INTERPRETATION AND EVALUATION OF THE RESULTS OF ANIMAL HUSBANDRY AND VETERINARY EXPERIMENTS

The object of this chapter is to present an outline of the manner in which statistics may be used to increase the efficiency of animal husbandry and veterinary experiments and to aid the research worker in making his interpretations of the results obtained. It is based on a paper prepared by Dr. H.O. Hetzer, in connection with the Buenos Aires meeting.

Increasingly successful use is being made of the science of statistics in the various biological sciences, and the established statistical methods can play an important part in solving many of the problems faced by the livestock industry, whether the problems relate to breeding, feeding, pasture or disease control investigations. Many animal husbandmen and veterinarians have not had detailed training in statistics. However, with a general understanding of the principles outlined below and with the assistance of mathematicians, they can readily apply the simpler techniques in their research work. Such applications are already being made in many institutions, and the importance of more attention to the application of statistics was recognized in the Baurú meeting.

Fisher (1930) defines statistics as a branch of mathematics applied to observational data, statistics being concerned primarily with (1) the study of populations, (2) the study of variation, and (3) the study of methods of the reduction of data. Snedecor (1948) says:

> Statistics have been called the technology of the scientific method. In the sequence constituting that method — hypothesis, experiment, test of hypothesis — the final stage is statistical... Unless the hypothesis is precisely stated, and unless the experiment produces unambiguous information about it, the test is futile and conclusions are unclear.

Variability is one of the common characteristics of all living matter. Even when dealing with individuals raised under similar environmental conditions, or with individuals who are identical in their inheritance, such as identical twins in cattle, for example, no two such individuals are ever exactly alike. As Cochran and Cox (1950) point out, it is this sort of variation which introduces a degree of uncertainty into any conclusions that are drawn from experimental results. The implication here is that results obtained under one set of conditions are never duplicated exactly under another set of conditions or, if obtained under the same conditions, the results from trial to trial may be so different at times as to leave their reliability in doubt. The development of methods for evaluating this element of uncertainty and applying statistical tests for the accurate summarization and interpretation of experimental data, constitutes one of the principal functions of statistics, as applied to the various biological sciences. Another field in which statistics have been increasingly helpful to the research worker in the plant and animal sciences is in the planning and designing of efficient experiments. Statistics may thus be considered to aid research in two important ways. One of these concerns the design of experiments and the principles which must be observed in order to permit the drawing of valid conclusions. Space will not permite mention of more than a few of the large number of statistical methods currently being used by research workers, but some discussion is included of the rules or steps that must be considered in planning experiments. Examples illustrating some of the experimental designs which appear to be particularly applicable to problems in livestock research are noted later.

Planning of Experiments

Consideration should first be given to the essential steps which are basic to successful experimentation. Cox (1951) once remarked that an ideal situation would be for every experimenter to know his science and the science of statistics. A similar view was expressed by Snedecor (1950). In discussing the steps involved in planning an experiment, Snedecor pictures the situation as follows:

> The experimenter specifies the conditions in which the trial (or experiment) is to be performed — materials and treatments, together with genetic and environmental circumstances — and the measumerements that can be made. The statistician selects or invents a plan (experimental design) which will furnish unbiased and unconfounded estimates with adequate precision. The experimenter conducts the laboratory or field work, taking pains to eliminate as nearly as possible all extraneous effects. If he is successful, the ensuing measurements will contain the information for which the experiment is set up. The statistician uses appropriate methods for extracting all the information brought into the data. Finally, the experimenter interprets this information in the light of existing knowledge in his science.

It should be noted that Snedecor places considerable emphasis on the selection of an experimental design which, as he expresses it, " will furnish unbiased and unconfounded estimates with adequate precision. " In other words, the experiment should be designed so as to yield results which are both accurate and free of extraneous effects, as well as precise in that they must be repeatable under similar conditions. There is a very important reason for this which should be kept in mind at all times. The point is that whatever statistical methods are used, they are only tools. To use these tools successfully, or to extract the desired information from an experiment requires that the experiment be so designed as to yield results which provide reliable information on the points at issue; i. e. regardless of how refined or how elaborate the statistical methods employed, they are powerless if applied to inadequate data or to data from faulty experimental designs.

