contract their diets as prey of high rank change in abundance. In early models, prey rank was assumed to be a function of only prey energy content and handling time, but more recent models have also considered nutritive and toxic properties of prey as well as the role of learning and sampling in prey choices. It is too soon to tell how accurately prey choices can be predicted by optimal foraging theory, but, to date, the theory has met with surprising success, and some of the most notable successes have been with birds (Krebs 1978, Pulliam 1980).

If we can discover how birds rank their potential prey and, if indeed, birds expand and contract their diets in predictable ways, then we can predict dietary changes in response to environmental fluctuations. Presumably, different bird species will rank the same prey in different ways. If so, we can predict how diets of different species will converge or diverge as a function of prey abundance. If the theory can also predict changes in habitat use, then we can predict how birds of one species will affect the resources available to and the feeding behavior of other species during the important years of resource shortage. I believe that it is at this micro-ecological level that competition must be understood before we can begin to reassess the importance of competition in structuring communities at the macro-ecological level. In a more general sense, greater attention to processes and relationships at such "micro" levels may be necessary before "macro-theories" can really further our understanding of many biological phenomena.

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On the Philosophical Basis of Ornithology

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My first premise is that ornithology is an important activity in the affairs of men. The study of birds is no mere whim. Rather, all effective civilization is dependent on the level and nature of the character in its citizens, and the practice of ornithology can build character. People are edified by their exposure to bird study.

My second premise is that the essence of all ornithology is in its data, or more generally, in the experience that people have with birds. The more interesting the experience or data, the more effective the ornithology. Interesting experience feeds the imagination, challenges the mind, stimulates reflection, exercises the memory, and energizes the person.

My third premise is that theory exists to make data more interesting or to discover

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interesting data. Ornithological experience (data) is often boring, but excitement and significance are added by some relevant theory. Theory adds interest to data by ordering it and making it meaningful.

My fourth and last premise is that people trying to do science are vulnerable and require discipline and method to prevent their falling into error. By error, I mean ways of thinking or acting that do not edify.

These four premises lead to some consideration of the major pitfalls in doing science and of the method that protects us from these pitfalls. First, I note some areas of vulnerability.

1. Research is a re-search, a searching again, a process of re-discovery, or of discovering more when one looks again. But there is in our human nature a desire to think we "have it." Scientists, especially those who teach, are susceptible to the temptation to act as if they *have* the answers, instead of as if they are "searching again" for the answers.

2. We never know the truth in science, so all ideas are more or less wrong. We do not evaluate ideas by their "rightness," therefore, but by their effectiveness in stimulating interesting experiences. Scientists, when mature, are humble enough to be comfortable with being corrected, are tentative in their interpretations, and yet are still willing to propose ideas for the sake of improving the data.

3. Basic science meets an important need that we have as human beings, our need to be curious and learn. We tend to downplay the need to be so satisfied, however, placing physical needs at a higher priority. This is backwards; it is better to be poor and happy than to be secure and depressed.

4. Scientific societies, including the AOU, exist to encourage science. In many areas (especially the journals), however, the society functions as a place of judgement rather than a place of encouragement. Judgement is a human weakness that is supposed to be resisted. Universities appreciate this fact in their tenure systems, where a scientist is protected from the overzealous judgements of his colleagues and his society. The work of scientists is evaluated by the fruit of that work, by the predictions that are tested and confirmed with new data. Because science is so patently and patiently empirical, there is no need for any scientist ever to judge or evaluate another, or the work of another. The facts speak for themselves, for or against any contributions. This principle is especially pertinent to editorial boards and reviewers working with a limited budget. Great care should be taken to choose especially relevant, timely, or popular articles for publication, without judging the "scientific merit" of papers not published, and discouraging their authors.

5. Most scientists earn degrees identifying them as scientists. This degree is called the Doctor of Philosophy, which means "Expert in the Love of Wisdom." Scientists place wisdom above knowledge, perspective above power, progress above prestige. Yet we are all tempted to neglect our philosophical title and responsibilities.

Our vulnerability in all these plus many other areas has prompted the development of a scientific method that we can follow to do good science, even when we are compulsively seeking knowledge, afraid of being wrong, critical of our colleagues, or basking in our authority. This method is:

First, we observe a difference in nature and ask a question about it, e.g. why it occurs, how it came about, or what does it mean. Then we propose an answer to the question. We speculate, guess, or hypothesize some idea that explains what we observed. We may then build a model of this explanation to clarify it and to help us make predictions from it. In order to make predictions, we consider all the things that we can measure that might be relevant to the system we are investigating. We then make predictions about these measurable quantities. Each prediction should be testable, unknown, and as surprising as possible. Finally, we test our predictions or have someone else test them. The results put us back to step one.

If I may borrow an analogy from C. S. Lewis, the scientific method in general and theory, hypotheses, or ideas in particular have the same relationship to the experience or data of science as garden tools have to a garden. Neglect the tools and the work goes very slowly, with a great lack of order and beauty. With the tools in hand, and properly used, weeds are eliminated, every plant grows in its place, and the whole garden accomplishes its purpose of being something that builds up all who visit there. The tools are therefore invaluable. But if we lay a hoe down next to the rankest weed, we see at once that the weed is far more beautiful, and amazing, than the hoe. So the most tedious list of measurements of an ornithological phenomenon is far more amazing and beautiful, when compared to the most elegant theory or model. Only the gardener who is thoroughly frustrated with a weedy garden rejoices at the sight of a hoe, and then not because the hoe is beautiful, but because he sees it as a means to a more beautiful garden.

The scientific method is drawn from the experiences of the best or most effective scientists in history. It is in part a method of imitation, but is also proven to be effective by application of Bayesian statistics (see R. A. R. Tricker 1965, The Assessment of Scientific Speculation, New York, Elsevier Publ. Co.). In the context of the scientific method, the value of models is apparent. Mathematical models clarify explanations by listing assumptions and explicating logical steps. They also allow one to make more precise and unlikely predictions from hypotheses. To see models as more than they are, either positively or negatively, is to be avoided, as such a view provokes resistance from those who are in a place to test the predictions generated by the model. Like all sciences, ornithology progresses when those who are efficient in gathering data employ their efforts in testing predictions proposed by theoreticians and when theoreticians employ their efforts explaining the data gathered by field workers or modeling the explanations offered by others. This requires humility on the parts of both kinds of workers, as well as a deep concern to see the field generate as many interesting results as possible. The most cursory inspection of the state of mind of the contemporary American citizen, coupled with some reflection on the effect of good ornithology on that state of mind, raises the whole issue to urgent status.

Mathematics, Ecology, and Ornithology

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Ornithologists and other field biologists, being accustomed to a science based on the solid cornerstones of fact and observation, often look with suspicion upon theory and mathematics and bristle at the invasion of their territory by a new breed of investigator with no formal credentials in the discipline. To some extent, these reactions are justified: much of mathematical ecology is simply mathematics dressed up as biology, and is dismissed by field biologists as being of no relevance to their

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