

The hidden complexity of simple models, or Why theorists of all kinds should be troubled by unmodeled variables having dynamical lives of their own

Peter J. Taylor

Critical & Creative Thinking Program

University of Massachusetts, Boston, MA 02125

peter.taylor@umb.edu

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Abstract

Can ecological theory generate principles that could be usefully generalized across ecological situations? Particularism has been a perennial attraction in ecology, but a new source of doubt gained momentum by the end of the 1980s after theorists started looking at “indirect interactions”—effects mediated through the populations not immediately in focus, or, more generally, through “hidden variables” that have their own dynamics. How much do indirect effects confound principles derived on the basis of observing the direct interactions among populations? My exploration of this question should challenge not only ecologists, but theorists in all fields that make use of models of any kind of sub-system elevated from the complexity in which the sub-system is actually embedded.

Like many, I came to work with Richard Levins and Richard Lewontin because they spoke of their science and politics productively informing each other (Lewontin 1979). Of course, to be open about connecting these two domains troubles the boundary patrollers of science (Gieryn 1983). They tend to give trouble back—many of L&L's students have been kept on a long march to find fields, institutions, funding sources, and publishers amenable to their work. Looking back on my own journey, the red threads I see are exploring the complexity of relationships between knowledge-making and influencing change and problematizing well-accepted boundaries. In this essay, I present previously unpublished work undertaken while a student of L&L on the consequences of making boundaries between the outside and inside of a system (Taylor 1995, 199-177; see also Taylor 2002). The issues raised have, I believe, relevance beyond the specific context of ecology.

I. Why were half the interactions in a community of competing protozoans predator-prey relations?

Vandermeer (1969) reported on a quantitative study of a community of four competing ciliate protozoan populations. The model he fitted to his observations indicated that three of the six pairs of interactions between the competitors were positive-negative (figure 1). One would expect this of predator-prey relations, not of competitive interactions. Were these interactions actually predator-prey? Indeed, were those pairs with negative-negative interactions actually competitors? How can the values Vandermeer derived be understood and related to the actual ecological relationships among the protozoan populations?

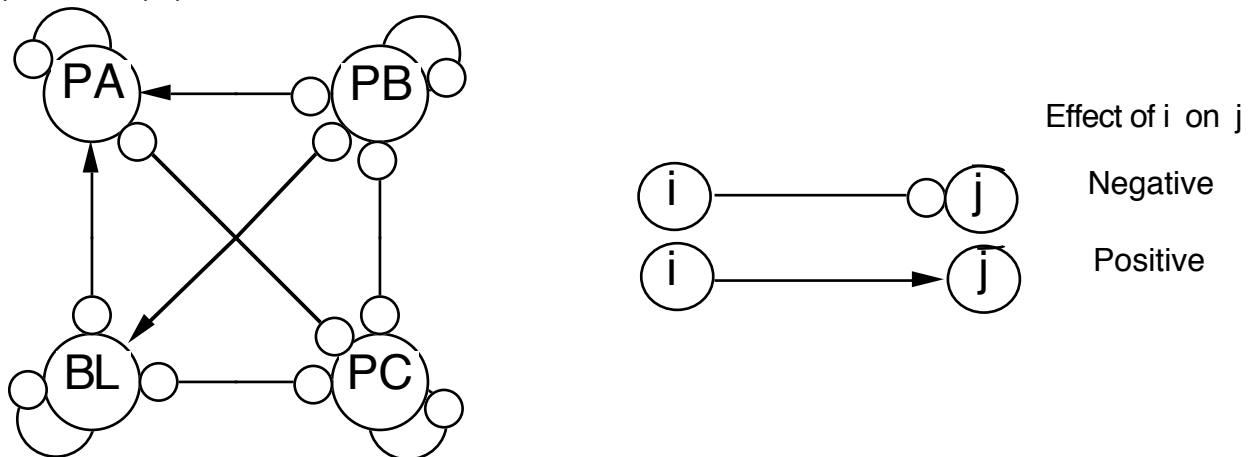


Figure 1. Community interactions reported by Vandermeer (1969). PA = Paramecium aurelia, PB = Paramecium bursaria, PC = Paramecium caudatum, BL = Blepharisma sp.

An obvious response might be that Vandermeer's model was inappropriate or inadequate, so let me examine this first. The inter-population interaction values he derived for his four protozoan species came from fitting the observed population trajectories to a model of the following form:

Model 1: Generalized Lotka-Volterra (GLV)

Per capita rate of change of population $X =$

Intrinsic growth rate for $X +$

Self-interaction within the X 's $+$

Sum of interactions of the other populations on X ;

where the first term is a constant, the second is a constant times the size of population X , and the inter-population terms are constants times the sizes of the other populations.

He estimated the intrinsic growth term and self-interaction term from isolated population growth experiments, and his inter-population interaction terms from two-population experiments. Contrary to the widely held opinion that the GLV is a poor ecological model, the fit for Vandermeer's four-population microcosms was fairly good and gave qualitatively correct predictions about coexistence of populations (Vandermeer 1981).

