

Annotated Bibliography

The Issue of Scale in Questions of Community Interaction and Composition

All of the following papers share one thing: the idea that “scale matters”.

I have included papers that summarize the historical role of scale in ecology, attempt to apply concepts of scale into their research, overcome the problems associated with scaling and even papers that develop ways to find your one true scale (of your study system that is).

John Doudna

Schoch, G.C. and M.N. Dethier. 1996. Scaling up: the statistical linkage between organismal abundance and geomorphology on rocky intertidal shorelines. *J. of Experimental Marine Biology and Ecology*. 201:37-72.

The author’s goal was to determine statistical relationships between species abundance and physical factors to extrapolate from fine-scale studies to large spatial scales if geomorphology is known. They claim that there are strong statistical relationships within a given geomorphological classification, and therefore statistical inferences can be made over large scales from fine-scale data collection.

Wiens, J.A. 1989. Spatial scaling in ecology. *Functional Ecology*. 3:385-397.

This is another review. It is the earliest, and therefore least comprehensive, review I found. Wiens outlines some issues in ecology that are on ever-increasing scales, briefly addresses just what scale is, and then cites some reasons scale could be a problem as well as how we should deal with it. The main line of thinking seemed to be, “we need to figure out whether this is a big deal”.

Melbourne, B.A. and P. Chesson. 2005. Scaling up population dynamics: integrating theory and data. *Oecologia*. 145:179-187.

The authors develop a systematic approach to scaling up population dynamics. They give four steps: derive a model to translate from local dynamics to regional scale, determine nonlinearities at local scale, measure spatial variation, and combine the nonlinearity and variation measures to obtain scale transition. Their study system is caddisfly larvae in stream riffles. They develop average density for riffles, and translate to regional scale with a factor which equals the difference between the stream-scale model and the mean-field, which they argue arises due to an interaction by nonlinearity and spatial variation. They use models and field data to support their claims. This may have potential for use in future studies.

Fuhlendorf, S.D., A.J.W. Woodward, D.M. Leslie Jr., and J.S. Shackford. 2002. Multi-scale effects of habitat loss and fragmentation on lesser prairie-chicken populations of the US Southern Great Plains. *Landscape Ecology*. 17:617-628.

This paper presents an empirical example of how examining only one scale will not give the whole picture. They found that different variables were important for the maintenance of the population depending on the scale they examined. Their use of the term scale refers to how far away from the lek they collected data on vegetation in various radii. Average vegetation seems to be quite different at different radii. This could be an example of why we need to be concerned

with scale, or it might be an example of why we should just know more about the biology and life history of the organisms we study.

Habeeb, R.L., J. Trebilco, S. Wotherspoon, and C.J. Johnson. 2005. Determining natural scales of ecological systems. *Ecological Monographs*. 75:467-487.

This paper is a follow-up to the Pascual and Levin (1999) paper. The goal is to use the ideas developed earlier to determine natural scales, but not need such long-term datasets. They essentially substitute a combination of space and time replication for long time series, and get similar results. This paper presents the best potential for the determination of a natural scale that I could find.

Pascual, M. and S.A. Levin. 1999. From individuals to population densities: searching for the intermediate scale of nontrivial determinism. *Ecology*. 80:2225-2236.

This paper develops a procedure to determine the natural scale of a process. It tries to determine if it is possible to find the scale which “‘maximizes the ratio of ‘deterministic’ information to stochastic fluctuations,’ but also as one ‘above which distant parts of the lattice are acting independently.’ In other words they want to be able to determine the scale of the system that relates the best picture of the process. There is a lot of mathematical and physical jargon in this paper. The main idea, however, is readily interpretable and combines well with Habeeb et. al.(2005). There are major issues with the technique in terms of the magnitude of necessary data, which are addressed in Habeeb et. al.(2005).

Levin, S.A. 1992. The problem of scale in ecology. *Ecology*. 73: 1943-1967.

