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Notes Towards a Science of Ecological Management

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The thesis presented here is quite simply that it is now possible to catalyze a new science of ecological management/ engineering. The need is obvious, but most significantly the essential pieces, independently developed, can now be integrated and/or used on ecological problems. And even more important, a relatively new concept emerging from ecology can provide a conceptual focus for a new regional strategy of ecological and resource management.

Now that the more intemperate extravaganzas of the recent concern for ecological issues have passed, it becomes possible to identify some solid foundations for ecological management science. On the ecological side these lie in three areas which have been developed over the past fifty years. The first two have come from applied areas - insect pest ecology and fisheries ecology. Both have been characterized within a rich scientific tradition, one which comes as a surprise to those more familiar with the "eco-freak" image of recent years. Here there is a remarkably sound empirical base - both extensive and intensive - characterized and indeed initiated by the R.A. Fisher school of statistics and sampling theory. There is also a mixture of laboratory and field experimentation that has unravelled and generalized many of the key causal relations that link organisms with each other and with their environment. And, finally, there has been an active mathematical tradition of modelling; differential equations initially, and then - with the appearance of computers - differential-difference equation mixes leading up to but, as yet, not beyond simulation models.

Simulation models in ecology, as in any fields, initially were oversold. There were noble and grand efforts to develop <u>the</u> generalized model of this, that or the other ecosystem.

Many models became so complex as to be as mysterious as the real world. We are through that inevitable stage now and we see growing numbers of effective efforts to bound, intelligently, problems from the outset - to compress and simply up to but not beyond the point where essential behaviour in space and time are retained.

The third area of relevant ecological development is the theoretical. Ecological theory has tended to be divorced historically from application, and finds its roots more in evolutionary biology. But from that theory have emerged a number of concepts of ecosystem structure which have begun to form a happy partnership with the empirical and modelling approaches of the applied branches of ecology. The result has been several rather major steps towards describing and quantifying the stability behaviour of perturbed ecological systems. It is this latter development that potentially provides the conceptual foundation which gives me the temerity to suggest that something new and innovative is possible in designing a science of ecological management. I shall amplify this point later.

Now, however, it is more important to touch on the missing pieces of this apparently glowing story. And the missing pieces represent the serious gaps which have made ecologists lousy managers. The main issue is that man and society have largely been left out of even the best of applied areas. It is true that economics (in its guise of resource economics) has crept into fisheries management. The partnership flowered for a time but began to wither as the economics tended to move into more and more esoteric academic numerology. There are notable exceptions, but the fact remains that the marriage of ecology and economics has been an uneasy one that has, with few exceptions, never been effectively consummated. The reason, I believe, is that the marriage was largely in isolation from broader societal concerns and from the techniques that have evolved in the management sciences, particularly policy analysis and decision theory. The result is that applied ecology has tended to be descriptive and not prescriptive. Hence the new conceptual focus should illuminate an integration of the best of ecology/economics modelling, policy analysis, and decision theory to provide the basis for a new science of ecological management/engineering.

Let me now touch further on the relevance and need for a fresh conceptual framework. The past management of ecosystems has implicity presumed that the consequences of an incremental action will be quickly detected. If the intervention produces higher costs than benefits, then a revised incremental action can be designed. It is this trial-and-error strategy that has succeeded in producing phenomenal increases in production of food, fiber and other resources needed by man. Little knowledge of ecosystems was required so long as the consequences of an erroneous trial were minor and alternate trials remained possible. It has been an admirable and effective method of improving our lot in spite of our ignorance.

But now, incremental acts seem to be producing more extensive and intensive consequences, consequences which resist further incremental solutions. The geographical scale of our interventions and their magnitude can now make an erroneous trial disastrous. That is dramatically obvious in nuclear power developments, but it is equally true of resource developments. In addition, other consequences are emerging from the accumulation of past incremental decisions. Our remedial responses to these new emergencies are as shortsightedly ad hoc as their original causes. Banning D.D.T. may seem admirable, but advocating such narrow solutions can lead the ecologist to join that group of apparent villains (I emphasize apparent) who planned our freeways and designed our dams. That is a good way to destroy the myth of the ecologist's moral rectitude but hardly a way to be responsive to significant social needs.

Trial-and-error seems to be an increasingly dangerous strategy for coping with ignorance. And yet the solution cannot be to withhold action until we have sufficient knowledge. We need a new strategy for dealing with the unknown. One direction to go might simply be to engineer nature, (i.e. the unknown) out of the equation. With enough concrete and energy we could make the world a known one. That is the route which led to the semi-humorous suggestion that the pest problems of "miracle" rice could be resolved by paving and then flooding all of southeast Asia. But we don't have enough concrete and energy and there is no way to engineer out those vexing and disturbing human demands for "quality of life". That scarcely is the route for dealing with unknowns.

