THE FUTURE OF ORNITHOLOGY IN NEW ZEALAND

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Attitudes to Basic Theory

Eighty years ago when comparative anatomy and taxonomy were young and exciting disciplines, the study of birds in pursuit of these subjects was a most respectable academic activity. Ornithologists produced ideas that excited large sectors of the scientific world. Today the limelight is on such studies as primate behaviour, cell structure and the chemistry of genes, and bird studies are something of a biological backwater. Tinbergen however is optimistic. When discussing Lack's work on adaptations in seabirds he remarks, " I feel that this field of research may well become one of the real ' growing points ' of ornithology in which our science will once more make a contribution to Biology in general." (Tinbergen 1967:53.)

Though ornithological journals flourish and like all aspects of the paper war increase in fire power, it has seemed to me that ornithological research generally has been plodding along accumulating vast quantities of empirical data with few attempts at syntheses, little evaluation of ornithological data in the light of other biological advances, and in particular with an uncritical attitude to basic theory. There is little point in fussing over our lack of prominence at the moment but when a science is in a state of quiet progress with no great theoretical advances appearing, I think there is a tendency for basic theories to be taken for granted until they sink into the background of thought where they take on the character of unexamined preconceptions.

Why should we worry about basic theory at all ? Why not just get on with collecting ' the facts ' ? Studies of human behaviour are showing more and more conclusively that we see what we expect to see, that all perception is influenced by preconception ! Consequently it becomes imperative that we should be very aware and critical of those preconceptions, those basic assumptions which affect even the simplest observational work. But moderation in all things. To be aware and critical of an assumption does not imply rejection. We must use basic theories to interpret and order our data or else the results would be chaotic and we would be unable to communicate what we have seen and heard to other people.

Ornithological Theorists

Three men whose theoretical papers have been of considerable interest in the past decade are Dr. David Lack as a synthesizer, and Dr. Ernst Mayr and Dr. N. Tinbergen as men who each work between two disciplines. Dr. Mayr's work on avian systematics has contributed considerably to general concepts of evolution, and Dr. Tinbergen's work has added much to behavioural concepts. I would like to discuss two papers of Lack's and Tinbergen's which were read at the last International Ornithological Congress and which have just been published. They are " Interrelationships in breeding adaptations as shown by marine birds " by D. Lack, and " Adaptive features of the Black-headed Gull " by N. Tinbergen.

'Description of Undisturbed Nature' versus 'The Experiment'

In his paper Tinbergen makes a very nice point about the relationship between experimental methods and methods involving observation, description and simple measurement. He is commenting on Lack's procedure in his paper on marine birds, in which Lack sets certain properties of birds against other aspects of their ecology with a view to finding correlations between them. " These correlations are then interpreted in terms of function, or survival value. For instance nesting on cliffs or islands is correlated with the relative inaccessibility of such places to mammalian predators, and therefore interpreted as a defence against such predators. . . . . The net result of this procedure is a set of hypotheses of the following type. ' If this species did not possess this particular feature it would be less successful than it is.' To Tinbergen experiments should test such hypotheses, but the result of the experiment " amounts to no more (and no less) than finding out whether a deviation from the norm is penalized." (Tinbergen 1967:43). This seems a very limiting view of the usefulness of experimentation, but Tinbergen does concede that if the experiments are well done they will provide information about the environmental pressure which is doing the penalizing i.e. which predator, and how it is exerting pressure.

Considering the academic respectability that usually accrues to experimental studies in Biology generally, Tinbergen's comments on descriptive methods are interesting. " First it is the descriptive interpretive method which provides the ideas " and the experiment only confirms or causes us to reject the idea. The next step is to " return to the interpretive method for new inspiration. *The interpretive method requires a great deal of sound intuition and imaginativeness.*" (my italics). Secondly he emphasises that there are many observations whose effects are so obvious that they do not need experimental verification. It would be pedantic to test experimentally the assumption that when a gannet throws an intruder off its nest, this is an effective nest defense activity. Thirdly experimental evidence has to be interpreted in the wider context of the animal as a whole and the environment as a whole. (Tinbergen 1967:44).

Description and interpretation is virtually the only procedure used in New Zealand ornithology. Tinbergen does not place much emphasis on *the* big problem of using descriptive methods though he does mention it when discussing an experiment done by Kruijt on the eggs of Herring Gulls. This experiment showed that plain coloured eggs put under gulls suffered heavier predation than cryptically-coloured, spotted eggs, but Kruijt was also able to show that the gulls did not sit so tightly on plain coloured eggs and left them exposed to potential predation for longer periods than they did their ordinary spotted eggs. This was a disturbing variable which was discovered " but with this type of experiment one can never be sure that one knows or even suspects all possible variables." (Tinbergen 1967:46). This conclusion applies even more strongly to the undisturbed environment which is simply observed than it does to the above experiment where a natural situation was altered. But if we set up a totally artificial environment and control as many variables as we can, what do the results mean ? The more artificial we make the environment, the more difficult it becomes to interpret

the results. We are caught between two unsatisfactory extremes. Lack's version of a multivariate analysis in his paper on seabird adaptations suggests an additional method which though it has its pitfalls is producing interesting results in many fields of research. The undisturbed environment is observed in great detail and a large number of variables are considered in conjunction.

