights should be recorded. With cattle used by Lush et al, some of the weights were recorded to the nearest 5 lbs. and some to the nearest 1 lb. They concluded that weighing to the nearest 5 lbs., would sacrifice a considerable amount of information, and that weighing to the nearest 2 lbs. was sufficient in most cases, except with really small cattle, when weighing to the nearest 1 lb. would be better. These conclusions are based on Fisher's statement (1925) that the loss of information is less than 1%, provided the group interval does not exceed one quarter of the standard deviation.

Table III.2 shows the standard deviations obtained by Lush et al in their investigations with cattle, together with the coefficient of variation determined from their results. Eight of the groups of cattle were weighed on several occasions with short intervals between dates of weighing and the estimates of the coefficient of variation in these cases will not be independent. There appears to be a definite association between the residual variation obtained from a group of cattle weighed at

Table III.2. Coefficient of variation for various groups of cattle, using the residual error variance (from data given by Lush *et al.*, 1928)

No. of Cattle	Mean Wt.	S.D.	C.V.	No. of Cattle	Mean Wt.	S.D.	C.V.
100	529	6.7	1.27	30	871	8.2	0.94
67	795	7.4	0.93	26	848	12.1	1.43
99	628	11.9	1.89	30	388	8.1	2.09
98	895	10.7	1.20	30	382	7.9	2.07
42	427	7.3	1.71	30	389	6.0	1.54
45	717	6.6	0.92	30	446	7.3	1.64
45	860	8.6	1.00	30	499	6.7	1.34
45	506	5.8	1.15	30	462	6.6	1.43
45	308	5.4	1.75	30	576	8.2	1.42
44	666	5.3	0.80	30	575	8.6	1.50
22	840	14.6	1.74	30	576	9.1	1.58
21	1179	11.2	0.95	30	662	9.4	1.42
26	542	8.3	1.53	30	615	11.5	1.87
25	876	6.2	0.71	30	655	11.5	1.76
24	780	11.5	1.47	30	898	13.7	1.53
24	1131	15.2	1.34	30	899	14.5	1.61
20	586	14.9	2.54	30	916	16.9	1.84
20	857	10.5	1.23	30	929	13.2	1.42
29	632	6.2	0.98	30	936	11.3	1.21
29	947	7.3	0.77	30	955	8.2	0.86
32	422	6.1	1.45	64	913	17.7	1.94
31	686	7.2	1.05	93	678	8.1	1.19
36	398	4.8	1.21	72	882	16.1	1.83
36	633	5.3	0.84	6	849	16.7	1.97
30	582	10.5	1.80	4	860	12.7	1.48
30	795	8.9	1.12	6*	1051	11.2	1.07
18	847	10.9	1.29	24*	1058	8.7	0.83
25	870	12.0	1.38	24*	1048	8.0	0.76
24	836	10.1	1.21	20*	1025	9.2	0.90
24	839	13.1	1.56	19*	995	10.7	1.07
34	804	16.0	1.99	20*	1010	10.7	1.06
29	822	11.6	1.41	20*	1120	12.3	1.10
28	860	13.5	1.57	20*	1057	9.8	0.93

*Several dates of weighing grouped together to determine a common standard deviation (small changes in number of animals were ignored).

different times. In other words a more variable group at one weighing appears to be more variable at a later date. In order not to bias the result therefore the weighings were combined together and a common standard deviation determined for each of the 8 groups, from which the coefficients of variation were found. When the dates were sufficiently wide apart, the association appears to decrease, and other groups of animals have not been combined in this way.

Groups of animals with a high mean weight are associated on the average with a high residual variation. The correlation between mean and standard deviation is significant at the 0.1% level of probability. It is to be expected therefore that the coefficient of variation, which is the standard deviation divided by the mean, should be a fairly stable value. Table III.2 shows that the actual range in coefficient of variation is small, and the mean value obtained was 1.57%. Thus for a given species of animals it would appear that the variation in live-weight is an almost constant ratio of the mean live-weight of the group of animals. It is possible, however, to go even further than this. Table III.1 gives the coefficient of variation, using the residual error variance, for a number of different species of animals. The mean value of these coefficients for all types of animals is 1.36%, which is in very close agreement with that obtained for cattle alone, and the range of variation is again small. The highest value obtained was for chickens. In this case, however, the birds were not weighed on three consecutive days, but at weekly intervals. The estimate is therefore biased too high because, in addition to the residual variation, there is the interaction between birds and time of weighing. In other words during the comparatively long period of one week, there is time for a distinct change in weight, and all birds will not behave similarly during that time - some will gain weight at a slow rate while others will gain weight much more quickly.

It seems possible therefore to regard the average coefficient of variation for the residual error of the live-weights of most animals as about 1.36%. Taking one quarter of this, the accuracy to which animal weights should be taken is therefore 0.34% of the mean weight of the group. Thus with large animals of about 1000 lbs.

weight can be recorded to the nearest 5 lbs. but with smaller animals of 50 lbs. weight to the nearest 0.2 lb. should be taken.

2. Time of Weighing Cattle

2.1 The Experiment

It has been shown that the residual error variance when weighing several types of animals gives a coefficient of variation equal to about 1.36% of the mean. With large animals of 1000 lbs. live-weight, the standard deviation would therefore be about 13.6 lbs., so that a difference of some 30 lbs. could occur by chance between two consecutive days' weighing under uniform conditions. Large variations of this order have been noted by several workers. As early as 1845, Boussingault found that a horse, when weighed at the same time on consecutive days could vary in weight by as much as 30 lbs., and Morrison (1947) states that the weight of a steer can vary by 20 - 40 lbs. from the weight of the previous day, when weighed under apparently similar conditions.

With such large day-to-day variations it is obvious that the technique of weighing should be examined in order to try to reduce these errors. Hughes and Harker (1950) give a summary of the investigations carried out by earlier workers, together with results of their own investigations.

Their conclusions are most interesting, but it is difficult to know how much reliance can be placed on them, for there appears to be little statistical control of the experiments, and no statistical analysis of the results. The results of the present experiment will be examined and compared with their results.

Forty-five cattle were used, and divided at random with certain restrictions into three groups A, B and C. Within each of these three main groups there were three subgroups, denoted by 1, 2 and 3. The first of these subgroups consisted of five two-tooth grade steers, the second consisted of five four-tooth grade steers and the last consisted of five mature zebu oxen. Each of the main groups was put onto a different treatment overnight for a period of five days, and then subsequently change round for another period of five days, when they were again changed round. The following system was adopted:-

GROUP	PERIOD		
	1	2	3
A	I	III	II
B	II	I	III
C	III	II	I

The Roman numerals denote the overnight treatments, during which no food was allowed, and the treatments differed solely in access to water: I denotes free access to water all night, II denotes no water during the night but water offered before weighing in the morning, and III denotes no watering overnight and no access to water before weighing in the morning. In all cases animals were given the chance to drink in the previous evening before being shut in the pens.

