
On the Status of Restoration Science: Obstacles and Opportunities

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Abstract

Terrestrial restoration ecology is not as well developed as aquatic and wetland restoration. There are several key obstacles to progress in restoration ecology, but these obstacles may also be viewed as opportunities to exploit. One obstacle is demonstration science, or an overreliance on simplistic experiments with few treatment factors and few levels of those factors. Complex, multivariate experiments yield greater insights, especially when teamed with sophisticated methods of data analysis. A second key obstacle is myopic scholarship that has led to little

synthesis and weak conceptual theory. A greater awareness of and explicit references to ecological principles will help develop the conceptual basis of restoration science. Where should restoration ecology be headed? We should consider forming partnerships with developers, landscape artists, and industry to do complex, large-scale experiments and make restoration a more common part of everyday life.

Key words: demonstration science, multivariate experiments, partnerships, restoration, synthesis.

Introduction

What is the present state of restoration ecology? And where should the field be headed? When I was asked these questions, my initial impression was that restoration ecology is a messy, romantic, fragmented hodgepodge (i.e., a heterogeneous mass or agglomeration). The practice of ecological restoration is still more art than science, and this is reflected in the state of the science of restoration ecology, especially in terrestrial habitats.

My background includes lake restoration and water quality management, plant community ecology in lakes, wetlands, savannas, and prairies, and it includes some ongoing restoration experiments. I was asked to contribute to this discussion because of my work on assembly rules and community assembly theory (e.g., Weiher et al. 1998; Weiher & Keddy 1999), which is part of the conceptual foundation of restoration ecology (Temperton et al. 2004). My experiences working in lakes, wetlands, and grasslands have influenced how I view restoration ecology. Lake restoration and management is now a rather well-established form of applied ecology and water quality engineering (e.g., Cooke et al. 1993). Wetland restoration is nearly as well established, with an overriding emphasis on hydrology (e.g., Galatowitsch & van der Valk 1994; Middleton 1999). Restoration in terrestrial systems, however, is a comparatively qualitative endeavor, and it seems a bit more like gardening than engineering (e.g., Packard & Mutel 1997). These differences largely stem from the

increasing level of difficulty and inherent differences between lakes, wetlands, and terrestrial habitats.

I am not going to address general conceptual issues, such as the idea of communities as complex, self-organizing energy-dissipating systems, the importance of stability, inertia, and alternative stable states for restoration success, or the role of biodiversity in ecosystems. Rather, I am going to focus on the general status of the science of restoration ecology, several common obstacles to progress, and therefore several opportunities for exploitation.

A General Appraisal

I wanted to collect some data on papers in *Restoration Ecology* to help test the validity of my beliefs about the status of restoration ecology. I decided to focus on this journal partly because this is the flagship journal in restoration ecology and partly because similar journals have slightly different missions: *Ecological Restoration* balances science with management; *Natural Areas Journal* includes management, restoration, as well as nonapplied studies; *Ecological Applications* tends to address basic ecological questions in an applied setting that includes forestry and agroecology, as does *Applied Vegetation Science*.

I haphazardly sampled seven issues of *Restoration Ecology* (within 2002–2005) and scored papers for a variety of factors. In some ways, restoration ecology is doing well, and I found the quality of the science to be quite good. I found very few, if any, examples of unreplicated descriptive site restorations or remediations. About 55% of the studies included a factorial experiment of some kind (many were mixed models), but there were perhaps too many cases where pseudoreplication (Hurlbert 1984) was likely problematic (nearly 25% of experiments). I found

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that space for time replacement, that is, where sites of different ages are used as a proxy for a true time series, were not common (<10% of studies), whereas true time series studies were more common (50%), and these were often combined with factorial experiments. Of these, very few (if any) were unreplicated, single-site descriptions.

Not surprisingly, the papers were biased toward plants (nearly 90%), and the issues of regeneration and reestablishment (50%). Few studies focused on animals (10%), and even fewer focused on fungi or soil microbes (<3%). Most studies focused on the restoration of composition (i.e., particular species and species richness, 70%), whereas only a few addressed the restoration of structure (functional traits and evenness, <10%) or ecosystem function (15%). The median number of taxa was seven, whereas the mean was 25, and this suggests that few studies are focusing on one or two target species.

