Socio-ecological webs and sites of sociality: Levins' strategy of model building revisited.

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Abstract:
This essay reformulates Levins' 1966 analysis of model building in ecology and evolutionary biology so as to identify several points where decisions are required that are not determined by nature. Determining the range of competing models compared is one such point. These decisions are an unavoidable part of modeling, which invites us to examine what else modelers are responding to, what reactions are taking place at these "sites of sociality." It seems that scientists select their problems, define their categories, collect their data, and present their findings so that, simultaneously, the models can be seen to represent their subject matter, the modelers can secure the support of colleagues, collaborators and institutions, and they can enjoin others to act upon their conclusions" -- scientists are weavers of "socio-ecological webs."

Keywords:
Modeling; Levins; Ecology; Sociality

In 1966 Richard Levins sketched a strategy of model building in ecology and population genetics that favored sacrificing "precision to realism and generality." Models should be seen as necessarily "false, incomplete [and] inadequate," but productive of qualitative and general insights. Discrepancies between a model and observations imply the need for additional biological postulates and, together with the qualitative insights, generate interesting questions to investigate until, eventually, a model becomes "outgrown when the live issues are not any longer those for which it was designed" (Levins 1966).

Mathematical modeling in ecology has proliferated and grown increasingly sophisticated in the subsequent decades. In the early 1980s, however, a strong counter-current developed. Reacting against ecological theory drawn from simple, general models, certain ecologists came emphasized experimental testing of specific hypotheses about particular situations (Simberloff 1980, 1982; Strong et al. 1984). Nevertheless the value of models for stimulating the development of ecological theory continued to be upheld, especially by, not surprisingly, mathematical ecologists (May 1973, Hutchinson 1978, Levin 1980, Hall and DeAngelis 1985, Caswell 1988). The resulting state of play might be summed up by saying that most ecologists no longer expect qualitative, general insights to be derived, but mathematical modeling and modelers have secured a place in ecology.

I sympathize with an emphasis on a stimulatory or "exploratory" role for models in ecology (Taylor 1989), but I want to look more closely at Levins' strategy of model building. I will reformulate his strategy and develop it in order to identify where social considerations are built into mathematical modeling. A model, as the *product* of modeling, might be valued according to its correspondence (precisely, roughly, temporarily) to reality. Yet Levins' emphasis on the *process* of modeling leads me to draw attention to the ways scientists select their problems, define their categories, collect their data, and present their findings so that, simultaneously, the models can be seen to represent their subject matter, the modelers can secure the support of colleagues, collaborators and institutions, and they can enjoin others to act upon their conclusions. The metaphor I develop is of modelers as weavers of "socio-ecological webs."

False models as a route to truer and more critical theories

Neither the critics nor the defenders of modeling have done full justice to Levins' 1966 views on theory production. (For recent assessments see Palladino 1991; Orzack and Sober 1993; and Levins' 1993 response to Orzack and Sober.) The focus has rested on Levins' classification of models as general, realistic, or precise, and on his claim that all these qualities cannot be achieved simultaneously. Three other features are, however, central to his thinking: robustness, contradictions, and commitment to social change.

Robustness enters as follows: Given that simple models are necessarily false, modelers should try to find insights that are common to many simple models and which thus depend not on the details of any particular model, but on the structure that is common to them. The resulting insight would be "robust." As the old advice to left-wing readers of mainstream newspapers goes, one tries to discern the truth by noting the points of "intersection of independent lies."

"Robustness" is, perhaps, an unfortunate term here, because it connotes solidity of the result. Levins is concerned with the vitality of the modeling process; he is not proposing a method intended to guarantee the truth of the outcomes (Levins 1993). In fact, truth for Levins is always provisional -- depending on some concordance between the problem posed and the criteria for answers, and always vulnerable to someone exposing "contradictions." Although the term contradiction is not used in the 1966 paper, the idea of a never-ending process of disturbing the (provisional) validity of models runs through Levins’ work. "It is the obligation of every scientist looking at even the most strongly established laws to ask under what circumstances might these laws be overthrown" (Levins, pers. comm.; interview with author and Iain Boal 1983). In this spirit, Levins delights in setting up a model, say, of a host-parasite relation, only to upset the model by bringing in a new or previously hidden factor, say, a predator (see this volume). His technique of loop analysis (Levins 1975), for example, has allowed him to highlight the counter-intuitive consequences of incorporating the indirect effects of species (or other variables) that are active in a system but omitted from the model being used. In a similar spirit the technique of time-averaging (see Puccia and Levins 1985) allows one to expose counter-intuitive correlations among variables.

