Science-Driven Restoration: A Square Grid on a Round Earth?

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Abstract

Is formal science necessarily an effective framework and methodology for designing and implementing ecological restoration programs? My experience as an ecologist in Hawaii suggests that even when scientific research programs are explicitly designed to guide and facilitate restoration, the culture of science, heterogeneity of nature, and real-world complexities of implementing land management practices often limit the practical relevance of conventional scientific research. Although alternative models such as adaptive management and transdisciplinary science may facilitate research that more robustly models the real world, there is often little professional support or incentive to orient even these nonconventional research approaches toward actually solving on-the-ground problems. Thus, if one's goal is to accomplish ecological restoration as quickly and efficiently as possible, a trial-and-error/intelligent tinkering–type approach might often be better than using more rigorous, data-intensive scientific methodology. However, the sympatric implementation of ecological restoration and scientific research programs can lead to valuable synergies such as mutual logistical and financial support and the exchange of distinct forms of knowledge. The professional activities and mere presence of scientists can also greatly enhance a program's prestige and visibility, which in turn can indirectly promote more and better ecological restoration. Improving our understanding of when formal science can directly assist restoration projects and when its value will more likely be synergistic and indirect could lead to better science, better ecological restoration, and better relationships between these two cultures.

Key words: ecological restoration, Hawaii, practical relevance, role and limits of science.

Introduction

As a research ecologist with a strong interest in effective and efficient restoration, I often design factorial field experiments that use a series of replicated plots to rigorously test the effects of specific treatments (e.g., supplemental seeding, watering, and weeding) and/or habitat characteristics (light availability, slope, soil, etc.). Yet I have found that the extent to which different ecosystems or even patches within a given ecosystem lend themselves to this approach varies considerably over space and time. After years of struggling to superimpose my rigid, uniform experimental grids over often defiantly plastic, heterogeneous landscapes, it occurs to me now that the power and limitations of this approach may also serve as a metaphor for the overall utility of formal science itself in restoration ecology and conservation biology.

Many authors have highlighted the importance of the various aesthetic, cultural, socioeconomic, and political components typically associated with ecological restoration projects. Some have also discussed the different cultures of and conflicts between scientists and restoration practitioners and stakeholders and suggested various reforms in the theory and practice of restoration science (e.g., Baldwin et al. 1994; Gobster & Hull 2000; Higgs 2003, 2005; Jordan 2003; Hobbs 2005; Naveh 2005; Turner 2005). For example, Higgs (2005) worried that the "scientific authoritarianism" of restoration ecology may be in danger of subsuming the effective practice of ecological restoration and argued for a broader, more holistic approach with greater respect for other kinds of knowledge. However, in this article, I temporarily put aside these kinds of valid and important concerns to focus on the practical value of the science itself.

The concept of science can mean very different things to different people. For example, in my experience, land managers often think of science as any careful, systematic approach that involves recording data and/or making careful observations, whereas academically trained researchers tend to have a much more formal and narrower sense of the word (hypothesis formation and testing, replication, statistical rigor, etc.). Although both these interpretations are technically valid (in Brown [2003], The New Shorter Oxford English Dictionary's 2b definition of science is "Skillful technique, especially in a practical or sporting activity," whereas its 3c definition is "An activity or discipline concerned with theory rather than method, or requiring the systematic application of principles rather than relying on traditional rules, intuition, and acquired skill."), here, I want to explore the value of science as expressed in the latter, more academic definition. Is this kind of science necessarily an effective framework and

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methodology for designing and implementing ecological restoration projects? Both because it is what I am most familiar with and because I do not wish to critique the value of other scientists' work, I begin by analyzing the contributions of my own research program (conducted in collaboration with numerous other scientists) within degraded tropical dry forests in Hawaii.

Study System

Tropical dry forests are among the most endangered and degraded of all ecosystems in the world in general and within the Hawaiian Islands in particular (reviewed by Janzen 1988; Bullock et al. 1995; Cabin et al. 2002*b*; Vieira & Scariot 2006). In 1993, the U.S. Fish and Wildlife Service agreed to coordinate conservation efforts for the long-term management and protection of remnant dry forests on the island of Hawaii. This effort eventually coalesced into a "Dry Forest Working Group," which now includes local residents and volunteers, native Hawaiians, scientists, and more than 40 other members representing above 25 different agencies. After an extensive search for an ecologically, economically, and a politically feasible site to initiate a model restoration project, this Working Group selected a remnant dry forest parcel within the Kaupulehu region of the island. For more details of this study system and restoration project, see Cabin et al. (2000, 2004).

In 1997, I moved to the island of Hawaii to take a job as a research ecologist for the U.S. Forest Service and continue my work at Kaupulehu. My personal mission was to conduct research that investigated academically interesting questions, informed the Working Group of the likely ecological consequences of different management strategies, and promoted the preservation and restoration of this ecosystem in general.

