broadening of perspective. We can at least demand that its students have a clear view of the theoretical and empirical approaches to a concept or question and demonstrate expertise in one and at least understanding, if not generative ability, in the other.

There certainly are theorists who are so good that we should keep them producing theory and away from the field, and, likewise, there are bird watchers so competent that it would be callous and unproductive to have them learn matrix algebra. Theirs are steep-sided adaptive peaks, and small distractions would diminish their output and worth. We should try to learn from such people, try to keep up, and hope for a biological liaison to interpret and disseminate their work. But the vast majority of us do not fall into this category and might well benefit from the reallocation of, say, 10% of our time to our weak side of the concept: I'm convinced the fitness set really is convex.

And then, it's by no means certain to what extent we can educate for the sort of breadth we think might be necessary for the ideal bird ecologist (let's say). It seems that people are educable in only limited ways, to a limited extent, and at limited times. This variation is presented to a selective society, and, among others, biologists and ornithologists are its products. For whatever reason, many ornithologists simply missed the boat at an earlier age and failed to grasp the inherent beauty of a purely symbolic representation of facts and their interrelations, with its greater potential for analysis, manipulation, and extrapolation. And many theoreticians get no further in the field than worrying about ticks, snakes, and poison oak, and no amount of training can change that.

Ultimately, good research is the product of two qualitites: judgement and perspective. There are so many variables to measure or model that one has to be selective; this selectivity is the basis for parsimonious and concise biological relations with both generality and realism, and it takes good judgement. It is a product of and contributes to a researcher's perspective, his own peculiar integration of theory, concept, and the facts of natural history against which each new datum is judged. Other than by submitting a paper to The Auk, it's difficult to know beforehand with whom these qualities lie.

Ornithological Theory: Whence and Whither?

James R. King[1]

Ornithology and other taxonomically oriented sciences (mammalogy, herpetology, entomology, and so on) are modes of inquiry that foster the synthesis of knowledge about the adaptations and manifold life-history patterns of free-living animals. They subsume aspects of many process-oriented disciplines (e.g. population dynamics, physiology, functional anatomy, embryology, ethology) and provide a focus for intellectual exchange and socialization among adherents who are interested in understanding the lives of intact organisms. Ornithology and its companion sciences thus offer arenas for the synthesis of knowledge derived from substituent analytical disciplines. It follows that there are no theories of ornithology itself, and the role of

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theory and abstraction in ornithology stems from the disciplines that ornithology comprises. As noted by a recent survey (AOU/NSF Workshop on a National Plan for Ornithology), specialists affiliated with ornithology overwhelmingly identify themselves as ecologists, closely followed by ethologists, population biologists, and evolutionary biologists. It is the practitioners in this large majority, therefore, who dominate the stage in any roles that theory and abstraction play in today's ornithology.

In an effort to cope more comprehensively with complex natural systems that elude clear-cut experimental manipulation (and often elude even clear-cut definition), ecologists and others working particularly at disciplinary interfaces (e.g. population ecology, behavioral ecology, community ecology) have resorted to an overt or formalistic mode of traditional scientific method that is commonly called "hypothetico-deductive," or "H-D" for short. Some of the adherents of this school of inquiry might prefer "predictive ecology" as a label for what they do, but I will use "H-D" as a generic and, I hope, inoffensive term. This mode of inquiry has not been a novelty since Francis Bacon first sketched the rudiments of a self-correcting system of investigation in the 17th century, but its resurrection in aspects of ecology and allied sciences has had pernicious as well as beneficial results.

The beneficial aspects of H-D formalities are powerful in helping to understand extremely complex natural systems. The method enforces an effort to identify all the important variables in a system and tends to promote a priori hypotheses that clarify the kinds of observations and tolerances of error required for falsifying or verifying the hypotheses themselves. In short, H-D methodology helps to organize efficient inquiry and promotes the search for alternative explanations, thus minimizing needless or deficient "data gathering" and monomaniacal interpretations of results.

H-D methodology has become quite fashionable. Fashions tend to attract a fringe of extremists, misguided innocents, and shallow imitators, and it is at the doorstep of this contingent that one finds the pernicious results of H-D formalisms and their ramifications into mathematical modeling and computer simulation. I have deliberately used the word "pernicious" to emphasize my apprehension that we are confronted here by more than merely one extreme of the usual continuum between reliable and questionable science. Instead, I believe that I detect several trends that perniciously subvert the purposes of the H-D philosophy that spawned them.

The Idols of the Tribe.—Francis Bacon in his *Novum Organum* (1620) identified several allegorical "idols," or sources of bias in scientific inquiry. The Idols of the Tribe include anthropomorphism in general and, in particular, the human tendency to insist that there is more order in nature than actually exists. Biological systems are vastly more complex than physical systems (in which "order" has been relatively easy to find) and have therefore provided a rich matrix for diversification that seems to illustrate that one of the rules of biological "order" is variety. Yet the Idols of the Tribe still afflict the fringes (at least) of H-D philosophy with a relentless insistence on generalization in the midst of adaptive variety. This insistance is not infrequently expressed in a Procrustean logic that says, in effect, "let's trim the horse to fit its harness" (or, "if our observations do not reveal 'order,' let's invent some anyway."). This is illustrated, for instance, by the frequent but unproven assumption (or "fact" to the Idolaters of the Tribe) that competition for resources is ubiquitous, continuous, and always coerces the diversification of life-history patterns, community structure, or whatever. Another example is the assumption that the evolutionary resolution of problems involving mutually incompatible stresses or limited resources tends toward

optimality. This is intellectually appealing, but what is proposed as a biological tendency not infrequently becomes transmuted into the insistence that evolutionary solutions *are* optimal. It seems to me that at least some H-D practitioners have swallowed these assumptions hook, line, and sinker without overtly considering any of the alternative hypotheses that their methodology is purported to foster (for instance, that organisms are not as seriously or as continuously stressed as we imagine them to be and can tolerate suboptimal solutions for most or all of their lives).