Of course, provisional validity can carry a lot of weight (Levins 1983). The circumstances under which laws might be overthrown are not always apparent to scientists, let alone able to be created by them. In his work on epidemiology, agro-ecology, and the dynamics of science and technology in general (Levins and Lewontin 1985), Levins considers the social conditions in which knowledge is produced. (For example, under a research and development system geared to firms making profits, pesticides have been favored over biological control of pests.) He has acted upon his commitment to social change through his solidarity work with scientists in Third World countries, especially countries attempting revolutionary transformations such as Cuba in the 60s, Vietnam in the 70s, and Nicaragua in the 80s.

In summary, Levins wants to find out what is going on in the world, but the boundaries of his world often include as much society as nature. The validity, necessarily provisional, of his insights depends less on how tightly the insights correspond to any particular situation, and more on how much they stimulate new ways of looking at the world. Change, whether or not we seek it, characterizes the world, natural and social. Science, and, in particular, our strategy of model building, needs to embrace the processes of change.

Exploratory modeling as one level of model building

Despite Levins' broader social perspectives, reality has not been displaced as the benchmark in his account of modeling. All models are "incomplete" -- they cannot possibly include all aspects of reality -- nevertheless they can provide insights about the real world. Discrepancies between a model and observations are taken as grounds for adding biological detail with the aim of tightening the correspondence. Admittedly, Levins' work has not emphasized data analysis or detail, so the power of model building for him, especially when enlivened by contradictions and context, rests on disturbing conventional notions of the way the world works. Models may be false but, in Wimsatt's words (1987), they are "means to truer theories." In what follows I suggest a more discriminating view of how models can be justified. This clarifies Levins' strategy of model building and allow us to elaborate on it without rejecting Levins' three interests, namely, in what is really going on, in social considerations, and in changing and disturbing conventional scientific accounts.

Although all models necessarily simplify reality, it does not follow that they are designed and applied according to the same standard for correspondence with observations. Some models are evaluated by a quantitative analysis of correspondence between patterns in the data and predictions from the model. Other models, or the same model in different situations, gain acceptance according to the general plausibility of their assumptions and predictions.

Furthermore, there are two faces to any model: On one face there is some distinguishing feature. Levins' loop analysis, for example, models a system of interrelated variables, such as a community of interacting populations, near equilibrium (Puccia and Levins 1985). The equations for the rate of change of each variable can, therefore, be linearized, that is, simplified to linear, additive combination of the populations. On the other face there are accessory conditions; for example, in loop analysis the system is assumed to be at an equilibrium or to regain it rapidly after any disturbance, the environment of the populations is constant in space and time (Williams 1972), and the modeled populations are effectively independent from the unmodeled or "hidden" variables. Accessory conditions are often overlooked or difficult to establish, especially the last condition of effective-independence, which requires the unmodeled context to enter only through the constant parameters of a model (Taylor 1985, chap. 3).

Figure 1 presents a schema of model justification incorporating both the distinguishing features that characterize a dynamic or causal model and the model's accessory conditions. (The other elements of the schema are described in the caption.) We can now distinguish three broad levels of correspondence between a model and observations (Taylor 1989), summarized in the table below. (It should be noted that two models, even if they have the same mathematical formulation, should be considered to be different models if one has its parameter values specified precisely and the other restricts the parameters only to the range of, say, positive numbers. This contrasts with Orzack and Sober 1993.)