The next several years were exciting: the Working Group grew steadily larger and stronger; our restoration program made slow but steady progress; and I felt confident that my research (as well as the work of an increasing number of scientific colleagues) would soon yield data and insights that would effectively guide and facilitate the restoration of native dry forests within and beyond Kaupulehu.

At the same time, however, I increasingly began to question the practical value of much of the scientific research I encountered in Hawaii and elsewhere that was allegedly designed to help conserve and restore species and/or their ecosystems. These doubts were later reinforced in an essay by Ehrenfeld (2000), the founding editor of the journal *Conservation Biology*. "From the beginning," he wrote, "the journal was by most objective measures a roaring success …. But occasionally I would experience a small spasm of doubt. Conservation biology was supposed to be, like medicine, a life-saving profession. Were we saving the lives of any species or ecosystems?" Later in the essay, he wondered whether

> … deep down, conservation biology isn't really like medicine—perhaps we are just ordinary biologists trying to find comforting and trendy justifications for doing what we love to do anyway. This possibility was supported by quite a few of the manuscripts I received, which seemed to have little to do with actual conservation. Typically, after devoting 16 pages to the genetics or ecology of a plant or animal that happened to be rare, or that might some day become rare, the authors would tack on a depressingly predictable final paragraph that would explain how important this work could eventually be to conservation and why more research was needed.

Ehrenfeld then reported the results of his survey of all the papers published in the February, April, and June 1999 issues of *Conservation Biology*.

For each of the 66 published articles, I asked this question: Is there strong indication that any actual conservation has been achieved already as a result of this work? Has the doctor made the patient better yet? The answer for all but 3 of the 66 articles was "no!" No matter how exciting and convincing 63 of those 66 papers were and no matter how painstakingly constructed their conservation arguments, the predicted conservation dividends were to be earned in the unspecified future. Why? Is conservation biology a delusion?

I was becoming convinced that the answer to Ehrenfeld's question for both conservation biology and restoration ecology was largely "yes!" (The results of my qualitative analyses of *Restoration Ecology* articles over the years have been very similar to those in Ehrenfeld's above-mentioned *Conservation Biology* survey; also see Whitten et al. [2001] and Kleiman [2003]). Yet I was equally determined to make my research yield concrete, significant "conservation dividends."

Challenges of Doing Science within a Restoration Program

From the beginning at Kaupulehu, I found that the roads leading to scientific achievement and effective ecological restoration frequently diverged. First, the type of science I could even attempt was often constrained by the pressing need to get things done on the ground. For example, our initial efforts to control the dominant alien species at this site appeared to facilitate the regeneration of both key native species and new and potentially invasive alien species. These results in turn raised several new intriguing questions that could have been elegantly addressed within this study system (discussed in Cabin et al. 2000). However, the Working Group understandably decided that the potential knowledge gained from studying these new aliens (as opposed to eradicating them before it was too late) was not worth the risk of jeopardizing the actual restoration of this forest.

Second, the goals and practice of science (e.g., basic knowledge acquired via methodical observation and experimentation) often conflicted with those of ecological restoration (e.g., immediate results guided by common sense, local knowledge, and informal trial-and-error procedures). Thus, to the people laboring to restore Kaupulehu, many of my experimental methodologies (growing control plants in highly unfavorable microsites, suspending weeding and watering regimens, destructive harvests of native plants, etc.) often appeared counter-productive if not downright stupid!

Third, the intrinsic ecological heterogeneity of this study system combined with the real-world complexities of implementing land management practices often severely limited the practical relevance of the "statistically significant results" that I detected within my grids of randomly placed square meter quadrats. For instance, one experiment (Cabin et al. 2002*a*) surprisingly found no significant differences in the biomass of newly recruited native species within weeded versus nonweeded plots. Yet although we had designed this study in part to help optimize the efficacy of our weeding efforts, our actual weed management program was ultimately driven by a mixture of other concerns that were beyond the reach of this experiment (Which microsite/native species combinations could safely be left unweeded, and for how long? How would a prolonged drought affect this relationship? Would untrained volunteers be able to efficiently find native species within thick weed patches? How would a weedy understory affect our outreach program and future funding capabilities …?).

Last and perhaps most importantly, the different goals and values of the diverse group of individuals and interest groups comprising the Working Group often generated conflicting visions of what we should do and how we should do it. For example, how much of our limited time and money should we devote to on-site restoration, how much to basic science, and how much to education and outreach? Should we employ the most effective restoration techniques or limit ourselves to the tools and methodologies that could most feasibly be emulated by local private landowners? How "pure" should our restoration program be—reintroduce locally extinct native species from other parts of the island? Use promising native species that may never have been in Kaupulehu, or even on the island? Non-native but culturally important species introduced by the Polynesians? "Benevolent" new alien species …?