Given that Vandermeer's model fits his observations well, one needs to look further to explain the anomalous (-+) interaction values between the competing protozoans. First note that Vandermeer's equations did not specify all the components of the community. Each day during his experiment he removed a sample from his experimental tubes and added an equal volume of culture medium with bacteria. The bacterial populations were alive and able to grow until consumed by the protozoa. They had dynamics of their own not referred to in the equation above. In fact, it is possible that the protozoan populations were affecting each other only through these shared bacterial prey. If all the fitted interactions had indicated competition, the unspecified components might not have caused me any concern—the protozoan populations could be described as exploitative competitors. Yet the interactions were not all competitive.

Notice that the observed behavior of the protozoan sub-community—the full community minus the bacteria—was fitted with a model containing interactions only within the sub-community. Because there was no direct reference to the relationships with the hidden part of the community, the fitted interaction values had to incorporate these other indirect relationships, if they existed. Let me call the fitted interactions *apparent* interactions and use this term whenever ecologists attempt to specify the ecological dynamics of a sub-community without explicit reference to the dynamics of the community from which it has been elevated. In practice, fitted interaction values might always be apparent interactions, because there will be components the ecologists do not know about or have no data on—

for example, larval and adult life stages will be lumped together, or decomposers or other components in the food web will be omitted.

The critical question is whether the distinction between direct and apparent interactions matters. Do apparent interactions deviate significantly from direct observations of interactions or from ecologists' intuition about plausible interactions among populations? Ecologists tends to think that the protozoan populations should be competitors because they share a food resource, but Vandermeer's study counters that idea. Can a more general conclusion be derived?

II. Apparent interactions in an 8-population food web

One way to examine the importance of distinguishing apparent and direct interactions is to explore a world of model communities, as follows. Let me take an all-knowing role and dictate the relationships among all the members of the (model) community. Then imagine ecologists who collect data on population sizes over time—trajectories—only for a subset of the full community in order to build a model. Because their model can include only the populations in the subset, whatever interaction values they fit to the model will be apparent interactions, which combine both the direct effects between modeled variables and the indirect effects mediated through the hidden variables. These ecologists, however, are skillful at curve-fitting, so that the trajectories predicted by the sub-community model mimic the actual ones as well as possible. Now, using my all-knowing position, I can compare the ecologists' results with "reality," that is, with the full model.

Actually, it is not quite that simple. The outcome of such a comparison would vary according to the form of the model used for the sub-community, and so it would be difficult to make any generalizations about such comparisons. I can circumvent this limitation, however, if the full model has a feasible (i.e., positive) equilibrium and I restrict the comparison to the trajectories close to this equilibrium. Whatever the original form of the model, it can be approximated well by the following form near equilibrium:

Model 2: Near equilibrium linear form (NEL)

Rate of change of population $X =$

Self-interaction within the X 's +

Sum of interactions of each of the other populations on X ;

where the first term is a constant times the deviation of population X from its equilibrium value, and the inter-population terms are constants times the deviations of the other populations from their equilibrium values.

Suppose I use model 2 to generate the population trajectories for the full community—strictly, the near-equilibrium approximation to the trajectories—and ecologists use model 2 to mimic the populations in the sub-community only. I can calculate the exact values for the full model and the ecologists can find values that give the best fit. When these values are compared, the ecologists' model necessarily mimics the actual trajectories imperfectly. Despite their limited information, however, their version of model 2 can sometimes fit quite well.

Consider the model community shown in figure 2, consisting of 3 plants, 2 herbivores, and 3 omnivores, and governed by a stable GLV model. Suppose the ecologists restrict their attention to the sub-community of the consumers—that is, they omitted the plants. The trajectories for the five populations oscillating towards the equilibrium generated by the near-equilibrium approximation to the full model and the ecologists' best fit to this are given in figure 3. The figures are clearly very similar.