Breeding Adaptations in Marine Birds

In his paper to the Ornithological Congress Lack takes into consideration data from 270 species belonging to 20 families. Of these families 12 are exclusively marine, 3 mostly marine and 5 contain some marine species. He subdivides the species into:—

(a) intertidal feeders — oystercatchers, shore plovers.
(b) inshore feeders who obtain their food on or under the water in sight of land — most gulls.
(c) offshore feeders — albatrosses.

This division in feeding habits Lack considers has profound effects on such things as the sizes of breeding colonies, on incubation and fledgling periods, clutch size and the age at which the birds first breed. At the same time he notes a strong correlation between the behaviour of the young (whether they leave the nest on hatching or remain in the nest), whether or not eggs and young are cryptically coloured and the nesting dispersion (solitary or colonial); the major controlling factor being predation, with feeding habits having some effect also. This method of considering many factors at once and comparing them over a large number of species creates logical patterns in the data and occasionally shows apparently non-adaptive behaviour to be the result of some compromise. As an illustration of the latter point Lack notes that the earliest layers in *Puffinus puffinus* raise the most young. Why do not all pairs breed earlier? Shearwaters lay relatively large eggs and presumably each female lays as soon as she can obtain enough food to produce her egg. Large eggs produce large chicks which are easier to pass food to and keep warm, and Lack presumes that this advantage outweighs the advantages that would be gained from laying smaller eggs and hatching smaller and more vulnerable chicks at an earlier date when the cycle of food supply would be better geared to chick feeding.

The Future

What sort of empirical data is required to produce deductions such as these? Obviously the first thing required is a wide knowledge of a given species' activities, combined with an acute awareness of basic assumptions which may have become built into the data. Feeding studies, breeding studies, behavioural work and the general physiological requirements of the species being studied should be considered in toto, not forgetting Tinbergen's plea for a leaven of imaginativeness. Though Lack does not make any use of statistical analyses, his method is a multivariate analysis without using measurements. It seems an obvious step to collect metric data on such things as clutch size, incubation and fledgling periods, on distances travelled to feeding grounds and so on, and to submit this data to the various multivariate analyses of the statistician. Yet a metrical test, like an experiment, is generally used to test the validity of an idea which has sprung from some general observations. From this it appears that the most productive part of the whole process of

research is the initial mass of observations and interpretations of what the birds are doing. I am not decrying the use of metrical methods. Measuring, graphing and the use of statistical analyses are necessary but I would like to suggest a priority system. There is no need to reject a course of action or a body of data because it does not lend itself to precise description or measurement. It is desirable to aim at a means of measuring some variable that may be affecting an aspect of the research in hand, but if it is data such as the distance to the feeding ground of a marine bird which can be only roughly evaluated, this is no cause for despair or the rejection of the imprecise data that are available. There is also the danger of distorting data through being too precise. The human love of naming and classifying may result in the setting up of discrete categories when a better interpretation would be to allow the data to lie in a somewhat amorphous and imprecisely defined continuum.

At the same time there are numerous aspects of bird study where there is an obvious need for the collection of precise and comparable data from different species of birds so that valid comparisons can be made between species and across families. The work done in the North Island on the identification of feeding stations in bush birds and the use of this technique in the study of Saddlebacks is an excellent example of what can be done to standardize a technique and enable several observers to produce complex but unified data. (Atkinson, 1966:12-17). Field study courses allow ornithologists to discuss and, we hope, agree on optimum field methods (and to produce some of those ideas that they have no 'good' evidence for). In Otago we have produced a cyclostyled sheet for recording plumage variations in Pied and Black Stilts which will help give uniformity to observations by a large number of observers. A similar format could be designed for recording plumages of other variable species.

Conclusion

I would like to suggest that the following lines of attack would result in interesting and productive ornithological research in New Zealand.

1. Studies in depth of given species rather than regional studies,
2. The seeking of interrelationships between the various adaptive characteristics of groups of birds in similar habitats,
3. Even more concentration than at present on the collecting of uniform and comparable data,
4. More discussion of theoretical matters.

This paper has had its origin in three main sources; the International Ornithological Congress papers, the problems encountered in encouraging ornithological activity in Otago while I was O.S.N.Z. Regional Representative, and some discussions of problems in fieldwork in another discipline altogether, that of archaeology.

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