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EDITORIAL

Controversies in community ecology and their consequences for ecological practice and funding allocation: a plea for common sense

Controversias en ecología de comunidades y sus consecuencias para el quehacer ecológico y la asignación de fondos: un llamado al sentido común

Community ecologists are a special breed of biologists who have chosen to study patterns and processes in systems of varied size, usually highly complex by virtue of the type and degree of interactions existing among sympatric species. This complexity does not stem only from the sheer number of interacting species, but from the variety of mechanisms operating, which do not line up neatly but usually form vicious loops and non-transitive networks.

None of these complications has discouraged ecologists from studying communities, although their approaches may differ considerably. I recognize four major approaches to community ecology. First, there are the mathematical ecologists that analyze the behavior of models that mimic simplified communities, but in my impression they are more into mathematics than into ecology. Second, there are the experimentalists, who manipulate simple systems with the aim of identifying the processes (say, competition, predation, competitive mutualism) that underline apparent patterns. Third, there are the observationalists, who scrutinize complex systems not amenable to non-destructive manipulation, and based on niche theory attempt to infer the processes that underline the presumed patterns. Fourth, there are the "nullists", who claim that prior to exploration of deterministic causation, ecologists should first falsify the hypothesis that observed patterns are stochastically produced. Because observationalist ecologists have generally been biased toward looking for evidences of the operation of a single process, namely competition, they have been labeled "competitionists" by irate opponents.

The opponents to competitionism (and by unfortunate extension, to observationalism) are from two different schools. Historically the first opponents are the experimentalists, whose leading personalities claim that predation, not competition, is the key organizing factor of communities. A more recent trench has housed the nullists, which upon statistical re-analysis (aided by simulation) of reputedly competition-structured assemblages, frequently find no ground for the claims of competitionists.

Contrary to the experimentalists (who can show positive evidence for their claims), the nullists' stance seems to be mainly negative. If a certain pattern in a local assemblage (say, the distribution of body sizes) has been claimed to be shaped by competition, they first simulate a colonization model with non-interactive species, then statistically examine the resemblance of the non-interactive model to the purportedly competition-structured assemblage, and finally, upon finding no significant differences they conclude that the assemblage has no structure at all (that is, many randomly operating causes may have acted, or none at all).

Because competitionism has (misguidedly) been equated to observationalism, attempts to pursue this latter line of research, even when clearly justified by the caliber of the study system, have been facing bitter criticism by ecologists from the other two schools (see for example Salt's 1983 "roundtable in ecology", Strong *et al.* 1984). Observationalists (and their radical wing of competitionists) have replied with commensurate energy, and all this has lead to a nasty confrontation, with plenty of name-calling (see for example the chapter by Gilpin & Diamond versus that by Connor & Simberloff in Strong *et al.* 1984). As it frequently hap-

pens, the controversy has generated a lot of heat and obfuscation, and a comparatively minute amount of light.

I recently became a victim of such obfuscation from a reviewer, who nearly deprived me of much needed research support. I requested funds for a project to conduct a comparative analysis of trophic relationships in two complex assemblages of vertebrate predators (including hawks, owls, snakes, and carnivores) living in similar habitats of Chile and Spain. By quantitative procedures I was going to assess the trophic guild structure in these two regions. Because of the differing species richness in the Chilean and Spanish localities (11 and 25 predator species, respectively), their comparison was intended to explore how the structure of assemblages reflects an increase (or decrease) in the number of coexisting species. The information obtained in the species-poor (Chile) and the species-rich (Spain) locality —apart from its inherent descriptive value— was going to be used to answer three fundamental questions about the causes, correlations, and consequences of guild structure: 1) Is it caused by diffuse competition over homogeneously distributed resources, or it merely reflects natural gaps in resource space? 2) Do more diverse communities have more guild structure than simple communities? 3) What are the effects of guild structure on the assembly, overall structure, and diversity of communities?

The approach of this project is outright observationalist, in the tradition of niche theory and community structure as practiced by —for example— Pianka *et al.* (1979). I was not a bit worried about this, because I thought that although in the past few years this approach was hotly contested, it now appeared that dust had settled and there was no reason to be ashamed of being one of those researches that first searches for patterns and then infers processes (see Salt 1983, Giller 1984, Strong *et al.* 1984).

This approach, however, bothered my reviewer to the point of obfuscation. He claimed that "the study, if funded, will produce a large quantity of data that can be summarized and manipulated in a variety of ways. But they will remain essentially descriptive, and the approach is largely a 'fishing expedition' looking for patterns". In my modest opinion, such a reviewer (which I take as being representative of a very radical position among community ecologists) fails to see that so far nobody (that I am aware of) has ever attempted this sort of search for patterns in complete assemblages of predatory vertebrates (comprising species in several classes). Indeed, there will be no replicates, and the comparison between Chile and Spain will involve a sample size of two assemblages only. It is here that I ask for some sympathy with my position: Definitely, I cannot increase the sample size. Although I plan to work with two assemblages, their analysis involve quantification of the diets of 36 predator species, and this represents scrutiny of thousands of prey items!

As an observational community-ecologist I was also annoyed with the following complaint by the same (type of) reviewer: "What will it all mean with so many hidden assumptions being made (about the meaning of the indices used) and with a complete lack of experimental controls?". As if my study areas, covering thousands of hectares were equivalent to boulders in the intertidal zone! Even if I could set a replicate study area, I would not effect the removal (with a machine-gun I presume?) of scarce, sometimes endangered top predators, just to have a "control" area. Model organisms and model systems have certain biological peculiarities that sometimes limit the application of experimental procedures. Although I heartily agree that clever experimentation is the major way to assign causality among processes, I also believe that there are cases in which hypothesis testing has to be done through other avenues of inquiry.

And not all testing protocols have to be experimental. Statistical exploratory analyses along the vein of "neutral models" (Caswell 1976) or "null hypotheses" (see Strong *et al.* 1984, and references therein), may suffice in systems not amenable to manipulation. In my opinion, the top-predator assemblages I plan to study qualify for an exemption of the requisite of experimental manipulation, and this is the reason why I dared request funds for a largely inferential approach to the identification of processes behind ecological patterns.

The only point I wish to make in this editorial is that there is no standard protocol —much less a magic recipe— for

community analysis. Some model systems are indeed amenable to experimental manipulation, some others are not. It is the system and the general question asked that dictates the protocol to be used. Failure to recognize this elementary fact by ecologists engaged in a holy war to impose their own view of how research in community ecology should be practiced, do a great disservice to ecology as a growing science. Obfuscation and single-mindedness can only retard progress, and I plea here for an eclectic and commonsensical approach to the study of ecological communities. If an open (but not uncritical) stance to all reasonable avenues of scientific inquiry into community ecology does not prevail, project-funding will become a matter of luck: of whether one's proposal is sent to ardent competitionists, experimentalists, or nullists. I was lucky that my project was sent to ecologists in opposing trenches, and the one who sided with me wrote very well. The practice of ecological science should definitely not depend on such vagrancies.

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FABIAN M. JAKSIC

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