The daily routine during the experiment was as follows:- At about 7.15 a.m. (local time) the animals in the group who were allowed free access to water during the night were weighed, and during this period, the group allowed water before weighing were given access to water. When both of these groups had been weighed, the water was removed, and the final group was weighed. The weighing of all animals was completed by about 7.45 a.m. All groups were then driven along a road which gave little opportunity for roadside grazing, for a distance of about half a mile and were then returned to the weigh-bridge for a second recording. After this second weighing (at approximately 8.45 a.m.) all animals were driven to the pasture.

At about 3 p.m. they were all returned to the night pen where they were given good quality oat hay ad lib. At 5 p.m. all cattle were again weighed, and then allowed free access to water. When the last steer had finished drinking all animals were re-weighed. This second evening weighing was completed at about 6 p.m.

2.2 Live-weight

Table III.3 shows the mean live-weights for each group of cattle at each time of weighing. These results are remarkably consistent for each group of animals and for each period of observation. A loss of about 3 lbs. is made during the walk between the two weighings in the morning; after the period of grazing during the day, there is very little difference in live-weight from that of the second morning weighing, with possibly a slight tendency to decrease; and an increase of over 40 lbs. on the average is attained after drinking in the evening. The small fall in weight between the two weighings in the morning is statistically significant at the 0.1% level of probability, and the large increase between the two evening weighings is also highly significant. The difference between the second morning and first evening weighing is small and is not significant. It is surprising that the weight of the animal empty in the morning should be the same as that after a full day's grazing. The large differences in live-weight obtained at different times of weighing during the day emphasise the importance of weighing at a fixed time for experimental

Table III.3. <u>Mean weights of groups of animals, under different treatments and at different times of weighing</u>

	Treatment											
	I				II				III			
	Time of Weighing											
	1	2	3	4	1	2	3	4	1	2	3	4
Group												
A	787	782	773	821	778	775	787	812	790	786	780	827
B	844	838	832	883	835	830	822	878	832	830	842	871
C	774	771	783	806	787	783	774	820	777	774	766	816
Mean	802	797	796	837	800	796	794	837	800	797	796	838

<u>Treatment</u>

I Denotes free access to water all night

II denotes no water during the night, but offered water before weighing in the morning

III denotes no access to water before weighing in the morning after a night without water.

<u>Time of weighing</u>

1 Denotes first morning weighing

2 denotes second morning weighing

3 denotes first evening weighing

4 denotes second evening weighing

purposes.

The differences between treatments are small in the extreme: the mean values for all groups of animals are almost identical. Hughes and Harker state that "the deleterious effect of fasting, particularly the withholding of water (Schalk and Amadon, 1928), may take many days to overcome". The results of this experiment, however, do not appear to support this statement, for there is no loss in weight when animals are not allowed water during the night. It may be argued of course that five days is too short a period for any change in weight to be significant. This argument is valid, but although a change in weight may not be significant a slight decrease in weight should be apparent. This perhaps is a matter of opinion, but it was observed during the experiment that all cattle drank very little or no water in the morning, when it was offered to them. This then seems to be conclusive evidence that water is unnecessary during the night, for these cattle under the weather conditions pertaining during the period of the experiment. The animals used in this experiment, however, were acclimatised to being without water for long periods due to their general management on the farm, and a further period of forced abstinence probably presented little or no further hardship. Many animals under tropical conditions are managed in a similar manner, with only very limited access to water. For such animals, therefore, it appears obvious that the presence or absence of water during the overnight period in the pens makes very little difference to the health of the animals. Under the very different management conditions in England, however, lack of water during the night may, as Hughes and Harker suggest, cause a set-back to the animals.

2.3 Residual Variation

The main reason for the experiment was to determine under what conditions the residual error variation would be the least. Table III.4 shows the mean residual standard deviations at different times of weighing and for different ages of cattle. Age of cattle appears to have the greatest effect: the older animals having the smaller variation. There is a slight overall decrease with the older grade steers, and a large decrease with the mature zebu oxen. The lower values for the older steers are significantly different from those obtained for the younger steers in the morning weighings, but the decrease in the evening is not significant. The difference between breeds is probably also an important factor in the large reduction obtained with the zebu oxen; being acclimatised to local conditions, variations in weight caused by changes in environment may not be so erratic. If age were the only factor active in reducing variation, the reduction with mature cattle would probably not be so noticeable; it seems safe to assume therefore that breed was also having an effect.

Table III.4. Mean residual standard deviations

Age of Cattle	Time of Weighing 1	2	3	4	Mean
1	7.3	6.9	6.3	8.1	7.2
2	6.9	6.0	6.1	7.6	6.7
3	4.7	4.4	5.2	5.2	4.9
Mean	6.3	5.8	5.8	7.0	6.2

Age

1 Denotes two-tooth grade steers

2 denotes four-tooth grade steers

3 denotes mature zebu oxen

Time of weighing also affects the residual variation. The difference between the two weighings in the morning is not significant, but the increase between the two weighings in the evening is significant. The effect of different amounts of water consumed by individual animals is obvious in this result. It is particularly surprising to find that the average variation for the first evening weighing is the same as that for the second one in the morning. Hughes and Harker stress the differences in behaviour of the different cattle during grazing in the day, and it would be expected to lead to a wider variation in weights at the end of the grazing period. This may be due to the fact that animals eat at different rates, but that the total consumed during the day is relatively constant for all animals. It may be possible also to infer from this, that if animals are interrupted in their grazing for weighing, that they will make up for loss in grazing time when returned to pasture. Hughes and Harker considered it important that weighing should occur at a time when there was the minimum of disturbance in the natural grazing habits. If the inference made above is permissible, this appears to be quite unnecessary.

All groups of animals gave similar results; it is therefore unnecessary to give the individual results for each group. There was in addition no real difference between treatments. Jones (1932) states that the withholding of water, however, results in a decrease in residual variation. He weighed two sets of animals at 10 a.m., one taken straight from pasture, and one after a period of sixteen hours fasting. If weighing had been earlier the results might have been different, for not only is the effect of overnight fasting being tested, but also the effect of several hours daylight grazing. Hughes and Harker found that the habits of a group of animals tended to diverge during the day, and if animals are weighed at a time when individual changes in behaviour are important, obviously the variation in live-weight will increase. It is possible therefore that their recommendation that animals should be weighed when there is least disturbance in grazing is correct. But the recommendation should be based on variations in weight and not on vague suggestions that the animals may thereby lose weight and condition. Hughes and Harker also weighed two groups of animals one having access to water overnight and the other deprived of water. They suggest that water reduces variation, but no evidence of this was found in the present experiment. The variation during the first period of five days was considerably higher than in the succeeding periods, presumably due to the animals being somewhat unused to the handling.

It will be noticed that, generally speaking, the higher residual errors are associated with higher mean weights as given in the previous section. It may therefore be possible that although differences in residual variation occur, these are eliminated when coefficient of variation is considered. Table III.5 gives the mean coefficients of variation for different times of weighing and different ages of cattle. It will be noticed that the same trends are obtained as in Table III.4, and in view of this fact it was felt it was unnecessary to subject the figures to statistical analysis, as it was obvious that in addition to an actual increase in absolute value (which is, of course, the important fact) there was also an increase in relative values.