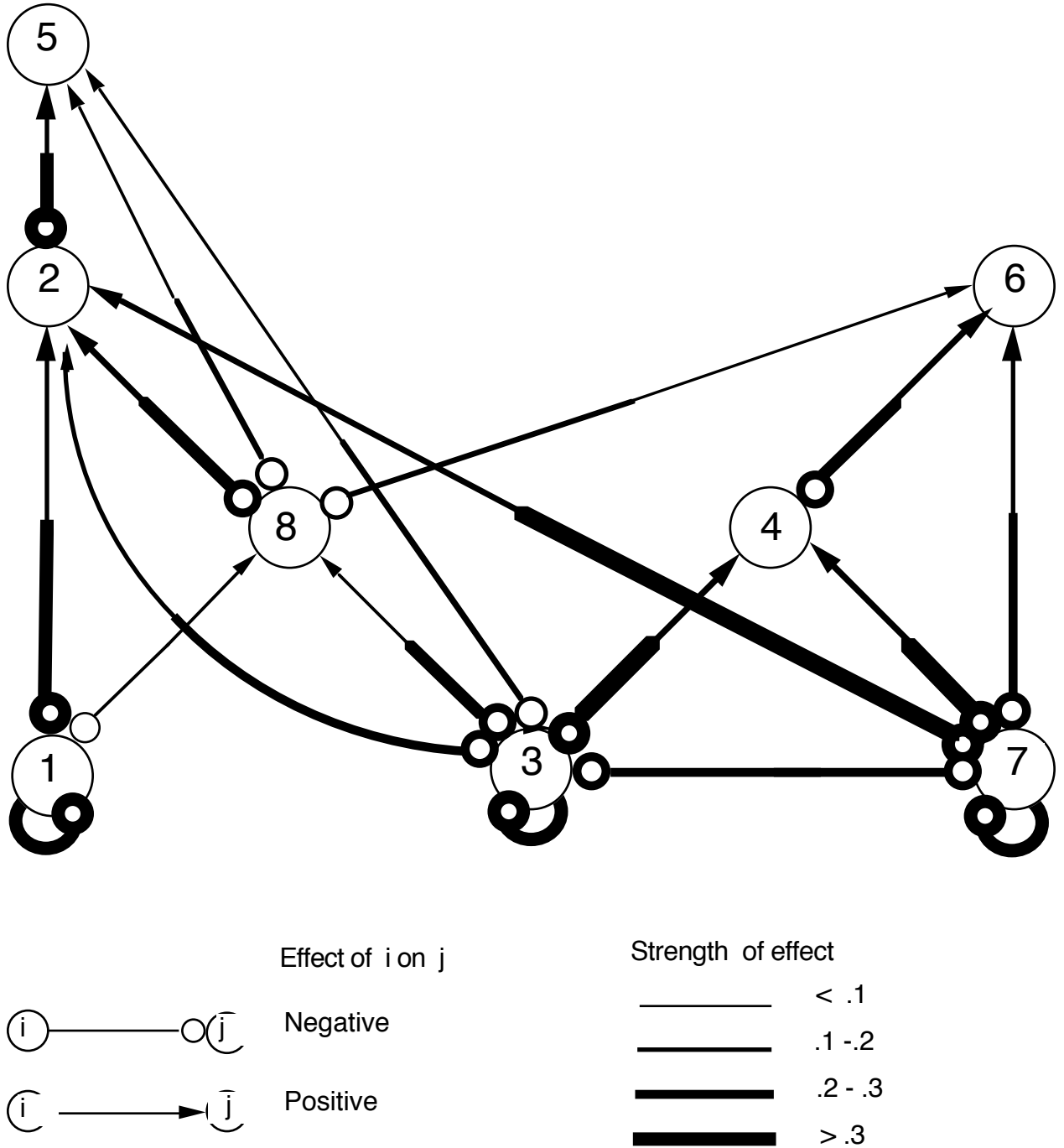
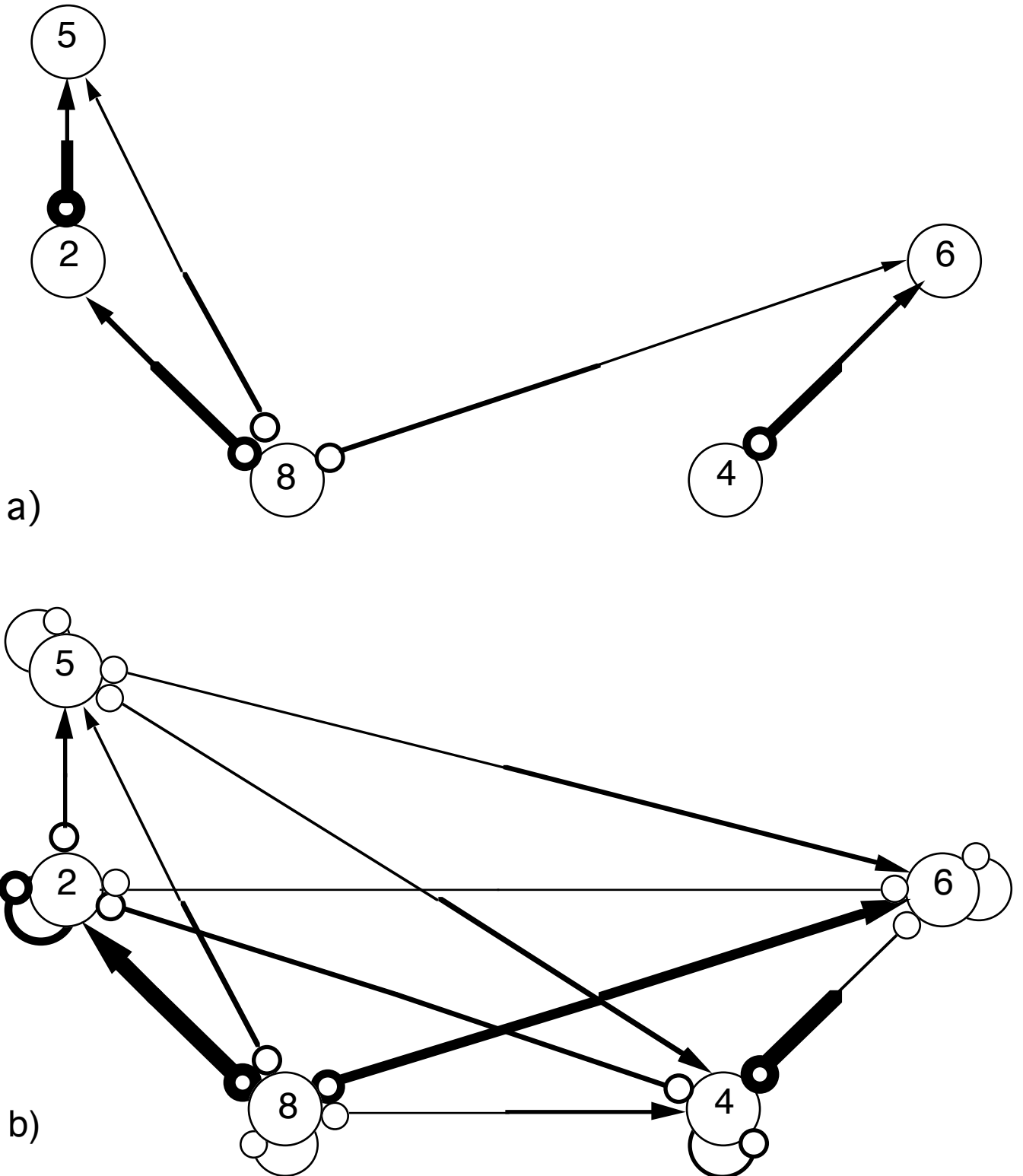