Consideration should be given to the individual rules or steps that should be observed in planning experiments. They are (1) a statement of the objective, (2) a description of the experiment, and (3) an outline of the statistical analysis of the results. There are, of course, other things that should be considered before starting an experiment, such as applicability of the results to the solution of a particular practical problem (i.e., economic justification), approximate total cost and availability of the necessary personnel and materials, including animals, feeds, housing, pastures, instruments, tools, etc. While all of these latter items are important, it is assumed here that they have been resolved before the experiment actually gets under way.

The first step in planning an experiment is to state clearly the purpose of the work. The principal objective may be in the form of a question to be answered, a hypothesis to be tested, a relationship to be measured, or treatment effects to be estimated.

To illustrate the importance of sound planning, a statement by Cox (1951) may be quoted as an example of faulty experimentation. The results were brought to a statistician with a question about testing the effect of different protein supplements on rate of growth in chickens. That is, the investigator wanted to know if the treatment differences were statistically significant, or, stated in more practical language, if the differences he attributed to treatments were real in the sense that they could not reasonably be attributed to the vagaries of mere chance. The experimental material being chickens, and knowing that males grow at a faster rate, on the average, than females, and that quantity, as well as quality, of protein may affect rate of growth, there should be no difficulty in recognizing the fallacies of the experiment described below.

The layout of the experiment, as described by Cox (1951) was as follows:

> Six kinds of protein supplements were fed to young chicks to estimate their relative effects on gain in weight. All the chicks receiving treatment A were kept in Pen A, and similarly chicks receiving each of the other five protein supplements were kept in separate enclosures. The sexes were mixed in unknown ratios, and the supplements were used in equal weights irrespective of protein content. No record was kept of individual food consumption. Based on the statistical analysis, if the lot having Treatment A gained significantly more than the lot having Treatment B, the answer might be (a) that the concentration of protein in A was greater than in B, or (*b*) that there was a larger proportion of males in Lot A than in Lot B, or (*c*) the environmental conditions were more favorable in Pen A, or (*d*) supplement A was more appetizing than B. This man came to the statistician to ask about the merits of various tests of significance, and he was seemingly unaware that the differences he was testing could not be identified and, therefore, were meaningless.

The reason, of course, is not that the man failed to state his problem correctly, but that he did not plan his experiment or select his materials in such a way as to exclude or properly account for the variability caused by differences in the amounts of protein eaten, the general sex differences in rate of growth, and environmental differences due to location.

The second step which should be considered in the planning of an experiment is, as previously mentioned, a description of the the experiment. In other words, there should be an outline of the plan of work, including a statement of the specific treatments or methods to be tested, number and kinds of experimental animals to be used, size and kind of housing, pastures or paddocks, variables or performance characteristics to be studied (body weight, milk and fat production, etc.), duration of the experiment and, finally, but no less important, an outline of the experimental design including a statement of the methods to be used in selecting and allotting the experimental units and treatment. It should be emphasized that a description of the experiment covering the various points listed above is perhaps the best insurance against failing to reach the particular objectives of an experiment. Needless to say, the experiment should be so designed and conducted as to provide the desired information in the shortest possible time, and at a minimum cost in labor and equipment. Also, the experimental design should be as simple as is consistent with the requirements which must be met to arrive at accurate answers.

Regarding the selection of the treatments, it is, of course, necessary for the investigator to determine how they provide information on the point at issue. The term 'treatment' as commonly used by professional statisticians, may cover a variety of operations, such as determining the effects of continuous *versus* rotational grazing in a pasture experiment or of self-feeding *versus* controlled feeding in a swine feeding trial, determining the nutritive value of different protein concentrates, evaluating the protective power of various biological products to a particular disease, estimating the response of different breeds to a particular set of climatic conditions, or testing the effects of a combination of different factors in the same experiment. Investigations of the latter type call for the use of factorial experiments. An important feature of factorial experiments in that the effects of two or more factors may be studied simultaneously with the same precision as where the effects of only one factor are studied. Another feature of factorial experiments is that, in addition to providing estimates of the main effects, they provide information on interactions among treatments, which is impossible in single factor studies. A factorial experiment might, for example, involve the testing of various intensities of grazing in combination with the application of various concentrations of a given fertilizer.