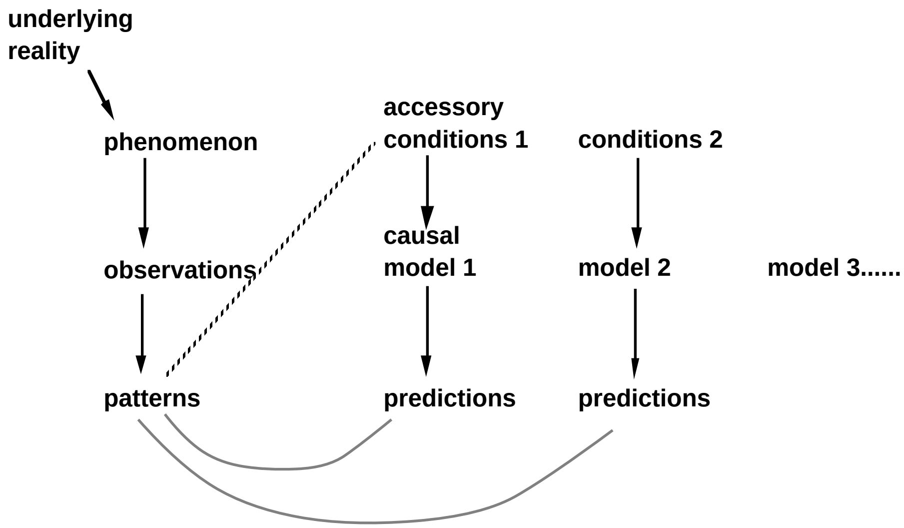

Figure 1 Schema of the confirmation of scientific models. Arrows indicate the sequence of steps in time; the dashed lines the inclusion of accessory conditions in any causal models; and the gray curves the two aspects of the analysis of correspondence.

Level of correspondence	*Fit*	*Accessory Conditions*
1. Framework	—	—
2. Redescription	√	—
3. Generative representation	√	√

√ = quantitative analysis of correspondence with data
— = correspondence judged to be plausible or not examined

Different applications are appropriate to each level of correspondence, therefore models should not be judged equally (i.e., tested) as representations of reality. Nor, on the other hand, should they be excused indiscriminantly on the grounds that the questioner is asking too much of the model. A schema simply focusses our attention on certain biological processes; e.g., in loop analysis, population growth and limitations on or facilitations of that growth. In addition, if the schema can be expressed in a mathematical formulation, the model becomes what I call an exploratory tool. It can be explored systematically as a mathematical system, e.g., how does the system's behavior change as its parameters change or become variables, as time lags are added, and so on? Such mathematical investigation may help us derive new questions to ask, new terms to employ, or different models to construct. A redescription or summary of the observations in the form of a model, whose parameter values have been estimated from the data, permits prediction (or extrapolation) on the basis that past patterns might continue (or extend). Finally, if the model not only fits the observations but also independently of that fit there is evidence for its accessory conditions, then we are justified in acting as if the model represented the biological relations that generated the observations. That is, we can accept the model as a generative representation. Because the model captures the necessary and sufficient conditions to explain the phenomenon observed, we can make confident predictions for situations not yet observed. (See Lloyd 1987 for a related discussion of theory confirmation.)

Levins' strategy is, in effect, an advocacy of exploratory modeling as a means of theory generation. I take this to be the meaning of his favoring generality and realism at the expense of precision. He is not interested in trying to achieve generative representations, to frame every new idea as a hypothesis to be directly tested, or to build detailed models of specific situations. A focus on these approaches stifles theoretical productivity.

The attribution of generality and realism to this strategy is, however, problematic. Strictly speaking, without a quantitative analysis of correspondence, the insights from exploration are insights about a mathematical system. Their relevance to biology is yet-to-be-established; truth or falsity is thus a moot issue (contra a strict reading of Levins 1966 and Wimsatt 1987). This distinction helps remind us of the uneasy tension that persists between mathematical tractability and the demand that, eventually, the exploratory model be more strictly evaluated against observations. Qualitative insights may have misguided research. The categories of exploratory models, often chosen with an eye to mathematical tractability, may have obscured profound issues about biology. For example, for seventy years population genetic models have been built up from ideal "genotypes" that possess differential fitness values. Within this framework it is difficult to incorporate the construction of characters during ontogeny or to examine the evolutionary significance of such construction (Oyama 1985, Taylor 1987).

Reservations about exploratory modeling as a sure way to produce realistic and general models do not oblige us to swing back to the view that the quickest route to better generative representations relies on every new idea being framed as a hypothesis and directly tested. Suppose, for example, we wanted to reevaluate the logistic as a generative representation. Instead of testing a prediction of the model, we could move back to the level where the logistic is an exploratory tool and examine the effects of the model population being genetically heterogeneous or spatially distributed, of including the population's resource (or other hidden variables) into a new model, and so on. Out of such explorations might emerge ideas about the conditions under which the logistic might work as a generative representation.