Figure 2. Eight species model community. (Symbols as in Figure 1)

--insert figure 3 here -- (Trajectories near equilibrium for the consumer sub-community, as determined by a. the direct interaction values for the full community; b. the apparent interaction values for the subcommunity alone.)

Now compare the actual interaction values and the ecologists' apparent interactions (figures 4a and 4b). The apparent interactions include each consumer self-inhibiting; omnivore 6 and herbivore 4 preying on top consumer 5; omnivore 2 competing with both 4 and 6; and herbivore 4 both competing with its predator 6 and being the prey of another herbivore, 8. In addition, there are apparent interactions matching in sign the direct predator-prey interactions of the actual community. Whatever intuition one has about the effects of the hidden resources—the plant populations—it would not, surely, include a top consumer being a prey of lower consumers. Yet such are the apparent interactions the ecologists would find best mimicked the trajectories they observed.



Before exploring the implications of this result, the following two slightly technical points should be noted (Taylor 1985, 119-176):

1) The best-fitting apparent interaction values vary depending on the initial sizes of the population deviations from equilibrium. When the exercise above is repeated over a random sample of starting points, the average values and spread around these averages can be calculated. It turns out that some of the counter-intuitive values disappear in the averages, but not all do.

2) When more variables are hidden, the fit between the ecologists' model and the actual trajectories becomes less satisfactory—as evident in figure 5—even though it is the best they can do. Furthermore, over a range of starting points to the trajectories, the spread of values around the average is relatively greater when more variables are hidden.

--insert figure 5 here --(Trajectories near equilibrium for the higher-consumer sub-community, as determined by the apparent interaction values for that sub-community alone.)

In summary, apparent interactions have the following characteristics:

- a. They can be counter-intuitive; yet
- b. they mimic well the trajectories of the populations.
- c. The spread of the estimated interactions increases as the range of starting points is enlarged; and
- d. the fidelity of fit decreases as more variables are hidden.

In section IV I tease out some broader implications of these results, but first I briefly note the challenge that apparent interactions pose for users of loop analysis and other techniques that relate changes in the equilibrium value of one variable to another.

III. Apparent interactions, indirect effects, and loop analysis

Suppose that an ecological community has a feasible equilibrium, but that the equilibrium population sizes readjust to new values as the conditions under which the community operates change. If ecologists assume that their observations of population sizes at different times are actually observations of population equilibria under different conditions, they can define the effect of one population on another in terms of their relative equilibrium values under the changing conditions—do they go up or down together, in different directions, or remain unchanged? This relation between two populations combines direct and indirect effects, because it builds in the interactions among all the populations, not only the two in focus.

Levins' loop analysis is the earliest and most general method of using the terms of the NEL (model 2) to calculate such effects. His method is as follows. Suppose the change in conditions of the

community—or, more generally, of any dynamical system—can be expressed as a change in some parameter, C , that directly affects the rate of change of one of the populations, X . The change in the equilibrium values of each of the populations can be calculated using a complicated expression involving all the interaction and self-interaction terms in Model 2 and the partial derivative of the rate of change of X with respect to C (Levins 1975, 40). This can be calculated exactly (a Fortran 77 program to do so is available on request), even though loop analysis customarily uses the sign of the interaction terms only and generates qualitative predictions (which may be indeterminate). Whether the two equilibrium populations change in the same or in different directions depends on the “node”—the population directly affected by a change in C .

In the context of a discussion of apparent interactions, the relevant question is whether hidden variables confound the values derived in loop analysis. This can be ascertained by calculating the changes in equilibrium populations using a sub-community only and comparing the results with those calculated using the full community. I investigated this question using the 5 population consumer sub-community (Taylor 1995, 150-153). First, I generated loop analysis results using the direct interaction values for the consumer sub-community. Unfortunately these were meaningless, because the predicted local stability was the opposite of what was correct. Hoping to overcome this problem, I followed conventional loop analysis protocol and added qualitative self-inhibitory values to represent the hidden resources (Lane and Levins 1977). The predicted changes were indeterminate in every case. Finally, I used the apparent interaction values summarized in figure 4b. Although the quantitative agreement was poor, the qualitative agreement was moderately good (Table 1).

Further investigation of the effect of hidden variables on loop analysis is needed, but these initial results suggest that apparent interactions might give qualitatively reliable loop analysis predictions, while direct interactions, even if supplemented by self-inhibitory interactions, do not. The loop analyst would need the direct interactions for the complete community or be able to substitute apparent interactions of the correct sign. But, as shown in section II, the sign of apparent interactions is not necessarily intuitive. What is to be done?