Once the objectives and plan of work have been stated, it is next desirable to indicate in outline form the method of analysis that will be used in processing and summarizing the experimental results. This means that the experimenter should have sufficient knowledge of statistics as to enable him to select the methods appropriate for testing a particular hypothesis, demonstrating inter-relationships and drawing inferences as to the generalization and applicability of the experimental results. There are various textbooks which illustrate the statistical analysis appropriate for various experimental design, some of which are cited at the end of this chapter.

Methods for Increasing Accuracy of Experiments

Cochran and Cox (1950) distinguish two main sources of experimental errors as follows: " The first is inherent in the experimental material to which the treatments are applied... The second source of variability is lack of uniformity in the physical conduct of the experiment, or, in other words, failure to standardize experimental techniques. " Since either of these two sources for errors may introduce considerable uncertainty into both the accuracy and the precision of the results, it is highly desirable that the investigator incorporate into his experiment any method or combination of methods that will increase its precision. The basic methods, as suggested by Cochran and Cox (1950) are (1) increasing the size of the experiment, (2) refining the experimental technique, and (3) handling the experimental material so that the effects of variability are reduced.

Size of Experiment

The usual method of increasing the size of an experiment is to include a larger number of replications. This has the effect of decreasing the error associated with the treatment provided that the experimental units are allotted entirely at random to the treatments. The reason for this is to ensure that one treatment is no more likely to be favored in any replicate than another.

An idea of how the number of replications affects the probability of detecting a real difference between the average effects of two treatments, or, in other words, how many replications are necessary in order that a difference of a given size is likely to be detected as significant, can be obtained from tables as suggested by Cochran and Cox (1950). Inspection of these tables shows that, in general, there is very little guarantee of detecting differences of 10 percent or smaller with two replications. It is also apparent that the larger the error that affects the observation for the individual unit, the larger number of replicates required for detecting a given size difference at a given probability. The larger the true difference in the average effect of two treatments, the smaller, in turn, is the number of replicates generally required for a given probability of obtaining significant results. Thus, if we postulate the true difference to have a certain value, and if we have some idea of the experimental error of our observations, we can, within certain prescribed limits, ensure the most efficient size for the experiment by estimating the required number of replications, as suggested by Cochran and Cox (1950). It should be emphasized in this connection that the values given by Cochran and Cox for the number of replications required for obtaining a significant result are estimates based on statistical theory. Consequently, the number of replicates required in actual practice may vary somewhat depending partly on the precision of the estimate of the experimental error, and partly on the magnitude of the true difference.

With regard to the subject of replications, attention should be called to a paper by Lucas (1950), in which he outlines the steps in determining the size of paddock and the number of animals per paddock necessary in studies on pasture and grazing. Based on expressions involving variances, cost factors and carrying capacity, Lucas has developed formulae which make it possible to arrive at the optimum number of animals per paddock, and the optimum number of replications for a number of different conditions, such as where the variance, cost, total number of animals, or total number of paddocks, are specified. On applying estimates for the various factors represented in the equations, Lucas finds the optimum number of animals per paddock to be about 7 for nutritive value studies, about 3 for yield studies with other animals such as non-milking dairy cattle or beef cattle. This number is considerably lower than those customarily used in grazing work. As Lucas points out, the common situation has been to carry as many as 10 to 20 animals per paddock, and to use but one paddock per treatment.

As regards the number of replications or paddocks per treatment, Lucas states that there is little chance of detecting differences of 25 percent or less where only 2 replications are used. He estimates that, as a general rule, at least 4 or 5 replications are necessary if the true difference between two treatments is 20 percent or more. It appears that the number of replications required would be twice as large, at least, if the real difference were 10 percent or less.