Damn the torpedoes.—Conceptual and mathematical models require parts (variables, parameters), and H-D methodology is useful in identifying the parts that are necessary and sufficient. Not uncommonly, however, overzealous practitioners plunge ahead at full speed even after discovering that a vital part is missing. They simply plug the gap with an assumption. This expedient is not peculiar to just H-D practitioners, and, given the complexity of the systems with which they work, it is surely more tolerable in them than in many others. But H-D extremists are often rather cavalier about their assumptions, and what was intended as a temporary expedient has a way of becoming a permanent fixture. They, and others, too often allow assumptions to be shoved under the carpet, and thus mislead the unwary. This could easily be avoided by undertaking an error analysis of the models in which this is possible, thus acknowledging soft spots explicitly and testing "robustness" (in the lingo of the modeling chaps). Readers may thumb through a few issues of the *American Naturalist,* for instance, to learn how often this form of self-examination and self-correction is supplied (i.e. not very often).

Science is such fun.—A number of the overly enthusiastic tendencies already mentioned occasionally converge and culminate in game-playing, or counterfeit science. This is the apogee of the Assumption Syndrome, discussed above. The formalistic H-D format in these cases seems to become an end in itself—a fascinating game of "what if?" One of the strengths of H-D methodology is its predilection for alternative a priori "hypotheses" that help to identify and assemble appropriate observations. Paradoxically, this also seems to be one of the major weaknesses of the method, as not all practitioners are able to separate the wheat from the chaff. Many a priori "hypotheses" are patently speculations that are intended to assist the organization of inquiry but instead become accepted as "explanations" for nonexistent data. Speculation is unquestionably a powerful heuristic force in the art of science. Occasionally, speculations are even fit to print, but only when clearly labeled as what they are. Anyone not sure about the difference between speculation and theoretical biology should read the Instructions to Authors in the *Journal of Theoretical Biology*. The Editors demand interpretations that are traceable to data. Lest anyone conclude that my preoccupation by data is the overwrought reaction of an outsider (my training is in physiology), I hasten to point out that ecologists themselves are voicing reservations about the state of their trade. In the same month in which this essay was written, four prominent practitioners published commentaries urging better attention to procuring (Fretwell 1979, Bird Watch 7: 2), reporting (Connor and Simberloff 1979, ESA Bull. 60: 154), or analyzing data (Innis 1979, ESA Bull. 60: 142).

The "science is such fun" fringe also displays its attitudes in the spattering of cutsie-pie descriptors that have appeared recently in the titles of journal articles (to coin an example: "The role of the aristocracy in avian foraging guilds"). I concede that concocting such descriptors may be fun. It may even be harmless, but only if they refer to exact homologs of human processes or organizations. If they do not,

then they are merely titillating allegories that reveal a suspect tolerance of anthropomorphism (again, the Idols of the Tribe).

The Pierian spring.—Time and natural selection may moderate many of the excesses and aberrations that I have just described, but what can humans do to accelerate this purge? One obvious answer is to reverse the trend toward hyperspecialization that currently tinges ecology and allied interdisciplinary fields of biology. I have examined many scores of undergraduate and graduate programs of study in ecology and sister disciplines and am perplexed by the frequency of heavy emphasis on "in-house" courses (to name but a few: terrestrial ecology, aquatic ecology, marine ecology, desert ecology, forest ecology, community ecology, population ecology, behavioral ecology, statistical ecology, ecological genetics, and so on). This seems a startling paradox in fields of inquiry that are purported to rely heavily on knowledge from the basic disciplines that, by definition, they subsume. The result is that instruction in the basic sciences is diluted by redundant "interdisciplinary" coursework. Advanced students in such curricula now, on the average, escape all but rudimentary training in chemistry, frequently escape any instruction at all in physics, and worst of all, commonly elude all but undergraduate experience in the basic biological disciplines (morphology, systematics, physiology, embryology, and so on) that are the sources of knowledge for the support of their interdisciplinary aspirations. This smacks of the inbreeding that characterizes many professional schools and can culminate by sharing their status: isolation from reality, a superabundance of True Believers, and a shortage of skeptics.

Thus, the circle closes. I began by identifying some problems in contemporary theorizing in the prominent subdisciplines of ornithology and end by suggesting that these problems are abetted by vocational hyperspecialization and the limited historical perspectives that this entrains. Of course, the criticisms that I have voiced are a double-edged blade that can lacerate not just a handful of theoretical ecologists and their allies but also anyone who ventures innocently or too enthusiastically beyond the limits of personal competence. Alexander Pope, in his *Essay on Criticism,* reminds us all to "Drink deep, or taste not the Pierian spring./ There, shallow drafts intoxicate the brain/ And drinking largely sobers us again."

Last of all, by way of summary of what I have tried to say, I am willing to provide *gratis,* to anyone who requests a copy, a flash-card bearing Elinor Wylie's quatrain: "Go study to disdain/ The frail, the overfine/ That tapers to a line/ Knotted about the brain."

On Digesting A Theory

H. Ronald Pulliam[1]

Good theories, like gourmet meals, require a lot of time to prepare, to swallow, and to digest. Robert MacArthur was a master preparer of theories. His recipes called for a thorough blending of equal parts natural history, common sense, and simple mathematics. Some years ago, I happily swallowed MacArthur's theory of community structure; now I am having trouble digesting it. Although I no longer

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