Lost Opportunity?

The American public may well have lost the opportunity to take effective measures for self-sufficiency with regard to energy (see P. H. Abelson, Editorial, 3 May, p. 525). But the opportunity could not be missed by the oil industry. They now have their profits and their pipeline. Furthermore, the so-called "crisis" ended just in time so that we are still locked into the same old game with hydrocarbons, while paying for it with our environment.

I submit that neither the "jackass" nor the elephant will budge as long as he has sufficient straw for a meal under his nose. However, before he is impelled to move, perhaps we should consider the difference between need and want, between bread and cake. As long as 1 gallon of gasoline is permitted to power a pleasure boat, a lawnmower, a snowmobile, or a dune buggy, we cannot claim that there is a "need," and the rape of our Alaskan wilderness or of a mountainside in West Virginia is not justified.

If his inertia is due to a plethora of choices, then one remedy might be for us to limit the options. Yelling "environmental impact" long or loud enough may reduce the number of undesirable options, slow down cancerous "growth," make us recognize the difference between bread and cake, or maybe all three.

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Scaling in Ecology

Robert May (Book Reviews, 22 Mar., p. 1188) mentions the lack of references to MacArthur, Levins, and Hutchinson in his evaluation of a recent book on ecology (*1*). At first I dismissed this as a complaint about Englishmen who are untouched by the public relations web that spun out "Theoretical ecology: Beginnings of a predictive science" (Research News, 1 Feb., p. 400), which mentions *only* MacArthur, Wilson, and their associates.

I now eschew such base notions in favor of a testable hypothesis that leads to predictions about the information flow among ecologists. My scaling hypothesis proposes that all ecologists tend to be either pure scalers (those who invariably attach numerical values to both axes of their graphs) or the scaleless (those who simply indicate the quality associated with an axis). The hypothesis that this would distinguish two discrete groups of ecologists was first tested on a Brookhaven symposium (*2*), to which a wide spectrium of ecologists contributed. There were nine pure scalers and the remaining five authors put scales on fewer than half their graphs. I then analyzed volumes 102 and 103 (1968 and 1969) of the *American Naturalist*. The frequency of pure scalers was 0.56 (sample $N = 18$) and a contingency table showed that there was no significant difference between the samples ($\chi^2 = 0.258$, 1 degree of freedom, $.5 < P < .7$). An average of 45 percent of the graphs from the scaleless set were without numerical scales.

The books (*3*) of Levins (96 percent scaleless), MacArthur (60 percent scaleless), and MacArthur and Wilson (50 percent scaleless) are above the mean for scaleless ecologists, and so the explanation for the lack of citations that concerns May seems obvious. A scaleless ecologist can use anyone's graphs, but a pure scaler cannot extract information from a scaleless graph.

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References

1. M. S. Bartlett and R. W. Hiorns, Eds., *The Mathematical Theory of the Dynamics of Biological Populations* (Proceedings of a conference, Oxford, England, September 1972, Academic Press, New York, 1973).
2. *Brookhaven Symp. Biol.* (1969), No. 22.
3. R. Levins, *Evolution in Changing Environments: Some Theoretical Explorations* (Princeton Univ. Press, Princeton, N.J., 1968); R. MacArthur, *Geographical Ecology: Patterns in the Distributions of Species* (Harper & Row, New York, 1972); ——— and E. Wilson, *The Theory of Island Biogeography* (Princeton Univ. Press, Princeton, N.J., 1967).

Using my book review as a launching pad, Mitchell has written a witty letter which makes some substantial points. Unfortunately, it gives a totally misleading impression of that review.

My praise of the book edited by Bartlett and Hiorns was qualified only by the remark that its scope was narrower than indicated by the title, *The Mathematical Theory of the Dynamics of Biological Populations*. Most of the mathematicians at the conference had backgrounds in statistics, rather than in classical applied mathematics or engineering; the collection tended to emphasize statistical problems such as arise in data collection and analysis, or in population genetics, and to give relatively little attention to broad patterns of energy flow in food webs, or to the biological interactions (predator-prey, competition, mutualism) which mold many communities. To substantiate these observations, I noted that, with the exception of the chapter by Murdie and Hassell, the text contained reference neither to MacArthur, Levins, and Hutchinson, nor on the other hand to Holling, Odum, and Watt. And to this catalog can be added a catholic collection of scalers and nonscalers: Hairston, Patten, Slobodkin, van Dyne, and others. The constructive aim of these oversimplified comments was to point to interesting transatlantic differences of emphasis within theoretical ecology.

Mitchell's letter raises larger and divisive issues. Population biology, like any other science, has need for a continuum of theoretical activity, from very abstract (strategic, scaleless graphs) models aimed at general questions, to very specific (tactical, scaled graphs) models aimed at specific applications. Sympathetically handled, tactical and strategic approaches mutually reinforce, each providing new insights for the other One of the symptoms of ecology's immaturity as a science is a tendency for a few people to regard their own pursuits—be they very abstract or very empirical—as the only truly legitimate activity. Examples of such silliness come from both ends of the spectrum; such polarized controversy is to the detriment of the subject, and of its funding. I modestly think a balanced view is to be found in my (only 19 percent scaleless) book (*1*): "In ecology, I think it is true that tactical models . . . , applied to specific individual problems of resource and environmental management, have been more fruitful than has general theory, and they are likely to remain so in the near future. But in the long run, once the 'perfect crystals' of ecology are established, it is likely that a future ecological engineering will draw upon the entire spectrum of theoretical models, from the very abstract to the very particular, just as the more conventional [and more mature] branches of science and engineering do today."

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