Table III.5. <u>Mean residual coefficients of variation</u>

Age of	Time of Weighing				Mean
Cattle	1	2	3	4	
1	.91	.89	.78	.95	.88
2	.89	.79	.80	.95	.86
3	.58	.55	.64	.62	.60
Mean	.79	.74	.74	.84	.78

2.4 Variation Between Days

The above results would indicate that weighing in the evening after the cattle are allowed access to water is not advisable, and that there appears to be little to choose between the other three times of weighing, with possibly a slight preference for either the second weighing in the morning or the first weighing in the evening. There is, however, one further important source of variation - that between days. Differences between days - general gains or losses in weight - have been termed "fills" or "shrinks" by Lush *et al* (1928) in accordance with the market usage of these terms. The causes of these differences are often obvious in changes in environment, such as fall in temperature, rain, etc. It is important also to weigh at a time when such fluctuations will have the least effect.

Time of weighing is, in fact, the only factor, apart from changes during the three periods of the experiment, which affects variation between days. The average overall standard deviations for the four different times of weighing during the day were 13.8, 12.1, 18.1 and 12.0 respectively. The highest value is that for the first evening weighing (significant at the 0.1% level of probability). This is to be expected because changes in weather, such as rain, will affect the grazing of the animal and its total intake much more than it can affect habits during the overnight period in the pen. The effect of rainfall on the habits of the cattle is indicated in Table III.6

Table III.6. Mean difference in weight (in pounds) between second morning and evening weighings, and rainfall during the day

Date: November	Mean diff.	S.D. of diff.	Rainfall (ins.)	Remarks
2	- 5.0	1.43	.01	-
3	- 4.4	1.16	0	-
4	- 10.8	1.64	0	-
5	- 10.1	1.24	0	-
6	- 11.7	1.48	0	-
7	- 13.6	1.62	0	-
8	- 11.2	1.46	.15	-
9	- 0.2	0.83	.01	light rain
10	- 16.1	1.87	.03	drizzle
11	+ 7.6	1.58	.43	rain
12	+ 4.7	1.43	.20	rain
13	+ 7.7	1.30	.25	rain
14	+ 28.3	1.41	.57	heavy rain
15	+ 9.6	0.69	.02	rain
16	+ 9.2	1.69	.13	rain

During fine weather, the animals consistently lost weight during the period of grazing, but when rain fell a consistent increase in weight was obtained. There are three exceptions to this general rule - on the 8th, 9th and 10th November.

Rainfall is recorded for the 8th, but there was an appreciable loss in weight. The rainfall, however, is recorded at 8.30 every morning; that recorded on the morning of the 9th is taken as the rainfall for the 8th. It is possible that all the rain which was recorded as that for the 8th fell on the morning of the 9th. During weighing, remarks on weather conditions were noted, and these are also shown in the table. From these it would appear that no rain fell on the 8th, and that the 0.15 in. recorded for that date fell on the morning of the 9th. This amount of rain did not result in an increase in weight during the day; it did, however, change a significant loss into a negligible one. It is possible that after a long dry period, the effect of this amount of rain would not be noticeable, due to speedy evaporation and percolation through the soil. The same factor was probably operative during the 10th, and because of the much smaller amount of rain, an actual significant decrease was obtained.

The small amount of rain associated with a large increase in weight on the 15th is possibly surprising also, but the effect of the previous period of rain, and particularly the heavy rain on the 14th, some of which in any case probably fell on the morning of the 15th will still be operating. In spite of these exceptions, there is a significant relationship between rainfall and change in weight during the daily grazing period. This confirms one of the basic principles of feeding experiments: that animals consume the same amount of dry matter, irrespective of water content. When the herbage is wet after rain, the same amount of dry matter is still consumed, but the actual intake is greater, resulting in a gain in weight during the day. There is a second alternative which may be operating to increase weight during wet weather - the amount of water clinging to the coat of the animals. It would appear, however, that this is small. If it were the only factor operating, the same amount of moisture would be present in the morning and evening, when there was rain throughout the day, and there should be no difference. On several occasions it was raining in the morning but fine in the evening and a loss in weight should have occurred. Under both conditions a gain in weight was obtained. There appears therefore to be no point in trying to make corrections for water on the coat of the animal, for the effects of a larger intake when eating are far more important.

In spite of the low residual variation obtained for the first weighing in the evening, weighing at this time is not to be advised, because of the large fluctuations between days. The second weighing in the evening has already been eliminated because of the large residual error. The variation between days is small for this second evening weighing because animals compensate for lack of water intake during the day, by a large consumption during the evening. It was noticed, for example, that very few cattle drank water on the evening of the 14th following a day of heavy rain. Differences between days will therefore be smoothed out by allowing cattle access to water.

Attention is therefore directed to weighings in the morning. The difference between the variations between days for the two morning weighings was just significant at the 5% level of probability. In view also of the slightly lower residual variation, the second weighing after a short walk is therefore to be preferred. The differences in the two cases, however, are not large and if labour and management restrictions prohibit the morning walk, very little loss in accuracy should result. Several workers have deplored the fact that the weighbridge was some distance from the site of an experiment, but the results of this investigation indicate that it is far from a disadvantage.

3 Personal Error in Weighing

A further source of error which may affect the accuracy of experimental results with animals is the use of different observers for weighing. It is possible, particularly when experiments extend over a long period, that the person who weighed originally is not available to weigh the animals at the end of the experiment. It is necessary therefore to determine whether such a procedure is likely to result in any loss of accuracy.

In order to test this three African assistants weighed nine sheep on three consecutive days. The nine animals were divided into three groups of three animals, and the order in which these groups was weighed was randomised so that each group was weighed by each observer in a different order on each day. The differences between observers were very small, and of the same order as the residual inherent variation. It is apparent therefore that provided reasonable care is taken during weighing the use of different assistants does not lead to any serious error.

It was also possible to test the accuracy of the different assistants from the point of view of variability of the results. The residual error variance was found for each of them, and by using Bartlett's test for differences between variances, it was found that all gave similar results. The actual residual standard deviations obtained were 0.40, 0.33, and 0.30. In this case then all assistants were of virtually equal accuracy.

4. Grazing and Clipping Trials

4.1 Introduction

In Part I it has been shown that to achieve a coefficient of variation of 10 per cent for forage yields, the size of plot in general should be at least 430 square yards, or about 0.1 acre. Boyd (1949) suggests that this size or even possibly 0.05 acre could be used for grazing trials in England, if both clipping and live-weight increase are to be used as measures of productivity. Such a size would apparently be useless in East Africa, for the variability of the herbage yield alone would make the results obtained from sample cuts inconclusive. Even in England it is doubtful whether a plot as small as this would be worth while, for as Boyd points out, it introduces artificiality into the grazing management, and in addition, there is the danger of transference of fertility from one treatment to another, and also the uneven spread of droppings in the plot. It is difficult to determine the size of plot required for such an experiment, particularly in East Africa where little is at present known about the variation to be encountered. The results of Part I give some indication, but when animals are introduced into the experiment further information is required. The whole plot cannot be mown or clipped to obtain herbage yield, and some data to determine the size and number of sample units within the plot is required.