Refining Experimental Techniques

Another method of increasing the accuracy of experiments consists in refining the experimental technique. Faulty or inaccurate scales may result in weight records that are continually biased, or increase the experimental errors to a point where they may actually mask the difference between treatments. To avoid biases such as those introduced by faulty equipment, or by improper handling of the experimental material such as would result if a particular treatment were continually favored in successive replications by some extraneous source of variation, it is essential that the principle of randomization be carefully considered in planning the layout of an experiment.

As was first indicated by Fisher (1947), both replication and randomization are necessary to obtain valid estimates of experimental error. While various restrictions may be imposed on the randomization, one occasion where there is need for randomization is whether treatments are allotted to the experimental material. In other words, care should be taken that each experimental unit included in the trial has an equal chance of being subjected to the different treatments. Sometimes, the experimenter may find that there is need for the application of randomization to other operations as, for example, in experiments in which the equipment introduces variation. It should be realized, however, that the need for randomization does not dispense with the use of systematic design for, as we shall see a little later on, many of of the experimental designs being used today involve several restrictions on the randomization, which is indicated by a knowledge of potential sources of bias.

As was indicated above, the principal objective in refining the experimental technique is to prevent errors such as those caused by faulty equipment. Mention was also made of randomization as the device commonly used to provide unbiased measures of the experimental error, as well as of the treatment effects. Other refinements may involve the development of more accurate methods of measurement, more adequate control over external environmental influences such as those due to changes in seasonal or climatic factors, or greater care in the selection of the experimental materials and treatments, so as to ensure that the experimental animals, pastures, soil types, etc., are a representative sample of the population about which inferences or generalizations are to be drawn. While many examples could be given to illustrate the above points, two examples are cited which are of particular interest because of their potential value in improving the quality of livestock research. One of these pertains to the improved methodology which is now being developed in estimating grazing capacity of ranges, as discussed by Stoddart (1952), and the other concerns the increase in the efficiency of feeding experiments with dairy cows, by use of the method of equalized feeding, as reported by Lucas (1943). There appear to be no published data as yet regarding the relative increase in accuracy that might result in estimating stock carrying capacity in terms of digestible nutrients, for example, but Lucas finds that to demonstrate a given difference between treatments in dairy cattle as statistically significant it would require only about one-fourth as many animals when using equalized feeding as when using the ordinary method. This is an excellent example of how gains in precision, obtained by refinements of technique, may substantially reduce the cost of an experiment.

Another method by which precision may be increased is by eliminating or controlling the variation due to tangible factors over which the experimenter has no immediate control. In certain experiments, it may be possible to obtain measurements on variables which are known to affect the performance of the experimental unit. In an experiment designed to determine the effects of different protein supplements on the rate of growth of pigs, for example, their initial weights may affect their subsequent performance. As it may not be possible or deisrable to equalize the initial weight of the pigs on the different treatments, adjustment of the observed increases in weight by the technique known as the analysis of co-variance will largely eliminate the effect of this variable from the estimates of the treatment effects. A similar approach involving the analysis of co-variance technique would be appropriate in studies on milk production in cattle, for example, where factors such as age of cow, initial production rate or stage of gestation, contribute to the variation of milk yield.

Proper Handling of the Experimental Material

This is the third important method by means of which the experimental error may be reduced. The approach is essentially that of choosing or inventing an experimental design which will provide the maximum amount of information per unit of cost. Since there may be limitations on funds and facilities, the choice of a particular design may be more or less dictated by prevailing conditions. It is impossible, therefore, to single out any one design as most efficient. Also, the relative efficiency of a given design will depend on the uniformity of the experimental material, as well as the number and relative importance of extraneous sources of variation. Before deciding on a particular design, it will usually be desirable, therefore, to secure as much information as possible on all potential sources of variation so that provisions be made for their control in planning the final layout. Frequently, it is on the basis of such information that a particular design is finally selected.