It became painfully clear to me that science alone could never resolve these kinds of critically important issues, and even when it could address at least some of the questions surrounding a specific contentious debate, I discovered at Kaupulehu and elsewhere that people (including many scientists themselves!) will often advocate using "objective science" to settle their disputes until that science suggests something that conflicts with a strongly held personal belief or value.

Synergies of Science and Restoration

For me, the many challenges of conducting research at Kaupulehu were often more than offset by three categories of major benefits. First, my colleagues and I would never have even attempted much of the research we ultimately accomplished without the Working Group's ability to develop and maintain the site's infrastructure (e.g., fire breaks and access roads, ungulate fences, irrigation networks, site security). Second, the on-site restoration program supported our research projects by directly and indirectly enhancing our ability to secure funding from nonconventional sources, obtain basic equipment, and recruit and organize a crew of highly dedicated volunteers. Third, being part of such a diverse community of people and organizations that cared about dry forests in particular and conservation in general informed and inspired me in ways that extended far beyond the reach of science. To take just one example, one day as I was walking through a grove of scrubby *Psydrax odorata* trees in full bloom, I told my native Hawaiian colleague that the light fragrance of the trees' small white flowers seemed to creep mysteriously in and out of my nostrils. He closed his eyes, smiled, and explained that the Hawaiian name for this tree, "*alahe'e*," literally means "to move through the forest like an octopus."

I also believe that my scientific colleagues and I significantly contributed to the success of the actual dry forest restoration program at Kaupulehu in three major ways. First, our scientific grants and research programs provided critical additional funding and motivation to develop and maintain the site's infrastructure as described above. Second, our professional activities, equipment, and simple presence at Kaupulehu significantly enhanced the restoration program's prestige and visibility, which in turn helped the Working Group obtain basic funding, recruit additional members and volunteers, and develop and deliver effective public relations and outreach programs (often in direct collaboration with us). Third, I think that many of the lay members of the Working Group and public at large were similarly informed and inspired by our general scientific knowledge and specific research findings at Kaupulehu; in other words, we also had some pretty good stories to tell.

Ironically, however, I believe that my actual research itself contributed very little in terms of directly applicable practical knowledge, tools, and ideas—the very items I had worked so hard to deliver. As shown above, the results and insights gleaned from my reductionist science were virtually impossible to translate into unambiguous actions and strategies for restoring this site, let alone other dry forests outside the Kaupulehu region. Frankly, looking back now, I think that the success of our various restoration projects was largely determined by the vagaries of our collective interpersonal dynamics, funding, and blind luck. However, over time, we also got much better at the art (not science!) of recognizing and capitalizing on a convergence of serendipitous events (e.g., a rare period of favorable weather conditions that happened to come when we had lots of seeds, transplants, money, equipment, and volunteer labor ready to go).

What is Good Science?

Recently, I was on a U.S. Department of Agriculture (USDA) "Biology of Weedy and Invasive Plants" panel charged with ranking the merit of more than a hundred grant applications. After finally whittling down our "must-fund" list to about a dozen proposals, we were informed that due to unforeseen budgetary shortfalls, the USDA would only be able to fund a few of the grants submitted to our panel. Therefore, we were instructed to further shorten our list down to the few proposals that represented "the very best science."

The ensuing debate illustrated just how subjective the concept of "good science" is. In our case, did it mean research that would most likely produce the greatest contributions to our academic, theoretical frameworks for modeling the biology of invasive species? Were clever and sophisticated proposals better than more creative and simplistic ones? Were high-risk/high-potential experiments better than sure-fire but less exciting ones?

As we agonized over such questions, I found that I was virtually the only panel member who considered "practical relevance" to be an important attribute of good ecological science, especially when the subject was weedy and invasive species. Yet I had to concede that the few proposals on our list of finalists that were most oriented toward addressing real-world problems were narrower in intellectual breadth, less rigorous (fewer replications, less tightly controlled variables, more collaborations with management-oriented groups, etc.), and riskier than those that focused on more theoretical and abstract intellectual issues. Several panelists also made the familiar argument that the grants in the latter group could lead to the development of models and knowledge that would eventually be valuable to the broader land management/conservation community. Needless to say, in the end, we funded the most basic, academically oriented proposals.

I know that many scientists and academics believe that it would be inappropriate for agencies like the USDA or the National Science Foundation to support "simplistic" applied research and that professional, PhD researchers should not "waste" their expertise on narrow, idiosyncratic land management problems. Indeed, it was made abundantly clear to me during my first semester of graduate school that there was little academic support, funding, prestige, or jobs available to scientists who focused on tackling applied problems. Moreover, throughout my subsequent career, I have often detected at least an implicit tendency within the larger academic community to consider the merit and prestige of research programs to be inversely correlated with their applicability to real-world problems and projects.