Several workers have used grazing and clipping techniques in pasture experiments and compared the results of the two methods. One of the most informative descriptions of such an experiment was given by Boyd (1949). The size of plot he used was about 5 acres, but because of the necessity for replication which was demonstrated in this experiment, he concluded that this was far too large. The size of sample unit in each plot was 7' x 1'4", and there were 4 units per plot. Large variation was obtained with sample units as small as this, but Boyd states, "The circumstance that sample cuts show considerable variability does not mean that they are incapable of providing valuable information; it should be remembered that the grazing animals are themselves subject to variability, although this is usually more difficult to measure. Moreover the grazing animals are subject to the operation of the factor of management; ..... The grass cutting techniques will at least give us a valuable record of what is available on the plots at any time during the season."

This last sentence is indicative of the present uncertainty characteristic of this type of experiment. This is even more forcefully expressed by Davies <u>et al</u> (1950), who states that "any experimental work on pasture productivity should in the first instance concern itself principally with a critical comparison of the methods that might be employed for its estimation."

The results obtained by Boyd indicate good agreement between the estimates obtained by grazing and clipping, but those obtained by Davies <u>et al</u> are not conclusive, although the "difference" method of estimating herbage yield gave results more in agreement with those obtained by grazing than the "pre-trimming" method. Robinson <u>et al</u> (1937) obtained comparable results from grazing and clipping estimates, but also found that the "difference" method was better. Other workers (see Brown, 1937, for references, also Brandt and Ewalt, 1939, Hodgson <u>et al</u>, 1942, Linehan and Lowe, 1946, 1947 and 1952) have also shown reasonable agreement in the results. In nearly all cases, however, the agreement was relative; i.e. the better pastures gave

4.2 The Experiment

A trial was conducted at Muguga to test the methods under East African conditions. The high variability generally associated with this type of experiment has been pointed out by many workers, and the even greater variability associated with tropical conditions led to the use of two sizes of sample units. The smaller one was the same as that used by Boyd (1949) at Rothamsted; the larger one was 3' x 45'. It was thought that an increase in size of the sample unit would reduce variation, and by making the difference so large it was thought that any benefit would be obvious. Unfortunately the grass chosen for the pasture was Kikuyu grass, which was shown in Part I to be extremely variable.

Two tests have been carried out on this pasture which was 60 x 60 square yards. In each case the sites of the sample units were selected by means of random co-ordinates, and 12 of the smaller and 10 of the larger units were used on each occasion. In the first period 11 cattle were put onto the area for 15 days, and in the second 4 cattle stayed on the pasture for 21 days. The results of the cuts from the sample units are given in Table III.7. In

Table III.7. Yield and variation obtained from sample plots in the grazing trial

	Size of Plot	3" x 45"			1'4" x 7"		
		A	B	C	A	B	C
First period	Mean	14.99	14.85	7.56	1.238	1.348	0.598
	Standard error	3.06	3.59	3.90	0.381	0.329	0.209
	Coefficient of variation	20.4	24.2	51.6	30.7	24.5	35.1
Second period	Mean	16.61	19.84	7.30	1.248	1.572	0.683
	Standard error	2.96	3.76	5.34	0.417	0.433	0.255
	Coefficient of variation	17.8	19.3	73.2	33.4	27.6	37.3

Column A: The initial cut from the protected sample plots

Column B: The final cut from the adjacent parallel strips in the protected plots

Column C: The final cut from the residue on the unprotected area.

both periods the variation from the larger units is lower for the two cuts in the protected areas. The difference is small compared with the difference in the size of sample unit, particularly in the first trial. If the form of the relationship between plot size and variation given in Part I is used, viz.

$$\log (C_x) = \log(C_1) - a \log (x)$$

and the results obtained here substituted, then the value of a in the first trial is 0.08 and in the second trial is 0.19. The particularly low value in the first trial is due to the equality of coefficients of variation for the second half of the protected plots. The value in the second trial is in agreement with the results obtained in Part I. In the case of cuts from the residue of herbage left after grazing on the unprotected area of the field, the coefficient of variation was lower for the smaller sample units. This was completely unexpected, and there appears to be no valid explanation; it merely emphasises the difficult nature of the grass used in the experiment.

Further evidence of the variability of the grass is obtained by comparing the two estimates of the residue after grazing, with the actual residue harvested from the whole plot. Using the large size of sample unit the estimate of the residue on the unclipped areas in the first trial was 1557 lbs. dry matter, with a standard error of 294 lbs., and using the smaller unit the estimate was 1781 lbs., with a standard error of 179 lbs. The actual weight obtained was 1325 lbs. This differs from the second estimate by more than twice its standard error, but the difference from the first estimate is less than its standard error. In the second trial the estimates and their standard errors were 1503 ± 348 and 2034 ± 219 for the large and small sample units respectively. The actual weight obtained was 354 lbs. This differs by much more than twice the standard error from each estimate.

The results of the investigation suggest that there may be some slight advantage in using the larger sample unit to reduce variation. But the evidence is not conclusive and far from consistent. In view of the extra work involved in cutting and the difficulty in mowing the larger units any slight theoretical advantage is completely nullified. As both units in any case give unreliable results, a discussion of their relative merits is possibly rather pointless.

A further criterion for testing the relative efficiency of the two sizes is to compare the results with those obtained from an investigation of animal weights. It would appear in fact that this is regarded as a standard for comparison, and that the closer the estimate obtained from some particular method of clipping to that obtained calculated earlier for the residue on the unclipped areas, due to plots being clipped on the final date of sampling). The estimated consumptions are therefore 1595.5 and 2178.6 lbs. dry matter respectively. With 11 animals for 15 days this gives consumptions of 9.67 and 13.20 lbs. dry matter per animal per day. During the second period the estimates of the available and final amounts of dry matter on the plot were 3911.5 and 1582.3 for the large sample unit and 4422.7 and 2321.4 for the small sample unit. The estimated total consumptions are therefore 2329.2 and 2101.3 lbs. dry matter. With 4 animals for 21 days, these represent consumptions of 27.73 and 25.02 lbs. dry matter per animal per day.

During each period the animals were weighed daily. As an estimate of the initial weight the average of three days' weights were taken - the morning of the day on which the animals were put onto the experimental plot and the day before and the day after. Similarly the final weight was taken as the average of the day before leaving the plot, the day they left the plot and the day after.