An example of the type of exploratory work which may help in selecting a design is the study on " Factors Affecting Rate of Gain and their Relation to Allotment of Pigs for Feeding Trials, " by Miranda, Culbertson and Lush (1946). These workers reported that 21 percent of the total variance in their data was associated with differences between breeds. Litter differences accounted for 29 percent of the total variance, or 37 percent of the variance within breeds, while 9 percent of the variance within litters was due to the difference between sexes. The intra-litter correlation between initial weight and gain was 0.24. Based on the above findings, Miranda *et al.* concluded that, in-so-far as rate of gain is concerned, breed and litter might well be considered in allotment but that the effects of sex and initial weight were too small to be of practical importance or to need correction. It would seem, therefore, that a feeding experiment with pigs might be improved by allotting an equal number of pigs from each litter to each of the treatments, thereby balancing the treatment groups with regard to litter. By allotting the treatments on this basis, other factors, such as breed, age and pre-trial environment are automatically balanced.

Experimental Designs

Only a few of the experimental designs from which one may choose are discussed here, with emphasis on those which are generally considered to be most useful in livestock research. Treatises as those by Cochran and Cox (1950), Goulden (1952), Lucas (1948) and others have been drawn upon for this purpose.

As Lucas points out, animal science experiments of the feeding and nutritional types may be considered as falling into either of of two classes, i. e. " continuous trials " and " change-over trials. " In the continous type of trial, an animal is subjected to a single treatment throughout the duration of the experiment, while in the change-over type, an animal receives in sequence two or more treatments. It is easy to visualize situations where both types of experiments might have a place in management studies, pasture and grazing studies or disease and parasite control studies. It is not possible here to go into the advantages and disadvantages of the two types of trials, except to point out that by using change-over trials, the number of observations per treatment and/or the number of treatments can be increased, without having to increase the number of animals.

Complete Block Designs

Within both types of trials as mentioned above, a number of different designs may be employed. To avoid confusion, it is desirable to discuss these, using the names by which they are decribed in most statistical textbooks. Classified broadly, experimental designs fall into one of two classes, (1) complete block designs, or (2) incomplete block designs. Applied to animal experimentation, the term " block " generally denotes a group of animals similar with respect to one or more factors, either inherent to the animals, or associated with the environment (Lucas, 1948). Complete block designs are characterized by the fact that each block or replicate contains a complete set of treatments, whereas in incomplete block designs the number of treatments is larger than the number of units per block.

In a completely randomized design, which is the simplest type, the animals are allotted to the treatments completely at random. This type of design has certain advantages in that any number of treatments and replicates may be used. The principal disadvantage is that there is no attempt to reduce the experimental errors, as might be the case if randomization were restricted so that all animals receiving a given treatment were similar in every major respect to those receiving another treatment. In order to take care of the situation, some investigators have followed the practice of allotting the animals so as to “balance” the effects as factors such as breed, age, sex, condition, initial weight, etc., among the treatment groups. This procedure has been criticized on the grounds that the variation within lots is made larger and that between lots is made smaller than it would be if the animals were allotted entirely at random. It is maintained, therefore, that the variation within lots is no longer valid for testing the significance of the treatment effects.

Randomized block designs differ from completely randomized or ungrouped designs in that the animals are first divided into groups coinciding with some major source of variation. Sub-division is made so that the number of animals in a group is equal to the number, or a multiple of the number, of treatments. The animals within each group are then allotted to each treatment at random. Usually several such groups are needed to obtain an estimate of experimental error. This is probably the most commonly used design in livestock research and many examples could be cited to illustrate its usefulness. In studies involving different breeds of cattle or sheep, e.g., animals belonging to the same breed would logically be considered as constituting a block. By applying the proper statistical methods, variation due to breed effects could thus be removed from the experimental error and thereby from the errors of treatment effects. Other applications of this type of design might be appropriate in situations in which insufficient numbers of animals at a given time or place make it necessary to conduct experiments over a period of intervals or at several places. In such cases, the periods or locations would constitute the blocks.