Obstacle 1: Demonstration Science

The term “demonstration science” has been used by ecologists to denote experiments that seek to demonstrate that a causal agent has an effect on some item of interest. In a sense, demonstration science is the bread and butter (or the white rice) of science. Demonstration science is used to experimentally establish causal relationships. This sounds well and good, but the problem comes when we seek to demonstrate well-established and well-known causal relationships and then pass this off as some kind of advancement. Wilson (1995) used the term the “Jack Horner Effect” for a similar notion (Jack Horner is a character in a children’s story who is proud of himself for finding and extracting a plum from a plum pie). Demonstration science turns us into a collection of Jack Horners.

I think we need to realize that much of what we do in experimental restoration ecology is indeed demonstration science. In the papers I surveyed, the median number of factors examined was one, and the median number of treatment levels was two. The typical experiment therefore included only a treatment and a control, and so it was minimally designed to demonstrate that a treatment had an effect.

How do we move away from demonstration science? If we increase the number of treatment factors, we gain greater understanding of their effects relative to one another, and we can learn about interactions. Another way is to design experiments with more than two levels of a treatment, that is, more than a singular treatment and a negative control, such as seeding and no seeding. Experiments that involve multiple levels of a treatment move the design toward a gradient approach, that is, a series of treatment levels, such as varying the number of seeds per square meter from 0, 100, 200, 400, 800, 1,600, or varying the numbers of species similarly. When this is done, then powerful tools for data analysis can be used (consider Gough & Grace 1999; Grace 2003, and the discussion that follows). I

recognize that this cannot always be accomplished because of the constraints imposed by limited space, funds, and time. However, the observation that less than 50% of experiments are in any way complex is troubling and it limits what we are learning from these experiments.

Opportunity 1: Multivariate Experiments

Simply adding treatments and levels does not mean that one has avoided demonstration science. Consider the effort and apparent complexity of experiments designed to demonstrate that diversity can affect ecosystem function (e.g., Loreau et al. 2002). To avoid demonstration science, we need to design multivariate experiments that allow us to recognize and explore the complex, multivariate nature of ecological systems. By designing multivariate experiments, we can assess the relative importance of several causal drivers, such as site preparation, seeding mixtures, the order of species introductions, management (burning, mowing, etc.), other disturbances, resources, or other key factors. If we conduct such experiments then we can move beyond mundane questions, such as “Does burning increase the cover of target vegetation?” to “What is the relative importance of burning, mowing, excluding herbivores, and soil quality on the cover of target vegetation?”

There are two major obstacles to implementing such an experiment. The first obstacle is how to analyze the data. The methods of structural equation modeling and related forms of multivariate hypothesis testing (Pugesek et al. 2003; Grace 2006) are an important solution. There is a growing acceptance of the utility of structural modeling, and I believe that we are at the onset of a global change in ecological data analysis and multivariate hypothesis testing. The other main obstacles are logistics and funding. One solution that we need to consider is even greater partnerships with both government and nongovernmental organizations to tap into the wealth of quasiexperiments that have been performed across the landscape. Even though management agencies are generally focused on achieving an outcome, sites and restoration methods are often heterogeneous and therefore amenable to hypothesis testing. Another solution may be to use the power and sophistication of multivariate hypothesis testing to impress funding agencies, especially when the proposed research is appropriately targeted to agency priorities.

Obstacle 2: Myopic Scholarship

As a group of scholars, we are rather short sighted in terms of how we relate and compare our work to the literature. This collective myopia is manifested in a lack of conceptual context and therefore there is little or no synthesis. Without synthesis and comparison, we have little hope of building causal understanding and general predictive capability.

There are two issues here—first, we tend to compare our results only to similar experiments and projects in the same community type (i.e., association or phytocoenon, the same dominant species; van der Maarel 2005), rather than minimally comparing other cases within the same formation or vegetation type (e.g., C4 grasslands). Indeed, we rarely make reference to similarly designed experiments in completely different vegetation types. This omission is problematic because it is a key step in creating generality (Pickett et al. 1994). We need to compare the effects of treatments among a variety of habitats and locations to establish predictive generalities. This is standard practice in ecology or any science, where repeated, confirmatory instances lead to both causal understanding and theory acceptance (Loehle 1987; Pickett et al. 1994). The alternative is to have a hodgepodge of anecdotes.

For example, I conducted a small experiment in wetland restoration that addressed two issues: site preparation (removal of non-native vegetation using plastic screening or herbicide) and replanting (using seeds or small plants; Weiher et al. 2003). Looking back, our scholarship was rather meager; we failed to compare our results to the wider variety of experiments addressing site preparation and replanting and therefore we did little to advance our general understanding of the roles of the history of site preparation and reestablishment.