Table 1. Qualitative loop analysis predictions for the five species consumer sub-community

Node of change in C	Predicted change in equilibrium value of population size of species				
	2	4	5	6	8
2	+	+	-(+)	-	-
4	+	+	-	+	+(-)
5	-	-	+	+	+

6	+	+(-)	-	+	-
8	-	-	+	+	+

Figures in parentheses indicate predictions calculated using apparent interactions that differ from the correct values calculated using the complete set of interactions.

IV. Making sense of apparent interactions, a dialogue

Let me run through several different ways of making sense of apparent interactions, by means of a dialogue with the ecologists.

Ecologists (E): Are you sure that the surprising values we obtained were not an artifact of the simplifying assumption that the populations are approaching an equilibrium?

My response (P): Vandermeer's "predator-prey" interactions indicate that counter-intuitive apparent interactions are not restricted to near-equilibrium situations. Furthermore, by a continuity argument, the other characteristics (b-d) would hold even if you had derived the apparent interactions away from the neighborhood of an equilibrium. The only reason not to test you with an example away from equilibrium is that this brings in the additional problem of deciding the form of equations to use for the apparent community.

E: It cannot be the case that all omitted variables will have a strong influence on the modeled relationships.

P: Conceded. If the time scales of the components of the sub-community are much longer than those of the hidden variables (e.g. elephant vs. bacterial generation times), the hidden variables may equilibrate quickly and their effects on the sub-community may be constant (Göbber and Seelig 1975). Or, if the interactions within the sub-community are very strong, they may override the influence of the hidden variables.

Nevertheless, when you build models or formulate hypotheses to be tested, this state of quasi-independence of modeled variables from hidden variables ought to be demonstrated, and not simply assumed. Furthermore, you should not go out looking for cases that show quasi-independence—by virtue of interaction strengths or time scales—and then generalize this property to all sub-communities.

E: What information then do you think a well-fitting model can provide about actual ecological relationships?

P: The same method was used to derive the intuitive apparent interactions as the counter-intuitive, and so you should not try to give biological significance to the former and discount the latter. If Vandermeer's apparent predator-prey interactions do not require biological interpretation, then neither do his apparent competitive interactions. Noting that apparent interactions are sensitive to

context and to starting points, a good fit may simply mean that the hidden variables happened to remain within narrow bounds, or that a limited range of starting points was entertained. Goodness of fit, achieved by Vandermeer's model and by your apparent interactions in the five-population consumer sub-community, does not, therefore, indicate that a model represents actual ecological relationships.

E: What else would you want in addition to goodness of fit?

P: You should ask for evidence, independent of that fit, that your model contains the full community—or at least contains all of a sub-community that is quasi-independent of other sub-communities. Furthermore, while it is rare in ecology to collect enough data to fit dynamic equations to them, this conclusion must apply equally well to the more common situation in which you merely assess the qualitative correctness of the model's predictions.

E: If the model fits well—whether or not it represents actual ecological relations—why not use it as a basis for predictions?

P: Some scientists judge a model by its predictive success and most would value a model that fits well over one that fits poorly. Note, however, that apparent interactions vary as the starting points of the trajectories vary. Suppose you have derived a well-fitting model. Even then, if subsequently you were to observe trajectories beginning with different starting points, the fitted parameter values would change, perhaps even qualitatively. This sensitivity would especially be the case if the original fit were for a narrow range of starting points or for one replicated starting point, which was the case in Vandermeer's experiments. Similarly, a well-fitting model might no longer fit so well if the composition of the hidden variables were to change. Conversely, any predictions for the changed circumstances based on the original parameter values could be poor. You should, therefore, specify the range of circumstances in which the fit was derived, and recognize that beyond this range a well-fitting model is an uncertain basis for prediction.

E: There will always be hidden variables, except in experimentally controlled and isolated systems. Pragmatically, why not eliminate the distinction between actual and apparent ecological interactions—that is, simply define the effect of one population on another to be the values we fit to a biologically sensible model? Most ecologists already do this in some situations—the concept of exploitative competition, for example, is explicitly one of apparent interaction: the shared resources are the hidden variables.

P: If you adopt this approach you should also note that apparent interactions can be counter-intuitive. It follows that, when formulating models and fitting them to the data, you should not incorporate constraints on your parameter values to make the models appear biological. In contrast, Vandermeer, for example, assumed that intrinsic growth terms had to be positive and self-interaction values had to be negative. Paradoxically, he did not insist that the inter-population interaction values—the pairwise interactions between the protozoans—had to be negative. I would suggest allowing all parameters to be free, within the form of the model chosen, to take any value and sign.

Furthermore, even if you keep the parameters of your models intuition-free, you could not expect the influence of hidden variables on these parameters to be constant over time or independent of the values of the hidden variables.

E: All models simplifications or caricatures. We expect any model to depart from reality, and we expect these departures to guide us in subsequent improvement through the incorporation of additional biological detail. If we allowed concerns about hidden variables to inhibit the formulation of models, “there would be nothing to modify and we should get nowhere” (Hutchinson 1978, 40).