The latin square design is basically a randomized block design. In the latin square, however, the treatments are arranged in complete replications in two ways, the grouping being done so as to provide for control of two different sources of variation simultaneously. Another feature of the latin square is that the number of replications always equals the number of treatments. Thus, if four treatments were being compared the design would be as follows:

A B C D
B C D A
C D A B
D A B C

where the letters represent the treatments. It will be noted that each row and column contains a complete set of treatments, thereby fulfilling the requirements of double restriction. In an animal feeding experiment of the continuous variety, the rows may correspond to four different farms or four breeds, while the columns may represent four kinds of housing or pastures. With an arrangement of the treatments, as indicated above, possible sources of error due to variation between farms, breeds, housing or pastures, will automatically be eliminated from the estimates of treatment effects. In replicating the experiment, it is, of course, desirable to use a different arrangement of the treatments so as to minimize the danger of confounding. While there is a definite place for latin squares in livestock experiments, it should be noted that they are usually impractical when the number of treatments is large. The same is true if the number of treatments being combined is less than four, unless the plan provides for two or more replications.

The simplest change-over trials involving application of the latin square, are the so-called switch-over or reversal designs, in which two treatments, A and B, are compared in two sequences, as follows:

	Sequence *1*	 *2*
Period 1	A	B
Period 2	B	A

This design is basically a 2 x 2 latin square, with the provision for control of expected time trends in the animals' behavior. The plan usually involves a group of animals, one half of which is allotted at random to each of the two treatments. An extension of this design is the " double-reversal design " which, like the former, compares two treatments in two sequences, the only difference being that the double-reversal design is continued through 3 or more periods.

Experiments illustrating the use of the latin square design in change-over trials have been published by several workers, notably Cochran *et al.* (1941), Lucas (1943) and Patterson (1950). The particular design used by Lucas was a 4 x 4 square in which 4 rations were compared in a feeding experiment with dairy cows. The 12 cows available for study were first divided into three groups of four, on the basis of their producing abilities. Four sequences of treatments, each consisting of four five-week periods, were then allotted at random to the four cows of each group, with the provision that no two cows in a group would receive the same treatment during the same period.

By sud-dividing the cows into three 4 x 4 squares, and choosing the cows so as to have all cows within each group as similar as possible, Lucas was able to demonstrate a substantial reduction in experimental error, as compared with the error that would have been obtained had the animals been assigned to the treatment completely at random.

Another example illustrating the application of the latin square in change-over trials has been proposed by Lucas (1950) for grazing studies in which three different rates of feeding protein supplement constitute the treatments. The design is given as follows:

Sequence Set I			*Sequence Set II*		
1	2	3	1	2	3
2	3	1	3	1	2
3	1	2	2	3	1
1	2	3	1	2	3
2	3	1	3	1	2
etc. as needed			etc. as needed		

Where the rows represent periods, the columns represent paddocks, and the numbers represent the three rates of feeding protein supplements. The design calls for six groups of animals with two groups receiving each supplement during a given period, and the animals rotated from paddock to paddock within each sequence set.

Incomplete Block Designs

These designs are particularly adapted for situations where large numbers of treatments are to be tested as well as where the number of experimental units falling into a natural grouping, such as litters or breeds, for example, is not large enough to include all treatments. Thus, an incomplete block design may be defined as a design in which the number of experimental units in a block is smaller than the total number of treatments being compared. Statisticians speak of " balanced " and " partially balanced" incomplete block designs, but workers in animal science generally have made use only of the balanced type of designs. It should be noted in this connection that, while incomplete block designs do not provide as much accuracy between certain treatment comparisons as complete block designs, balanced incomplete block designs are characterized by the fact that all comparisons among pairs of treatments are made with equal precision. Another feature of balanced incomplete block designs is that, while there is no limit regarding the number of treatments or the number of units per block, the number of replications is fixed by these variables.

Using 4 treatments, A, B, C and D, in blocks of three experimental units each, a balanced incomplete block design would look as follows:

Block 1	A B C
Block 2	A B D
Block 3	A C D
Block 4	B C D

It will be seen that each pair of treatments occurs within a block the same number of times, filling the requirements of balance. However, in the above example, the blocks cannot be grouped in separate replications since four is not divisible by three, the number of units per block.