The contributions and limitations of exploratory modeling as a means of theory generation warrant further discussion, but this will not be pursued here (see Taylor 1989). Before we move on, however, notice that we have slipped from justification of models to justification mixed with issues of theorizing or model generation. From here on let us permit, in the spirit of Levins' strategy, this interpenetration of model generation and justification. We now have sufficient ideas in place to identify where in the process of model building social considerations are built in.

Sites of sociality

At several points in the preceding sketch of model justification decisions are required that are not determined by nature. Even if the modeler is barely conscious of these decisions, they are an unavoidable part of modeling. That is, the referentiality of modeling is also unavoidably social. Let me characterize six areas or "sites of sociality" (see figure 2) in which something other than experiments, observations and comparisons must come into play.

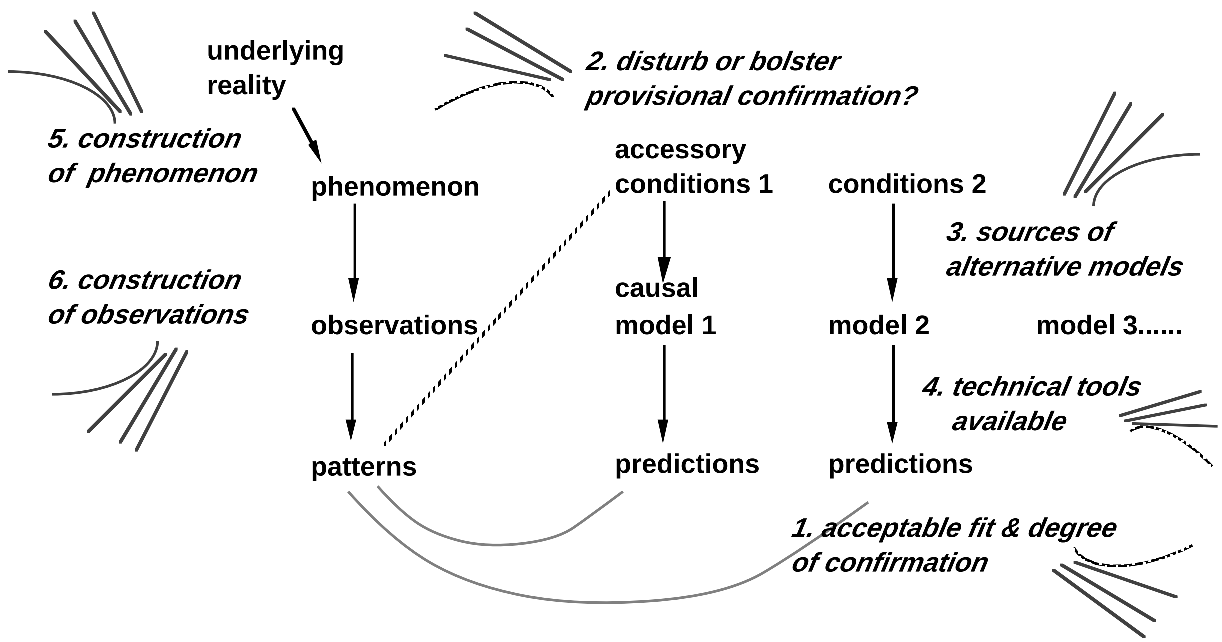

Figure 2 Sites of sociality superimposed on the schema of the confirmation of scientific models from figure 1. The bunches of lines entering each site indicate the potential diversity of influences on decisions.

1. Strictness of confirmation. The correspondence of models and observations varies according to the degree of fit and the strictness with which accessory conditions have been established. (Because confirmation is relative the levels of the previous section become ideal types whose boundaries are, in practice, indistinct.) Disagreements about acceptable fit abound; sometimes even assumptions that are obviously plausible to one person are self-evidently incorrect to another. (For example, I am sceptical of micro-economists' ubiquitous assumption of self-interested, utility maximizing individuals.) Nature does not tell us what degree of fit is acceptable or necessary for good science. Instead groups of scientists decide, invoking one or more of the following, not necessarily consistent, considerations: a) previous experience or standard practice in the field; b) satisfying requirements for technical control over the system; c) comparison with a null model, i.e., one without the distinguishing feature; d) comparison with a range of competing models (see 3 below); and e) openness to periods of exploratory modeling in which analysis of correspondence is loose at best.