P: The rationalization that all models are caricatures is weak. You have seen that a well-fitting model may require counter-intuitive parameter values in order to incorporate the effects of hidden variables. As a corollary, you should not translate your simple verbal models directly into mathematical terms. You cannot assume, for example, that exploiting a shared resource in a community is well represented by a negative-negative interaction in the sub-community consisting only of those “exploitative competitors.” Correspondingly, theory based on simple models, e.g., competitive exclusion demonstrated on two-population phase diagrams, becomes problematic. In short, there is a hidden complexity to simple models.

Furthermore, unless you know that the model contains the full community, you cannot argue that lack of fit or counter-intuitive parameter values signify that some biological feature is missing from the model. For example, the stated purpose of Vandermeer's study was to detect “higher-order” interactions. By modeling only a sub-community, however, he could not resolve issues about the actual biology. Additional terms may have improved the fit of Vandermeer's model, but it is not warranted to describe this as improving the model by the addition of biological detail.

Although this conclusion was derived using the sparsely parameterized GLV model, hidden variables might also confound models that include a large number of parameters—for example, parameters related to the behavior and physiology of individual organisms, or to the flows of nutrients through ecosystem compartments. The conclusion also has relevance for debates about models based on deterministic relations, such as consistent inter-population interactions. If the fit to observations of such models is not significantly better than that of corresponding “null” models (Strong et al. 1984), the current convention is that the existence of the deterministic relations has been called into doubt. A plausible alternative hypothesis, however, is that model specification is incomplete and the relations exist, but are confounded by hidden variables (Schaffer 1981).

E: Are you saying that ecological models need to include every bit of detail or else they are, strictly speaking, biologically irrelevant?

P: No, not every detail, but at least include variables having dynamics of their own.

E: Suppose we controlled these variables, and they became merely parameters, then we could get more biological mileage out of our models of sub-communities, right? We could, for example, try to redo Vandermeer's experiment keeping the bacterial populations more or less constant.

P: In principle, yes. Simple mathematical models have sometimes proven effective for laboratory microcosms (Williams 1972)—but not always (Mertz and McCauley 1980). This strategy would, however, sidestep the primary issue of models of non-laboratory ecological situations.

E: Sidestep? It is a standard scientific strategy to learn about the functioning of variables in a controlled situation and then to use this knowledge to help understand their functioning in a larger context (Bechtel and Richardson 1993).

P: Yes, it is a strategy, but an ambiguous one. Some scientists and philosophers of science like to see it as a way to expose or localize the mechanisms used by organisms, and from this to derive, at least in principle, their functioning. We can also think of it, however, as a strategy providing redescriptive heuristics. Heuristics draw your attention to a conjunction of certain factors and invite you to act as if these factors governed the process in question. In other words, you can use a heuristic to guide your work provided you know that they will break down when you apply them too far out of the domain in which they were derived. When a model fits quantitative data, but you doubt that it captures the actual causal relations, you could call this a redescriptive heuristic.

E: There seems to be a circularity in your strategy. To specify how far out of the domain—in our case, how much change in the hidden variables and starting points is too far—we would need to know more about the dynamics of the full community. And if we knew that, we would not have to restrict ourselves to a model of the sub-community.

P: I agree. The challenge seems to have two parts. First, you do not have to wait until you can write down a model for the full community, but instead can use redescriptive heuristics to guide you in formulating hypotheses, experiments, generalizations. At the same time, however, you need a way of questioning the scope of the heuristics. Any heuristic may turn out to have misguided you, and you do not want to go a long time without finding out. This challenge also holds for insights derived by exploring the qualitative behavior of simple models (Taylor 2000a, 2002).

E: Instead of playing around with fallible heuristics, why not avoid the problem by including all the variables, or at least, omitting fewer of them? It seems to us that systems ecologists are less likely to omit variables from their models because they trace the flow of nutrients and energy through an entire ecological system, especially through the decomposer components.

P: The full system consists not only of variables, but of their dynamic interrelations. Systems ecology, in my opinion, too readily translates measurements of co-varying variables into equations without elucidating the biological dynamics (Taylor 1988). Instead of trying to be all-inclusive, we need, I think, to find ways to use models heuristically, all the time checking that they are not applied too far out of the domain in which they were derived.

E: In this respect maybe there is something to be learned from Holling's (1978) idea of "Adaptive Environmental Assessment and Management." This assumes that the dynamics of any ecological situation will not be not fully captured by any model or composite of models, especially because

management practices produce continuing changes in those dynamics and make the ecological situation a moving target.

P: I agree. Ongoing assessment might allow us to correct for the confounding effects of unmodeled dynamics and take account of turnover in the components of the modeled system. But it is an open question exactly knowledge production can be reorganized in ecology to use models heuristically, build in ongoing assessment and revision, and accommodate management and exploitation by humans. It is a question for us to address together. In the meantime, let me summarize what apparent interactions show you about the strategy of searching for basic principles about the interactions of a subset of the ecological community and building up a picture of ecological complexity from these: Unless you know that the full community has been specified, a model is primarily a redescription of the particular observations which does not provide, through its fit or its lack of fit, sure or general insight about actual ecological relationships (Taylor 2000a). If this conclusion seems too extreme to accept, it should at least challenge you to clarify your reasons—sociological as well as scientific—for building models that refer only to a few populations when those populations are embedded in naturally variable and complex ecological situations. Simple models mostly hide their complexity.

E: Hold on. Your points must also apply to other fields, such as economics and social theory, when their models omit explicit reference to variables that have dynamics of their own.