The mean initial and final weights of the 11 animals in the first period were 636.2 lbs. and 648.4 lbs.

representing an average increase of 12.2 lbs. Taking the mean of the initial and final weights of the animals, the average daily requirement for maintenance is

$$6.5\left\{\frac{642.3}{1120}\right\}^{\frac{2}{3}} = 4.49 \text{ lbs. starch equivalent}$$

And taking 2¼ lbs. starch equivalent as necessary for an increase of 1 lb. live-weight, the daily requirement for a total increase of 12.2 lbs. in 11 days is 2.50 lbs. starch equivalent. This means a total daily intake of 6.99 lbs. starch equivalent. The percentage starch equivalent in the dry matter of the herbage should therefore be 72.3 in the case of the larger sample unit and 53.0 in the case of the smaller sample unit. The former is too high to be obtained in practice, and the estimates of the amount of dry matter consumed are too low (see Woodman, 1952, page 30) for animals of this weight.

The mean initial and final weights of the 4 animals in the second period were 575.8 and 614.0 lbs. respectively, giving a mean increase of 35.2 lbs. In this case the average daily requirement for maintenance is 4.26 lbs. starch equivalent, and that for a daily increase of 1.68 lb. per animal is 3.77 lbs. starch equivalent. The average total daily intake is therefore 8.03 lbs. starch equivalent. The estimates of the percentage starch equivalent in the dry matter are therefore 29.0 and 32.1 for the large and small units respectively. These values are not exceptional. A daily intake of about 26 lbs. dry matter, however, represents an intake which is highly unlikely with animals of about 600 lbs. live-weight. According to Woodman (1952) a consumption of this amount would be required by animals of about 1400 lbs. live-weight.

It is evident therefore that there is little relationship in this trial between estimates from animal production and estimates from clipped herbage. The use of somewhat arbitrary conversion factors for expressing animal units in terms of herbage units will probably lead to some discrepancy. Forbes (1936) has in fact criticised the method, and states that its use will only lead to confusion. It is necessary however, to have some basic units of expression so that results from different animal experiments may be compared. In addition it serves as a useful check on the two techniques. It is nothing more however and should not be used in a strict comparison with the results obtained by clipping, and above all it should not be used as a standard for comparison.

In this experiment it is possible that the high variability of the grass itself is largely responsible for the discrepancies. It further strengthens the argument in Part I leading to the conclusion that Kikuyu grass must be treated with caution in experimentation, and avoided whenever possible.

REFERENCES

1. In the Introduction

Ahlgren H.L. (1947). A Comparison of Methods Used in Evaluating the Results of Pasture Research. J.Amer.Soc.agron., XXXIX, 240.

Watson, S.J. (1939). "The Science and Practice of Conservation: Grass and Forage Crops" (Vol.II. p.437) London. "The Fertiliser and Feeding Stuffs Journal".

Report of Joint Committee of the Amer. Soc. agron., the Amer. Dairy Sci. Ass., the Amer. Soc. Anim. Prod., Amer. Soc. Range Man. (1943) J. Dairy Sci., XXVI, 353.

——— (1952) Agron. J., XLIV, 39.

2. In Part I

Batchelor, L.D. and Reed, H.S. (1918). Relation of the Variability of Yields of Fruit Trees to the Accuracy of Field Trials. J. agric. Res., XII, 245.

Boyd, D.A. (1949). Experiments with Leys and Permanent Grass. J. Brit. Grassl. Soc., IV, 1.

Christidis, B.G. (1931). The Importance of the Shape of Plots in Field Experimentation. J. agric. Sci., XXI, 14.

Clements, F.E., Weaver, J.E. and Hanson, H.C. (1929). "Plant Competition", 287. Carnegie Institution.

Cochran, W.G. (1937). Catalogue of Uniformity Trial Data. Suppl. J. R. statist. Soc., IV, 233.

Davies, J.G. (1931). The Experimental Error of the Yield from Small Plots of "Natural" Pasture. C.S.I.R. Bull. 48.

Day, J.W. (1920). The Relation of Size Shape and Number of Replications of Plots to Probable Error in Field Experimentation. J. Amer. Soc. agron., XII, 100.

Dougall, H.W. (1953). Kikuyu Grass I. Seasonal Variation in Composition of Degenerate and Productive Pastures. E. Afr. agric. J., XIX, 74.

——— (1954). Kikuyu Grass II. An Attempt to Improve a Worn-out Pasture. E. Afr. agric. J., XIX, 212.

Garber, R.J., McIlvaine, T.C. and Hoover, M.M. (1926). A Study of Soil Heterogeneity in Experimental Plots. J. agric. Res., XXXIII, 255.

Hald, A. (1949). Maximum Likelihood Estimation of the Parameters of a Normal Distribution which is Truncated at a Known Point. Skand. Aktuar. Tidskr., XXXII, 119.

Harris, J.A. (1913). On the Calculation of Intra-class and Inter-class Coefficients of Correlation from Class Moments when the Number of Possible Combinations is Large. Biometrika, IX, 446.

——— (1914). On Spurious Values of Intra-class Correlation Coefficients Arising from Disorderly Differentiation Within the Classes. Biometrika, X, 412.

——— (1915). On a Criterion of Substratum Homogeneity (or Heterogeneity) in Field Experiments. Amer. Nat., XLIX, 430.

——— (1920). Practical Universality of Field Heterogeneity as a Factor Influencing Plot Yields. J. agric. Res., XIX, 279.

Hodnett, G.E. (1953). A Uniformity Trial on Groundnuts. J. agric. Sci., XLIII, 323.

Hudson, H.G. (1941). Population Studies with Wheat. J. agric. Sci., XXXI, 116.

Immer, F.R. (1932). Size and Shape of Plot in Relation to Field Experiments with Sugar Beets. J. agric. Res., XLIV, 649.

Justesen, S.H. (1932). Influence of Size and Shape of Plots on the Precision of Field Experiments with Potatoes. J. agric. Sci., XXII, 366.

Kulkarni, R.K., Bose, S.S. and Mahalanobis, P.C. (1936). On the Influence of Shape and Size of Plots on the Effective Precision of Field Experiments with <u>Juar</u> (Andropogon sorghum). Indian J. agric. Sci., VI, 460.

Lord, L. (1931). A Uniformity Trial with Irrigated Broadcast Rice. J. agric. Sci., XXI, 178.

McClelland, C.K. (1926). Some Determinations of Plot Variability. J. Amer. Soc. agron., XVIII, 819.

——— (1929). The Effect of Narrow Alleys on Small Grain Yields. J. Amer. Soc. agron., XXI, 524.

McRostie, G.P. and Hamilton, R.I. (1927). The Accurate Determination of Dry Matter in Forage Crops. J. Amer. Soc. agron., XIX, 243.

Mercer, W.B. and Hall, A.D. (1911). The Experiment Error of Field Trials. (With an appendix by "Student"). J. agric. Sci., IV, 107.

Metzger, W.H. (1935). The Relation of Varying Rainfall to Soil Heterogeneity as Measured by Crop Production. J. Amer. Soc. agron., XXVII, 274.

Pereira, H.C. (1953). Annual Report. East African Agriculture and Forestry Research Organisation, Kenya. Page 55.

Smith, H.F. (1938). An Empirical Law Describing Heterogeneity in the Yields of Agricultural Crops. J. agric. Sci., XXVIII, 1.