Four major classes of uncertainties and unknowns may be identified. We have incomplete, although growing, knowledge of the functional relationships within ecosystems -- of their number, kind, form, and intensity. Also, we have limited knowledge of the social objectives for ecosystem management. There are hidden objectives and they remain so until they are suddenly no longer satisfied. These two sources of ignorance -- the descriptive and prescriptive -- are important but manageable. Presently techniques can identify and hedge against these sources of uncertainty in inputs, parameters, functions, and alternate values. Much of systems analysis is directed to these problems.

But what of the qualitative unknowns inevitably dealt us by 'fickle fortune'. The basic rules underlying linked economic-ecological systems can change. Unexpected species can suddenly appear and dramatically alter ecosystem structure. Unexpected economic changes can do the same - witness the observed and potential impact of the energy shortage on food production. And the one-in-a-thousand year flood or drought is as likely to occur this year as any other. In the same way, prescriptive aspects of management can experience equally unpredictable changes. Human objectives which seem so clear at the moment can and do dramatically shift, leaving society committed to policies and systems that cannot themselves shift to meet these new needs.

Few systems that have persisted for extensive periods exist in a state of delicate balance, poised precariously in some equilibrium state. The ones that are, do not last, for all systems experience unexpected traumas and shocks over their period of existence. The ones that survive are explicitly those which have been able to absorb these stresses. They exhibit an internal resilience. Resilience, in this sense, determines how much disturbance - of kind, rate, and intensity - a system can absorb before it shifts into a fundamentally different behaviour.

Historically, ad hoc management approaches have succeeded specifically where applied to highly resilient systems. The inevitable mistakes, made from ignorance, were first additional disturbances that could be absorbed by the resilience of the system. But that resilience is not infinite. We can now show, from our ecological models, that ecological systems are multi-equilibria ones and, moreover, can demonstrate the causal mechanisms leading to multiple equilibria. These equilibria are bounded and so produce stability regions within which the variables fluctuate and move with relatively weak damping. Exogenous disturbances - natural or man-made - generally cause modest or undetectable numerical change within this highly fluctuating world. The qualitative behaviour remains unchanged and, most significantly, no signal is generated of a possible contraction of a set commonly inhabited stability regions. That signal is only generated when the disturbance is great enough to flip the system into regions normally not occupied. Or it is generated by accumulation of past incremental decisions that have led to a contraction of the normal stability regions. A disturbance, such as a normal fluctuation of climate, that previously could be absorbed no longer can be. That is what much of the eutrophication literature is all about; and that is what has led to the collapse of most of the freshwater fisheries of the temperate world. A more detailed treatment, with examples, can be found in Holling 1974 (C.S. Holling, Resilience and Stability in Ecological Systems; copy attached).

The point I wish to make is that the traditional view of stability, as presently practised, concerns responses to small perturbations and considers stable systems as those which fluctuate least and damp most rapidly. But an equally valid view concentrates on the responses to large perturbations and reveals that highly fluctuating systems can be immensely "stable" in that they can persist in the face of major disturbance.

This view leads to a strategy of management that can attempt to work with the natural dynamic rhythm of ecosystems - that attempts not to eliminate fluctuations but to transfer them into directions less in conflict with man's desires; that attempts to design systems which are not so much fail-safe but safe in the inevitable event of their failure (remember Hurricane Agnes?)

With that rhetoric behind me, let me attempt to encapsulate the ingredients of this new science of ecological engineering.

1. Conceptual - a rigorous development of the resilience/stability concepts based on representative theoretical and applied models ranging from coupled differential equation (for historical reasons), through simulation models of simple ecological systems (few state variables) to those of complex ecosystems (many state variables, non-linear, spatial disaggregation).

2. - numerical quantification of resilience: the ecological "Reynolds" number(s).
   - retrospective case studies from ecology, resource sciences and social sciences analysing

the resilient behaviour of the systems in response to major stress.

2. Development of resilience indicators that provide at least surrogate measurements reflecting the size and nature of stability regions. Such indicators seem to fall into three main classes: resilience in unused environmental "capital", resilience in relation to stability boundaries, and resilience of policy failure .

3. Development of environmental standards that recognize the fluctuating nature of systems and lead to a balance between preventative and remedial responses to meeting standards (see Fiering & Holling 1974; Management & Standards for Perturbed Ecosystems: copy attached).

4. Development of a strategy for generating policy alternates ranging from the "fail-safe" to the "safe-fail".

5. Blending the above with existing and expanded techniques of systems analysis that have been so effectively developed in the water resource field in particular: in essence all those techniques of policy analysis including optimization (where it can be stretched) and more heuristic, "dirty" techniques.

6. Joining the above, in turn, with decision theory to deal with questions of decision-making in the face of uncertainty, and of problems of multi-attribute decision making.

7. Finally, developing communication formats and process that force the analysis to be responsive,

useable and transferable to the man who makes decisions and those who endure those decisions.

All this, I hasten to add, should be developed around carefully chosen case studies which possess both applied significance and the potential for conceptual methodological advances.

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