P: Yes, I think theorists of all kinds should be troubled.

V. Coda: Intersecting Processes

My work on apparent interactions was, for the large part, a critique of theory that assumes well-bounded systems. Given that this is the case for most mathematical ecology, it is not surprising that a journal reviewer dismissed my analysis as presenting an “insoluble problem.” I did not try to tame the argument about apparent interactions for ecologists, but moved on. In retrospect, I see that I have taken the insolubility of the problem as a starting point. The picture of “intersecting processes” (Taylor and García Barrios 1995) that I arrived at is best conveyed through a brief case study and discussion (extracted from Taylor 2000b).

In the mid 1980s resource economist Raúl García-Barrios, and his ecologist brother, Luis, studied severe soil erosion in a mountainous agricultural region near San Andrés in Oaxaca, Mexico, and traced it to the undermining of traditional political authority after the Mexican revolution (García-Barrios and García-Barrios 1990). The soil erosion of the twentieth century is not the first time this has occurred in this region of Oaxaca. After the Spanish conquest, when the indigenous population collapsed from disease, the communities moved down from the highlands, abandoning terraced lands, which then eroded. The Indians adopted labor-saving practices from the Spanish, such as cultivating

wheat and using plows. As the population recovered during the eighteenth and nineteenth centuries, collective institutions evolved that reestablished and maintained terraces and stabilized the soil dynamics. Erosion was reduced and soil accumulation was perhaps stimulated. This type of landscape transformation also needed continuous and proper maintenance, since it introduced the potential for severe slope instability. The collective institutions revolved around first the Church and then, after independence from Spain, the rich Indians, caciques, mobilizing peasant labor for key activities. These activities, in addition to maintaining terraces, included sowing corn in work teams, and maintaining a diversity of maize varieties and cultivation techniques. The caciques benefited from what was produced, but were expected to look after the peasants in hard times. Given that the peasants felt security in proportion to the wealth and prestige of their cacique and given the prestige attached directly to each person's role in the collective labor, the labor tended to be very efficient. In addition, peasants were kept indebted to caciques, and could not readily break their unequal relationship. The caciques, moreover, insulated this relationship from change by resisting potential labor saving technologies and ties to outside markets.

The Mexican revolution, however, ruptured the moral economy and exploitative relationships by taking away the power of the caciques. Many peasants migrated to industrial areas, returning periodically with cash or sending it back, so that rural transactions and prestige became monetarized. With the monetarization and loss of labor, the collective institutions collapsed and terraces began to erode. National food pricing policies favored urban consumers, which meant that in Oaxaca corn was grown only for subsistence needs. New labor-saving activities, such as goat herding, which contributes in its own way to erosion, were taken up without new local institutions to regulate them.

Although my synopsis of the García-Barrios brothers' account is brief and smoothed out, it is sufficient for me to identify several characteristics of an intersecting processes viewpoint on social-environmental change:

1. Differentiation among unequal agents: Sustainable maize production depended on established relationships between cacique and peasants, and the inequality among these agents resulted from a long process of social and economic differentiation. Similarly, the demise of this agro-ecology involved the unequal power of the State over local caciques, of urban industrialists over rural interests, and of workers who remitted cash to their communities over those who continued agricultural labor.