Stephens, J.C. and Vinall, H.N. (1928). Experimental Methods and the Probable Error in Field Experiments with Sorghum. J. agric. Res., XXXVII, 629.

Stringfield, G.H. (1928). Types of Field and Plot in Crop Tests. J. Amer. Soc. agron., XX, 1073.

Summerby, R. (1923). Replication in Relation to Accuracy in Comparative Crop Tests. J. Amer. Soc. agron., XV, 192.

Wassom, C.E. and Kalton, R.R. (1953). Estimations of Optimum Plot Size Using Data from Bromegrass Uniformity Trials. Agr. Exp. Stn., Iowa State Coll., Res. Bull., 396.

Wiebe, G.A. (1935). Variation and Correlation in Grain Yield among 1,500 Wheat Nursery Plots. J. agric. Res., L, 331.

Wood, T.B. and Stratton, F.J.M. (1910). The Interpretation of Experimental Results. J. agric. Sci., III, 417.

Zuber, M.S. (1942). Relative Efficiency of Incomplete Block Designs Using Corn Uniformity Trial Data. J. Amer. Soc. agron., XXXIV, 30.

3. In Part II

Acocks, J.P.H. (1953). Veld Types of South Africa. Bot. Survey. Mem. 28. Dept. Agric., Div. Bot., S.A. Pretoria.

Archibald, E.A.A. (1948). Plant Populations: I Ann. Bot., N.S., XII, 221.

——— (1950). Plant Populations: II Ann. Bot., N.S., XIV, 7.

Anscombe, F.J. (1949). The Statistical Analysis of Insect Counts Based on the Negative Binomial Distribution. Biometrics, V, 165.

Anscombe, F.J. (1950). Sampling Theory of the Negative Binomial and Logarithmic Series Distributions. Biometrika, XXXVII, 358.

Bartlett, M.S. (1947). The Use of the Transformations. Biometrics, III, 39.

Bauer, H.L. (1943). The Statistical Analysis of Chaparral and Other Plant Communities by Means of Transect Samples. Ecology, XXIV, 45.

Beall, G. (1940). The Fit and Significance of Contagious Distributions when Applied to Observations on Larval Insects. Ecology, XXI, 460.

Blackman, G.E. (1935). A Study of the Distribution of Species in Grassland Associations. Ann. Bot., XLIX, 749.

——— (1942). Statistical and Ecological Studies in the Distribution of Species in Plant Communities. Ann. Bot., N.S., VI, 351.

Brown, D. (1954). Methods of Surveying and Measuring Vegetation. C.A.B. Bull. 42.

Clapham, A.R. (1936). Over-dispersion in Grassland Communities. J. Ecol., XXIV, 232.

Clark, P.J. and Evans, F.C. (1954). Distance to Nearest Neighbour as a Measure of Spatial Relationships in Populations. Ecology, XXXV, 445.

Cottam, G. and Curtis, J.T. (1949). A Method for Making Rapid Surveys of Woodlands by Means of Pairs of Randomly Selected Trees. Ecology, XXX, 101.

Cottam, G., Curtis, J.T. and Hale, B.W. (1953). Some Sampling Characteristics of a Population of Randomly dispersed Individuals. Ecology, XXXIV, 741.

David, F.N. and Moore, P.G. (1954). Notes on Contagious Distributions in Plant Populations. Ann. Bot. London, N.S., XVIII, 47.

Fisher, R.A., Corbett, A.S. and Williams, C.B. (1943). The Relation Between the Number of Species and the Number of Individuals in a Random Sample of an Animal Population. J. Animal Ecol., XII, 42.

Fracker, S.B. and Brischle, H.A. (1944). Measuring the Local Distribution of Ribes. Ecology, XXV, 283.

Goodall, D.W. (1952 (a)). Quantitative Aspects of Plant Distribution. Biol. Rev., XXVII, 194.

——— (1952 (b)). Some Considerations in the Use of Point Quadrats for the Analysis of Vegetation. Aust. J. Sci. Res., B, V, 1.

Greig-Smith, P. (1952). The Use of Random and Contagious Quadrats in the Study of the Structure of Plant Communities. Ann. Bot., N.S., XVI, 293.

Hopkins, B. and Skellam, J.G. (1954). A New Method for Determining the Type of Distribution of Plant Individuals. Ann. Bot., N.S., XVIII, 213.

Kendall, D.G. (1948). On Some Modes of Population Growth Leading to R.A. Fisher's Logarithmic Series Distribution. Biometrika, XXXV, 6.

Levy, E.B. and Madden, E.A. (1933). The Point Method of Pasture Analysis. N.Z. J. Agric., XLVI, 267.

Moore, P.G. (1953). A Test for Non-randomness in Plant Populations. Ann. Bot., N.S., XVII, 57.

Murray, J.M. (1946). A Guide to Botanical Techniques for Pasture Research Workers. Government Printer, Pretoria.

Neyman, J. (1939). On a New Class of Contagious Distribution Applicable in Entomology and Bacteriology. Ann. math. Statist., X, 35.

Numata, M. (1954). Some Special Aspects of the Structural Analysis of Plant Communities. J. Coll. Arts Sci., Chiba Univ., 1, 194.

Parker, K.W. and Savage, D.A. (1944). Reliability of the Line Interception Method in Measuring Vegetation on the Southern Great Plains. J. Amer. Soc. agron., XXXVI, 97.

Pearson, K. Tables for Statisticians and Biometricians. Part II. Cambridge University Press (p. cxxxx iii).

Pereira, H.C. and Beckley, V.R.S. (1952). Grass Establishment on Eroded Soil in a Semi-arid African Reserve. Emp. J. Exp. Agric., XXI, 1.

Penfound, W.T. (1945). A Study of Phytosociological Relationships by Means of Aggregations of Coloured Cards. Ecology, XXVI, 38.

Pound, R. and Clements, F.E. (1898). A Method of Determining the Abundance of Secondary Species. Minn. bot. Stud., II, 19.

Quenouille, M.H. (1949). A Relation Between the Logarithmic, Poisson, and Negative Binomial Series. Biometrics, V, 162.

Raunkiaer, C. (1909). Formationsundersøgelse og Formationsstatistik. Bot. Tidsskr., XXX, 20. (English translation in "Life Forms of Plants and Statistical Plant Geography" (1934) Chap. VI).

Steiger, T.L. (1930). Structure of Prairie Vegetation. Ecology, XI, 170.

Stewart, G. and Keller, W. (1936). A Correlation Method for Ecology as Exemplified by Studies of Native Desert Vegetation. Ecology, XVII, 500.

Thomas. M. (1949). A Generalisation of Poisson's Binomial Limit for Use in Ecology. Biometrika, XXXVI, 18.

Troup, L.G. (1953). Inquiry into the General Economy of Farming in the Kenya Highlands. Government Printer, Nairobi.

West, O. (1937). Botanical Analysis of Pasture. S.A. J. Sci., XXXIII, 501.