2. Acceptance versus disturbance of confirmation Correspondence is also *provisional*, being contingent on the variety of circumstances in which the fit and the accessory conditions have been established, on the stated level of resolution, and on the range of accessory conditions exposed for scrutiny. At some level of resolution most accessory conditions will cease to hold, e.g., organisms that we abstracted as identical units, in order to subsume them into a single variable in the logistic equation, are usually genetically heterogeneous (Lomnicki 1980).

The relative and provisional nature of correspondence means that levels of correspondence are better thought of as levels of modelers' acceptance of models. By mobilizing the freeplay we can reconsider, or be pressed by others to reconsider, our acceptance of a model. The outcome of such reconsideration is more likely to be revision of the model than simply its rejection or acceptance. Revision, moreover, is not necessarily directed at tightening the fit of a model; it may run a gradient from attempting to expand acceptance of the model to attempting to disturb acceptance. In practice, we might (Taylor 1985, chap. 1):

0. *elevate* a model's status, e.g. convert an exploratory tool into a generative representation;
1. *generalize*, e.g., claim an expanded domain of application for the model.
2. accept the model and *shift focus*, e.g. to other models in the theory;
3. *refine*, i.e., attempt to improve the fit by adjusting parameter values or adding details;
4. *qualify*, i.e., give more consideration to accessory conditions;
5. *disturb* acceptance, i.e., search for circumstances in which confirmation breaks down -- in Popperian terms, test a risky prediction; or
6. *decompose* the old model into one with new variables and relationships.

In short, the second site of sociality is constituted by scientists deciding which direction, in the spectrum from accepting a model to disturbing its current degree of confirmation, they will move.

3. Range of competing models Of course, active revision (in the various senses above) cannot be the ultimate source of competing models. How, for example, would it occur to us in the first place to explore possible effects of heterogeneity within a population (mentioned above)? And have we overlooked other ways of questioning the model? At some point ecologists have to borrow from other fields and other situations in order to invent new models. Yet, invention of new models is not an inevitable process in science -- categories can remain plausible and competing models scarcely considered for long periods of time. A few or many models may be subject to comparison, or be available to be compared. The range of competing models constitutes the third site where the sociality of modelers' decisions comes into play.

4. Technical considerations The range of competing models may be constrained by the existing capabilities of mathematicians (aided perhaps by computers) to elucidate the behavior of different models, to calculate equilibria and stability, and so on. Furthermore, some modelers (Levins included) prefer models whose dynamics can be analyzed and understood relatively easily. That is, they dislike models composed of a complexity of different kinds of equations and parameters, holding that the results, necessarily produced by computer simulations, are difficult to explain in terms of the constituent dynamics. Levins argues, in this spirit, that systems ecological models are unlikely to provide insights that can be applied or adapted to other situations. (Palladino 1991 reviews counter claims and places Levins' claim in the context of the late 1960s.) While some technical constraints may be loosened with the progress of science (in the conventional non-social sense), stylistic preferences and the acceptance of other technical constraints remain, contributing to another site of potential sociality.

In a complementary way, technical considerations also constrain the extraction of patterns from data and the statistical techniques we plan to use influence our design of trials (see 5. below). It is not straightforward to relate the models that lie behind statistical techniques to causal models (Lewontin 1974), especially in ecology (Austin 1980, Faith et al. 1987, Minchin 1987), and statistical evaluation and interpretation of explanatory claims is governed by many (necessarily social) conventions.

5. Construction of phenomena Few sciences confine themselves to the raw phenomena of nature. Instead conditions are established in which nature is placed under greater control or is made more reproducible, often with the result of creating situations which never occur otherwise. In ecology, field sites are selected, species or varieties transplanted into defined habitats, microcosms established in laboratories, multi-factor field trials conducted, and so on. While the construction of phenomena is often held to be a way to expose systematically the underlying reality of nature, nature does not tell us the ways we must construct. Moreover, any decision to perpetuate a constructed phenomenon, e.g., in waste treatment plants, forest plantations, or hedgerows, so that the phenomenon becomes a long-lived part of nature, is a social decision.

Of course, the choices in construction are influenced by our theories or models of causality, even if we have yet to state these explicitly (though see Hacking 1983 for reservations about theory ladenness). This does not, however, eliminate the sociality, because the range of competing models is socially negotiated (see 3 & 4 above).