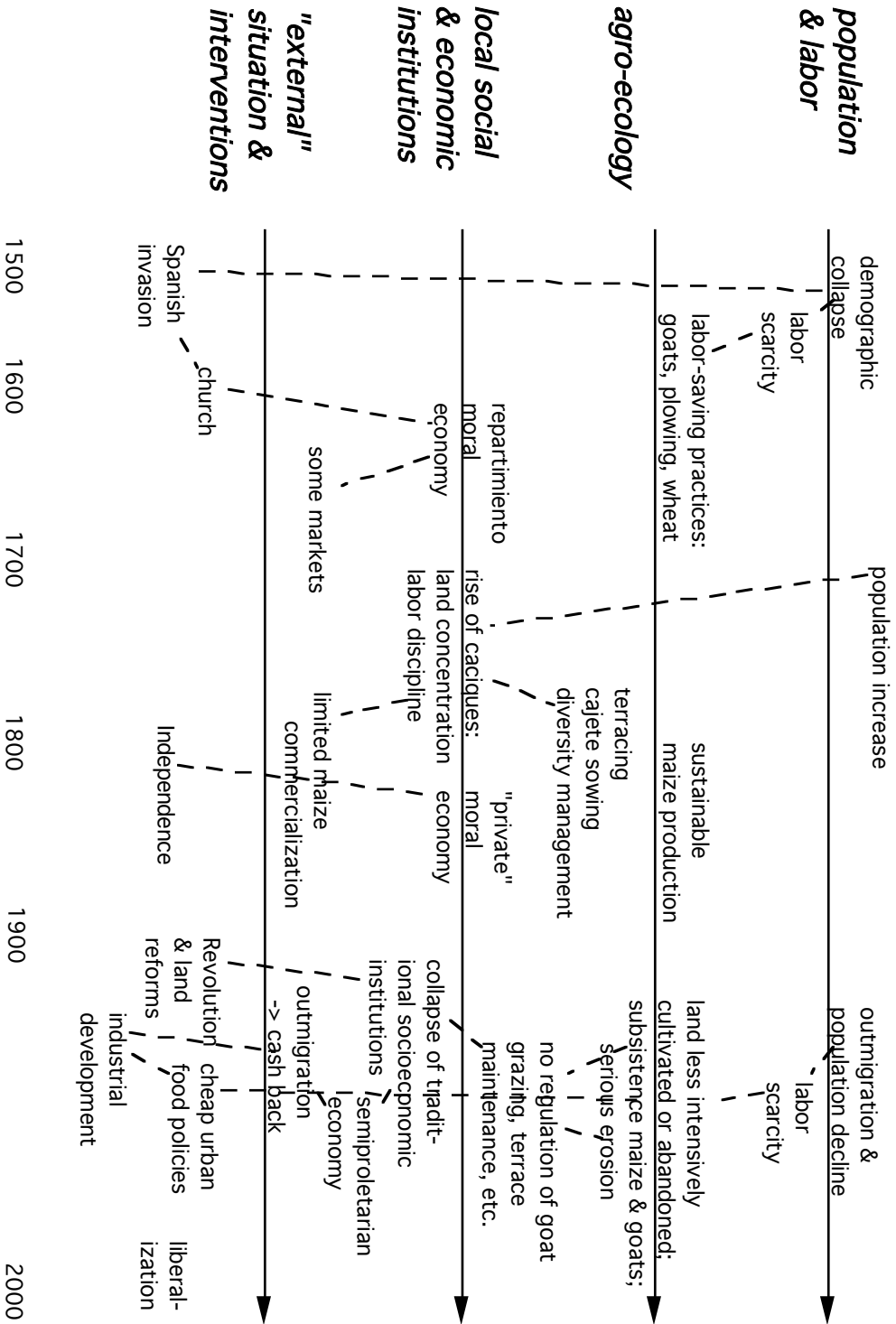


Figure 6. Intersecting processes leading to soil erosion in San Andrés, Oaxaca. The dotted lines indicate connections across the different strands of the schema. See text for discussion.

2. Heterogeneous components and inseparable processes: As highlighted in figure 6, the situation arises from intersecting processes operating at different spatial and temporal scales, involving elements as diverse as the local climate and geo-morphology, social norms, work relations, and national political economic policy. The processes are interlinked in the production of any outcome and in their own on-going transformation. Each is implicated in the others, even by exclusion, such as when caciques kept maize production during the nineteenth century insulated from external markets. No one kind of thing, no single strand on its own, could be sufficient to explain the currently eroded hillsides. In this sense, an intersecting processes account contrasts with competing explanations that center on a single dynamic or process, e.g., climate change in erosive landscapes; population growth or decline as the motor of social, technical, or environmental change; increasing capitalist exploitation of natural resources; modernization of production methods; or peasant marginalization in a dual economy.

3. Historical contingency of processes: The role of the Mexican revolution in the collapse of nineteenth-century agro-ecology reveals the contingency that is characteristic of history. The significance of such contingency rests not on the event of the revolution itself, but on the different processes, each having a history, with which the revolution intersected.

4. Structuredness: Although there is no reduction to macro- or structural determination in the above account, the focus is not on local, individual-individual transactions. Regularities, e.g., the terraces and the moral economy, persist long enough for agents to recognize or abide by them. That is, structuredness is discernable in the intersecting processes.

5. Distributed agency: The agency implied in the account of the García-Barrios brothers was distributed, not centered in one class or place. In the nineteenth-century caciques exploited peasants, but in a relationship of reciprocal norms and obligations. Moreover, the local institutions were not autonomous; the national political economy was implicated, by its exclusion, in the actions of the caciques that maintained labor-intensive and self-sufficient production. Although the Mexican revolution initiated the breakdown in these institutions, the ensuing process involved not only political and economic change from above, but also from below and between—semi-proletarian peasants brought their money back to the rural community and reshaped its transactions, institutions, and social psychology.

6. Intermediate complexity: The García-Barrios brothers include heterogeneous elements in their account, but, as my synopsis and figure 6 indicate, different strands can be teased out. The strands, however, are cross-linked; they are not torn apart. In this sense, the account has an intermediate complexity—neither highly reduced, nor overwhelmingly detailed. By acknowledging complexity, the account steps away from debates centered around simple oppositions, e.g., ecology-geomorphology vs. economy-society. Similarly, by placing explanatory focus on the on-going processes involved in the historically contingent intersections, the account discounts the grand discontinuities and

transitions that are often invoked, e.g., peasant to capitalist agriculture, or feudalism to industrialism to Fordism to flexible specialization.

7. Multiple, smaller engagements: Distributed agency, intermediate complexity, and the other features of intersecting processes have implications, not only for how environmental degradation is conceptualized, but also for how one responds to it in practice. Intersecting processes accounts do not support government or social movement policies based on simple themes, such as economic modernization by market liberalization, or sustainable development through promotion of traditional agricultural practices. They privilege multiple, smaller engagements, linked together within the intersecting processes.

Moreover, this shift in how policy is conceived requires a corresponding shift in scholarly practice. On the level of research organization, intersecting processes accounts highlight the need for transdisciplinary work that is grounded but not localized in particular sites. They would not underwrite the customary, so-called interdisciplinary projects directed by natural scientists, nor the economic analyses based on the kinds of statistical data available in published censuses. In short, the project of representing intersecting processes is inseparable from engaging with them; science is inseparable from politics.

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