The second issue is that we tend to ignore ecological theory. Here I mean conceptual theory (ideas), not theoretical ecology (the ecology of equations). Only about 25% of papers I surveyed had one or more references that dealt with the conceptual nature of the study. Most of these were singular, passing references that merely acknowledged the existence of the more general literature. I was rather surprised in the near absence of references to foundation literature in plant and community ecology. Occasionally (about 25% of the papers that included general references, less than 10% of all papers), the authors discussed the implications of their study in light of general theory (examples of fine scholarship included Alexander & D'Antonio 2003; Duncan & Chapman 2003; Matthes et al. 2003).

Why bother with theory? To me this is the same as asking why bother with science, just restore the place as best you can and be done with it. Share your observations with others informally. But if one cares about the science, the building and testing of general principles, and the prospect of moving restoration practice from something that is best left to experienced artisans to a form of applied science or engineering, then we need to understand how communities and ecosystems work. Enhancing the linkages between restoration practice and theory has been a consistent theme in restoration ecology and land management (Hobbs & Norton 1996; Dale et al. 2000; Temperton et al. 2004; Young et al. 2005).

As far as I could tell, few of the papers advanced the conceptual science of restoration. By this, I mean that there was general paucity of novel ideas presented and very little

in the way of general synthesis. This is also a symptom of demonstration science. That is not to say that all the studies I considered were not original, pertinent, and novel. For example, Matthes et al. (2003) included the effects of human visitor density on target plants, and human effects on restoration success are something that is not often considered.

The cause of our myopia may also be partly due to peer review, where reviewers may be highly critical of placing one's work in a broader context. Reviewers may call this speculation or overreaching, but we must also address the broader context. When my colleagues and I teach our students how to search the literature, we nearly always have to push them to broaden their search. For example, the dispersal of animals into restored habitat patches is a general phenomenon, and so a study of mammals in restored riparian forest may indeed be germane to a study of butterflies in restored grassland. Why is it in our nature to so easily focus on the details of the question at hand only to forget the broader conceptual science?

As editor in chief of *Restoration Ecology*, Richard Hobbs has made broader scholarship a core editorial policy to encourage us to make conceptual connections (Hobbs 2005).

How Should Restoration Ecology Develop in Near Future?

I have suggested some avenues for moving restoration ecology forward. These include moving from a demonstration science mode to a multivariate and mesocomplex mode, and from a myopic style of scholarship to a broader synthetic view. In doing so, there will be a greater synergy between restoration ecology and basic research in ecology, and both will advance more quickly. What ecological theory can bring to restoration ecology includes some nascent theory (e.g., Weiher & Keddy 1999; Temperton et al. 2004) and some methods for making better progress. These include methods for designing and analyzing complex experiments (e.g., Pugsek et al. 2003; Grace 2006), and methods for building generality via comparison and synthesis (and sometimes controversy, e.g., Loreau et al. 2002). Some have criticized community ecologists for making little progress, but the conceptual progress that has been made, although not great, is substantial. Restoration ecologists might learn from the lessons (both the successes and failures) of their neighbors in community ecology. Conversely, community ecologists have a lot to learn about basic theory from restoration experiments. After all, the greatest test of theory comes when we apply it to real problems.

To do complex, large-scale experiments, we should be open to collaborations beyond the norm. I think there is a need to consider restoration experiments that might appear to be on the fringe of science. Rosenzweig (2003) has encouraged us to use as much land for conservation as possible, and this means actively creating seminatural habits in urban areas, such as traffic islands and mall

landscaping. Many of these inhospitable places likely provide the kind of stress that is required by many threatened species and communities (Grime 2001; Körner 2003). Savvy landscape architects and planners such as Shane Coen & Partners in Minneapolis, Minnesota, plan and design housing developments to ecologically fit with the landscape, and so part of their plans often involve restored prairies and savannas (<http://www.coenpartners.com/>). When I laid out a 4.5-ha grid for a prairie restoration experiment, I was so proud of my Cristo-like achievement, I was inspired to consider the idea that we might benefit by working with landscape-scale artists. Artists might help current restoration practitioners/artisans produce landscape-scale art, or perhaps help us turn experimental treatments into something aesthetically pleasing. Perhaps more pragmatically, power companies might be interested in restoring vast areas of prairie for biofuel (Tilman et al. 2006). This would certainly help with the logistics of more complex experiments, and it might just be a good thing as well.

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