6. Construction of observations The translation of phenomena into observations and data requires choices of categories, sampling frame (including the spatial and temporal extent of observations), and equipment to record the values. The design of equipment might be added to the earlier list of technical considerations (see 4 above). The other aspects of observation construction allow ample room for social considerations, such as available funding, to influence which models can be supported empirically.

Socio-ecological webs

At each of the six sites above, model building is open to negotiation. Referentiality is impossible without social decisions, even if these involve, for any particular group of scientists, taken for granted conventions. The view of sociality here is not that society writ large determines science; at each site a diversity of considerations can potentially influence the decisions. Across the different sites these influences are likely to show some interdependence, and so, co-opting historian of science Rosenberg's term "ecologies" of knowledge, we can think of the composite of all this sociality as a "socio-ecological web."

There is, of course, a way to assimilate this sociality back to a more conventional focus on referentialty. Decisions are made, but one might claim that those that persist and become conventions are those that have shown their value through the effectiveness of subsequent research and applications based upon them. This discounts social considerations; they are theoretical superfluous, at least they become so by the time the scientific community has reached a consensus or standardized practice (Taylor 1990). The argument from persistence also downplays the process of modeling, and instead revolves around the correspondence of its product to natural reality. Some evolutionary scheme is implied, analogous to that in which ideas that map reality best will best survive through experimental tests and disputes over correct interpretations.

The problem with such schemes for the evolution of ideas or conventions is that they leave unclear what scientists do. Unlike the process of genetic mutation, scientists surely do not vary randomly their ideas or their decisions at the sites of sociality. What are the processes through which scientists can bring about this "survival of the most effective?" Without such details, evolutionary schemes tend to collapse to a tautology of conceiving most effective as those persisting at any given point of time. The implied strategy of modeling, a distinctly non-Levinsian one, becomes simply to stay close to what is currently accepted.

Suppose, however, that we decide not to try to assimilate sociality to referentiality, but instead acknowledge that modeling operates within socio-ecological webs. How can we reconceive our strategy of model building? What do we have to do to change understanding of ecology as instantiated in accepted models and theories? Recall that Levins' exploratory modeling (as I called it) appeals to those who think that ecological theorizing can be stifled by an emphasis on hypothesis testing. Similarly, once we recognize what is omited by focusing on the validity of models and their underlying assumptions, we should pursue a wider examination of the sociality of the decisions modelers make. Or, at least, we should alternate looking "inwards" (to the referentiality of modeling) and "outwards" (to the decisions of modelers in their contexts). Through such examination we could trace how the social conditions in which knowledge is produced influences that knowledge.

Of course, the picture, although more detailed than previous accounts of modeling in ecology, is still quite abstract. Or, to use Levins' terms, I would claim that it is general and realistic, but would not claim that the picture is precise. The make-up, not just the existence, of the social negotiations and the implicated influences needs to be spelled out. What specific reactions take place at the sites of sociality? How do the different decisions enable modelers to secure the support of colleagues, collaborators and institutions, and enjoin others to act upon their conclusions? (Taylor 1992, 1995 tease out the heterogeneous influences in two case studies of modeling.)

Yet there is a prior, more serious task involved in developing a "web-sensitive" strategy of modeling. Modelers, or for that matter scientists in general, lack the experience, vocabulary, and motivation to open socio-ecological webs to systematic examination. My response to this challenge has been to attempt to develop particular researchers' experience and vocabulary directly, through workshops in which the participants “map” both the natural situations they are studying and the social situations in which they organize their research (Taylor and Haila 1989, Taylor 1990).

This essay might be seen as a further contribution to breaking down scientists' resistance to "web-sensitive" model building. I have argued that any account of modeling without attention to its sociality is insufficient. This means -- at least to the extent that I can appeal to scientists’ professed interest in faithful representations of reality -- that modelers should be debating decisions made at the various sites of sociality. Moreover, once we acknowledge the simultaneous referentiality and sociality of scientific practice and theorizing, the old dichotomies of realism and relativism, science and politics become meaningless. Attention to the the diversity of influences allows each of us to identify a multiplicity of possible interventions, with the virtue that many of them lie within our practical reach. Consistent with the spirit of Levins' strategy, contradictions proliferate and the provisionality of models remain central to the process of model building.

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