4. In Part III

Baker, G.A. and Guilbert, H.R. (1942). Non-Randomness of Variations in Daily Weights of Cattle. J. Animal Sci., I, 293.

Baker, A.L., Phillips, R.W. and Black, W.H. (1947). The Relative Accuracy of One-Day and Three-Day Weaning Weights of Calves. J. Animal Sci., VI, 56.

Bean, H.W. (1946). Single Weight versus a Three-Day Average Weight for Swine. J. Animal Sci., V, 30.

——— (1948). Single Weight versus a Three-Day Average for Sheep. J. Animal Sci., VII, 50.

Boussingault, J.B. (1845). "Rural Economy in its Relation with Chemistry, Physics and Meteorology". 2nd Ed. London. Baillière.

Boyd, D.A. (1949). Experiments with Leys and Permanent Grass. J. Brit. Grassl. Soc., IV, 1.

Brandt, P.M. and Ewalt, H.P. (1939). Pasture Yields as Measured by Clip Plots and by Grazing Dairy Cows. J. Dairy Sci., XXII, 451.

Brown, B.A. (1937). Technic in Pasture Research. J. Amer. Soc. Agron., XXIX, 468.

Brown, D. (1954). Methods of Surveying and Measuring Vegetation. C.A.B. Bull. 42.

Cowlishaw, S.J. (1951). The Effect of Sampling Cages on the Yield of Herbage. J. Brit. Grassl. Soc., VI, 179.

Davies, R.O., Milton, W.E.J. and Lloyd, J.R. (1950). Pasture Productivity in Mid-Wales. Part I. Emp. J. exp. Agric., XVIII, 203.

Davies, R.O., Milton, W.E.J. and Lloyd, J.R. (1950). Pasture Productivity in Mid-Wales. Part II. Emp. J. exp. Agric., XVIII, 264.

Dunlop, G. (1933). Methods of Experimentation in Animal Production. J. agric. Sci., XXIII, 580.

Fisher, R.A. (1925). "Statistical Methods for Research Workers". Oliver and Boyd, Edinburgh and London.

Forbes, E.B. (1936). Procedure in Pasture Research. Proc. Amer. Soc. Anim. Prod., XXIX, 32.

Hodgson, R.E., Knott, J.C., Miller, V.L. and Wolberg, F.B. (1942). Measuring the Yield of Nutrients in Experimental Pastures. Agric. Exp. Stn. Wash., Bull. 411.

Hughes, G.P. and Harker, K.W. (1950). The Technique of Weighing Bullocks on Summer Grass. J. agric. Sci., XL, 403.

Jones, Ll. I. (1932). Variations in the Weight of Sheep: A Note as to Procedure. Welsh Plant Breeding Stn., Bull. H.13, 93.

Klingman, D.L., Miles, S.R. and Mott, G.O. (1943). The Cage Method for Determining Consumption and Yield of Pasture Herbage. J. Amer. Soc. agron., XXXV, 739.

Linehan, P.A. and Lowe, J. (1946). The Output of Pasture and its Measurement. J. Brit. Grassl. Soc., I, 7.

——— and Stewart, R.H. (1947). The Output of Pasture and its Measurement. Part II. J. Brit. Grassl. Soc., II, 145.

——— (1952). The Output of Pasture and its Measurement. Part III. J. Brit. Grassl. Soc., VII, 73.

Lush, J.L. and Black, W.H. (1927). How Much Accuracy is Gained by Weighing Cattle Three Days instead of One at the Beginning and End of Feeding Experiments. Amer. Soc. Animal Prod., Proc. 1925/26, 206.

Lush, J.L., Christensen, F.W., Wilson, C.V. and Black, W.H. (1928). The Accuracy of Cattle Weights. J. agric. Res., XXXVI, 551.

Morrison, F.B. (1947). "Feeds and Feeding". 20th Ed. New York. Morrison.

Patterson, R.E. (1947). The Comparative Efficiency of Single versus Three-day Weights of Steers. J. Animal Sci., VI, 257.

Report by Joint Committee of the Amer. Soc. agron., the Amer. Dairy Sci. Assoc., the Amer. Soc. Animal Prod., and the Amer. Soc. Range Management. (1952). Agron. J., XLIV, 39.

Robinson, R.R., Pierre, W.H. and Ackerman, R.A. (1957). A Comparison of Grazing and Clipping for Determining the response of Permanent Pastures to Fertilisation. J. Amer. Soc. agron., XXIX, 349.

Schalk, A.F. and Amadon, R.S. (1928). Bull. 216, N. Dakota Agric. Exp. Stn.

Wood, T.B. and Stratton, F.J.M. (1910). The Interpretation of Experimental Results. J. agric. Sci., III, 417.

Woodman, H.E. (1952). Rations for Livestock. Min. Agric. Fish., Bull. 48.

Yates, F. (1934). A Complex Pig-Feeding Experiment. J. agric. Sci., XXIV, 511.

5. Literature not Cited

5.1 Uniformity Trials and Size of Plot

Allard, R.W. (1952). The Precision of Lattice Designs with a Small Number of Entries in Lima Bean Yield Trials. Agron. J., XLIV, 200.

Bose, R.D. (1935). Some Soil Heterogeneity Trials at Pusa and the Size and Shape of Experimental Plots. Indian J. agric. Sci., V, 579.

Bose, S.S., Ganguli, P.M. and Mahalanobis, P.C. (1936). The Frequency Distribution of Plot Yields and the Optimum Size of Plots in a Uniformity Trial with Rice in Assam. Indian J. agric. Sci., VI, 1107.

Elliot, F.C., Darroch, J.G. and Wang, H.L. (1952). Uniformity Trials with Spring Wheat. Agron. J., XLIV, 524.

Frey, K.J. (1953). The Effect of Cost Functions on Number of Replications and Samples per Plot in Field Plot Experiments with Oats. Agron. J., XLV, 265.

Frey, K.J. and Baten, W.D. (1953). Optimum Plot Size for Oat Yield Tests. Agron. J., XLV, 502.

Garber, R.J., McIlvaine, T.C. and Hoover, M.M. (1931). A Method of Laying Out Experimental Plots. J. Amer. Soc. agron., XXIII, 286.

Harris, J.A. and Scofield, C.S. (1921). Permanence of Differences in the Plots of an Experimental Field. J. agric. Res., XX, 335.

Johnson, I.J. and Murphy, H.C. (1943). Lattice and Lattice Square Designs with Oat Uniformity Data and in Variety Trials. J. Amer. Soc. agron., XXXV, 291.

Keller, K.R. (1949). Uniformity Trial on Hops, *Humulus lupulus*, for Increasing the Precision of Field Experiments. Agron. J., XLI, 389.

Koch, E.J. and Rigney, J.A. (1951). A Method of Estimating Optimum Plot Size from Experimental Data. Agron. J., XLIII, 17.

Ma, R.H. and Harrington, J.B. (1948). The Standard Errors of Different Designs of Field Experiments at the University of Saskatchewan. Sci. Agric., XXVIII, 461.