The usefulness of incomplete block designs is best illustrated by reference to an actual experiment. The example chosen is from a feeding trial with swine, described by Comstock *et al.* (1948). The particular design used was a 3 x 3 balance lattice design for 9 treatments in blocks of three litter mates. The design, using four replications, was as follows:

	Block 1	*Block 2*	*Block 3*
Replication 1	1 2 3	4 5 6	7 8 9
Replication 2	3 4 8	2 6 7	1 5 9
Replication 3	1 4 7	2 5 8	3 6 9
Replication 4	3 5 7	2 4 9	1 6 8

Note that the blocks are incomplete, since each contains only 3 of the 9 treatments being compared. Also note that each treatment occurs once in the same incomplete block or litter with each of the other treatments. By removing the variation due to litter differences, the gain in efficiency of the incomplete block design as used in this particular experiment, compared to randomized complete blocks designs, was about 20 percent. Other situations in which incomplete block designs may be found useful is where the number of animals at any one time or at any particular place is smaller than the number of treatments.

There are many questions about experimental designs which remain open for further discussion as well as for further study. What has been attempted here has been to survey the designs which appear to be most useful in animal science experimentation, and to point out how different designs are necessary to meet different experimental situations. It might be well also to re-emphasize that the efficiency of a particular design will vary with the nature of the experimental material, as well as with the conditions under which the experiment is conducted. Another important point to remember is that the experimental design should be as simple as is consistent with the requirements necessary to obtain reliable answers.

Analysis and Interpretation of Results

Having collected all the data necessary to obtain the information for which the experiment was conducted, the next step in carrying the experiment to its logical and successful conclusion is to select the appropriate statistical methods for summarizing and interpreting the results. As Cox (1951) points out, " the analysis of data calls for clear thinking and for careful selection of the statistical tools to be used. " Also, " ... the statistical analysis cannot increase the validity of the data. " This adds emphasis to the point stressed earlier, i. e., regardless of how refined or how intricate the statistical methods employed, they are powerless if applied to data lacking the necessary precision, or to data derived from faulty experimental designs. Assuming then that the experiment was properly planned and conducted, the next important step is that we use the proper statistical methods in order to extract all the pertinent information contained in the data. Proper use of these methods, such as those employed in the computation of averages, standard deviations, coefficients of variability, or *t*, the quantity commonly used in testing the significance of the difference between two averages, generally does not require more than a working knowledge of statistics. As a general rule, however, the parameters to be estimated and the relationships to be determined are more complex than the statistics just mentioned. In order to obtain the desired information from such data, it would be necessary for the investigator to know something about the scope and flexibility of the various statistical methods, including the basic assumptions underlying their use. Fortunately, there are several textbooks available on statistical methods as applied to experiments in the agricultural sciences. Anyone intent on improving the quality of his research accomplishments in the animal sciences can learn a good deal just by studying these books. In the great majority of cases, however, the most effective way of dealing with the situation would be for all agricultural institutions giving training at a graduate level to include in their curricula at least one course on experimental designs and statistical methods. In the meantime, everyone concerned with quantitative studies in livestock research should be encouraged to seek the advice of a qualified statistician before embarking on an experiment. Experience shows that a person taking advantage of the experimental designs and statistical methods that are available for his use not only has less of a problem analysing and interpreting his results, but also contributes towards reducing the cost of experimentation.

In addition to the material on statistical techniques summarized above, attention should be drawn to the importance of statistical analyses in pointing to new clues in the solution of problems. Use of the appropriate statistical methods not only provides a means of interpreting the results in terms of existing knowledge, but also may actually point to new and important clues regarding the solution of a particular problem. Also, if the experiment were properly designed and conducted, the results might serve as a basis for prediction which, after all, is the ultimate purpose of any experiment.

In other words, an experiment can be considered a success only if the results are such as to make it reasonably certain that similar results will be obtained under similar conditions at some future date. It should also be emphasized in this connection that it is important and desirable that the investigator report negative as well as positive results. By so doing, he not only helps to clarify knowledge regarding certain phenomena, but also serves the scientific cause by preventing duplication of effort on the part of other investigators who, with this information, could devote their energy to other more fruitful tasks.

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