This was a MacArthur Award Lecture, and therefore is a personal essay on the part of the author expressing his views on the importance of pattern and scale in ecology. He summarizes some of the issues that have been developed by other researchers and presents the problem of transition between scales. He also applies the concepts to other current theory such as metapopulation models, food webs, and global climate change. Levins argues that the “problem of relating phenomena across scales is the central problem in biology...”(p.1961). This paper is most useful as a summary of questions and an early example of scale being posited as central to ecology.

Inouye, B.D. 2005. Scaling up from local competition to regional coexistence across two scales of spatial heterogeneity: insect larvae in the fruits of *Apeiba membranacea*. *Oecologia*. 145:188-196.

Inouye combines data on the aggregation of larvae at the tree level with biogeographic dispersal data and competitive ability to explain species coexistence at the regional scale. I'm not sure why this is scaling up. It rather seems like he is saying that we need to be cognizant of the multiple scales on which things happen or patterns are seen, and we need to combine data from all of them. He finds that it makes good sense that these species co-exist, based on all the data, but it would not make sense if we looked on only one scale (so we could not have predicted it).

Petersen, J.E. and G. Englund. 2005. Dimensional approaches to designing better experimental ecosystems: a practitioners guide with examples. *Oecologia*. 145:216-224.

This paper acts as a guide for developing mesocosm systems using previously-defined methods (See Petersen and Hastings, 2001). They do talk about more specific manipulations and choices to be made by the practitioner. Could be useful for defending the use of mesocosm and microcosm studies.

Underwood, N., P. Hamback, and B.D. Inouye. 2005. Large-scale questions and small-scale data: empirical and theoretical methods for scaling up in ecology. *Oecologia*. 145:177-178.

This is an introductory paper which leads to the special topic on "scaling-up in ecology" found in the September 2005 issue of Oecologia. All of the other papers in this bibliography from Oecologia, 2005 issue:145 are from this special topic. In this short paper, they outline the reasoning and goals of this special topic.

Schneider, D.C. 2001. The rise of the concept of scale in ecology. *BioScience*. 51:545-553.

This paper has a wealth of information. It summarizes the history of the terms and concepts associated with scale in ecology and describes the major concerns of other researchers about why we should be considerate of scale. At a couple of points in the paper, Schneider alludes to the potential of scale theory as a unifying theory of ecology. He discusses the determination of whether a power law describes habitat structure. He follows from these ideas and lays out how we could determine if scale-divergent dynamics should be expected in the system. There is little explanation about what scale-divergent dynamics mean and I could find little elsewhere in the literature on the subject. Schneider makes a somewhat convincing case about the fact that there might be scale-associated dynamics driving systems.

Petersen, J.E. and A. Hastings. 2001. Dimensional approaches to scaling experimental ecosystems: designing mousetraps to catch elephants. *The American Naturalist*. 157:324-333.

This paper is an attempt to give guidelines for the rigorous development of mesocosm systems. The authors use techniques developed in engineering to claim that representative systems can be created on a much smaller scale. The authors argue that with unitless numbers (such as percent habitat used), or by offsetting one distortion with another (smaller patch, smaller organisms, with smaller effective home ranges), one can develop a mesocosm that can be directly related to natural systems. While the idea seems promising, and allows us to dream of strict rules such as those found in the physical science, the paper does little to convince one that this approach would hold for the ecological sciences, and it does little for the understanding of the problem of scale in ecology. Rather, it almost presents the opposite idea: that we can scale all we want, we just need to be careful.

Schmitz, O.J. 2005. Scaling from plot experiments to landscapes: studying grasshoppers to inform forest ecosystem management. *Oecologia*. 145:225-234.

This paper is an attempt at what I think of as “theoretical broadening”. The researchers have spent a lot of time studying spider-grasshopper-herb trophic interactions, and so they would like to apply their knowledge to broader-scale issues. And since a major issue in their study area is the protection of large herbivores and timber harvest, they applied their knowledge to this. They tried to apply the concept of a “landscape of fear” which they found in the invertebrate predator-prey interactions to the vertebrate interactions. They found that in both cases, stimuli from predators or fear of being eaten could change browsing patterns and alter plant composition in the community. They effectively applied knowledge from one system of interaction to another. While interesting, the paper does little to promote the importance of scale in ecology.