Odland, T.E. and Garber, R.J. (1928). Size of Plot and Number of Replications in Field Experiments with Soybeans. J. Amer. Soc. agron., XX, 93.

Sayer, W., Vaidyanathan, M. and Iyer, S.S. (1936). Ideal Size and Shape of Sugarcane Experimental Plots based upon Tonnage Experiments with Co 205 and Co 213 Conducted in Pusa. Indian J. agric. Sci., VI, 684.

Siao, F. (1935). Uniformity Trials with Cotton. J. Amer. Soc. agron., XXVII, 974.

Swanson, A.F. (1930). Variability of Grain Sorghum Yields as Influenced by Size Shape and Number of Plots. J. Amer. Soc. agron., XXII, 833.

Torrie, J.H., Shands, H.L. and Leith, B.D. (1943). Efficiency Studies of Types of Design with Small Grain Yield Trials. J. Amer. Soc. agron., XXXV, 645.

5.2 Botanical Analysis

Armstrong, S.F. (1907). The Botanical and Chemical Composition of the Herbage of Pastures and Meadows. J. agric. Sci., II, 283.

Charpentier, C.A.G. and Saarela, O. (1943). The Point Quadrat Method of Levy and its use in the Investigation of Vegetation on Pastures. Herb. Abst., XIII, Abs. No. 908.

Crocker, R.L. and Tiver, N.S. (1948). Survey Methods in Grassland Ecology. J. Brit. Grassl. Soc., III, 1.

Eisele, H.F. and Aikman, J.M. (1933). The Development and Survival of Species and Varieties in Planted Pastures. Ecology, XIV, 123.

Hanson, H.C. (1934). A Comparison of the Methods of Botanical Analysis of the Native Prairie in Western North Dakota. J. agric. Res., XLIX, 815.

Hanson, H.C. and Love, L.D. (1930). Comparison of Methods of Quadratting. Ecology, XI, 734.

——— (1930). Size of List Quadrat for use in Determining Effects of Different Systems of Grazing upon Agropyron Smithii Mixed Prairie. J. agric. Res., XLI, 549.

Hasel, A.A. (1941). Sampling Error in Range Surveys of Sagebrush - Grass Vegetation. J. For., XXXIX, 52.

Lynch, D.W. and Shumacher, F.X., (1941). Concerning the Dispersion of Natural Regeneration. J. For., XXXIX, 49.

Murray, S.M., and Glover, P. (1935). Some Practical Points Regarding the Detailed Botanical Analysis of Grass-Veld or Other Pastures by the List Quadrat Method. Ecology, XXIII, 536.

Myers, E. and Chapman, V.J. (1953). Statistical Analysis Applied to a Vegetation Type in New Zealand. Ecology, XXXIV, 175.

Nichols, G.E. (1930). Methods in Floristic Study of Vegetation. Ecology, XI, 127.

Pearsall, W.H. (1924). The Statistical Analysis of Vegetation: A Criticism of the Concepts and Methods of the Upsala School. J. Ecol., XII, 135.

Pechanec, J.F. (1941). Sampling Error in Range Surveys of Sagebrush - Grass Vegetation. J. For., XXXIX, 52.

Romell, L.G. (1930). Comments on Raunkiaer's and Similar Methods of Vegetation Analysis and the "Law of Frequency". Ecology, XI, 589.

Shoemaker, H. (1935). Testing Raunkiaer's Law of Frequency Against Distribution of Fishes in the Streams of Wayne County, Indiana. Ecology, XVI, 568.

Tinney, F.W., Aamodt, O.S. and Ahlgren, H.L. (1937). Preliminary Report on a Study on Methods used in Botanical Analysis of Pasture Swards. J. Amer. Soc. agron., XXIX, 835.

Williams, C.B. (1954). The Statistical Outlook in Relation to Ecology. J. Ecol., XLII, 1.

5.3 Clipping and Grazing Methods of Estimating Pasture Production

Aldous, A.E. (1930). Effect of Different Clipping Treatments on the Yield and Vigor of Prairie Grass Vegetation. Ecology, XI, 752.

Albertson, F.W., Riegel, A. and Launchbaugh, J.L. (1953). Effects of Different Intensities of Clipping on Short Grasses in West-Central Kansas. Ecology, XXXIV, 1.

Bartlett, M.S. and Greenhill, A.W. (1936). The Relative Importance of Plot Variation and of Field and Laboratory Sampling in Small Plot Pasture Productivity Experiments. J. agric. Sci., XXVI, 258.

Culley, M.J., Campbell, R.S. and Canfield, R.H. (1933). Values and Limitations of Clipped Quadrats. Ecology, XIV, 35.

Green, J.O. (1949). Herbage Sampling Errors and Grazing Trials. J. Brit. Grassl. Soc., IV, 11.

Groenewald, J.W., Myburgh, S.J., Lawrence, G.B. and Louw, J.G. (1950). Digestibility of Lucerne Hay with Special Reference to Experimental Technique in Digestion Trials. Onderstepoort J. Vet. Sci. Anim. Industr., XXIV, 67.

Hodgson, R.E., Grunder, M.S., Knott, J.C. and Ellington, E.V. (1934). A Comparison of Rotational and Continuous Grazing of Pastures in Western Washington. Agric. Exp. Stn., Wash., Bull. 294.

Jones, Ll.I. (1937). The Comparative Value of Pastures as Measured by the Grazing Animal. Fourth International Grassland Congress, p. 386.

Knott, J.C., Hodgson, R.E. and Ellington, E.V. (1934). Methods of Measuring Pasture Yields with Dairy Cattle. Agric. Exp. Stn. Wash., Bull. 295.

Louw, J.G. (1944). The Nutritive Value of South African Feeding Stuffs: II. Onderstepoort J. Vet. Sci. Anim. Industr., IX, 85.

Marais, J.S.C. and Smuts, D.B. (1939). The Effect of Supplementing Lucerne with Cystine and Methionine on the Growth of Rats. Onderstepoort J. Vet. Sci. Anim. Industr., XII, 369.

Nevens, W.B. (1945). A Comparison of Sampling Procedures in Making Pasture Yield Determinations. J. Dairy Sci., XXVIII, 171.

Rhoad, A.O. and Carr, R.B. (1945). Measuring Productive Capacity of Pastures through Maintenance Studies with Mature Steers. U.S.D.A., Tech. Bull. 890.

du Toit, P.J., Louw, J.G. and Malan, A.I. (1935). A Study of the Mineral Content and Feeding Value of Natural Pastures in the Union of South Africa, IV. Onderstepoort J. Vet. Sci. Anim. Industr., V, 215.

Wagner, R.E., Hein, M.A., Shepherd, J.B. and Ely, R.E. (1950). A Comparison of Cage and Mower Strip with Grazing Results in Determining Production of Dairy Pastures. J. Amer. Soc. agron., XLII, 487.

Williams, T.E. (1947). The Animal and Pasture Evaluation. J. Brit. Grassl. Soc., II, 207.

——— (1949). Evaluation of British Grassland. J. Brit. Grassl. Soc., IV, 17.