Yet whose job is it to rigorously research how best to, say, contain an invasive species or restore a degraded forest? At least in Hawaii, I found that these kinds of problems were largely not being addressed by the land management/research extension communities—their people were simply too busy, too underfunded, and too poorly trained. Consequently, some of the most important yet least flashy land management questions (e.g., what is the best way to propagate this rare native species? What is the most effective herbicide to control that weed, and how exactly should we apply it?) were rarely subjected to thorough and rigorous research methodologies.

When I first ran up against these kinds of questions in Hawaii, I tried to build an academic framework around them in an effort to both contribute to the intellectual advancement of the ecological sciences and directly facilitate the actual preservation and restoration of Hawaii's native biodiversity and ecosystems. Yet I consistently found that attempting to simultaneously achieve each of these goals ultimately compromised them both. Eventually, I shifted my approach toward establishing collaborative partnerships with land managers; I thought that if I superimposed my experimental designs and data collection protocols on top of the work they were already doing anyway, together we might begin to uncover more effective and efficient methodologies for addressing some important real-world problems. Yet this research model also largely failed: my spatially limited square grid methodologies once again did not sufficiently encapsulate the relevant ecological/human world variables; the land managers and their crews eventually became too busy doing their "real jobs" to rigorously maintain our research programs; and/or I became too busy doing my "real job" to provide sufficient oversight and timely guidance when the inevitable unforeseen complications arose.

Some authors have argued that the solution to the present disconnection between formal science and on-the-ground conservation and restoration programs is to reform the culture and practice of science. For example, Naveh (2005) believes that better integration between the bioecological and human ecological aspects of ecological restoration could be achieved if restoration ecology was transformed into a transdisciplinary "metadiscipline." He maintains that this new discipline would transcend the conventional "normal" sciences via a paradigm shift away from the current emphasis on reductionistic and mechanistic processes in favor of more holistic, organismic, and nonlinear approaches. Many others (see reviews and discussion in Groom et al. 2006) have similarly argued that the dynamic, nonlinear nature of natural ecosystems demands more flexible, robust, and nonhierarchical alternative models such as adaptive management (sensu Holling 1978; Holling & Meffe 1996) protocols that strive to implement management actions as explicit and discreet scientific experiments so that their effectiveness can be continuously assessed and refined.

Yet although transdisciplinary science, adaptive management, and some even more radical, "postnormal" approaches (see discussion in Allen et al. 2001) often sound good in theory and may even eventually lead in practice to research that more robustly models the real world, at present, there is still little if any professional incentive to orient even these nonconventional approaches toward actually solving on-the-ground problems or accomplishing significant conservation and restoration projects. Thus, even if these alternative models are one day widely adopted by practicing professional scientists, the net result might simply be different (but not necessarily more practically relevant and useful) academic frameworks and specialized jargons. I would therefore argue that the best way to promote research that actually is practically relevant and directly applicable to real-world ecological problems is to provide the necessary incentives and rewards (e.g., give on-the-ground accomplishments at least as much weight as theoretical and academic advancements; create prestigious research positions, journals, and/or funding for practitioners of ecological restoration).

The Limits of Science and the Value of Intelligent Tinkering

I love to experiment in my garden but have slowly learned to leave my academic research hat at the garden gate—there is just too little space, too few replicates, too many variables, and too much actual work to meaningfully employ formal research protocols. Although it is fascinating to think about all the potentially important interacting ecological variables and processes within my garden ecosystem, I have not found my academic training and research experience to be of much practical use there. Similarly, although I am grateful for the knowledge provided by my local agricultural extension service and the horticultural sciences in general, most of the information and techniques I actually employ have come from years of my own observations, trial-and-error experimentation, and interactions with other skilled gardeners. Even if I had a team of agroecologists at my disposal, the last thing I would want would be for them to usurp some of my precious garden space for their research, especially if they expected me to do additional work on their behalf!

I think that the relationship between restoration ecologists and practitioners of ecological restoration can often be analogous to the above-mentioned agroecologists/home-gardener example. The reality is that at least in the short term, our research often does not produce much of practical value to the people and projects we are at least theoretically trying to help. It is sobering to realize how often the "management implications" section of our publications and presentations largely consists of lists of yet more potentially important new things for beleaguered practitioners to consider and address. Indeed, at Kaupulehu and elsewhere, I have often been irritated by visiting academics who arrogantly criticized our work for not devoting more resources toward incorporating some onerous pet model or collecting ever more extensive and labor-intensive data. What these people seemed unable to grasp was that in addition to more money and labor, what we and other restoration programs typically needed most was not more and better intellectual tools but more and better *real* tools!

In my opinion, the pursuit of pure knowledge is a sufficient justification for research; whether or not it ever intentionally or serendipitously leads to anything of practical value is a separate issue. But I do have a problem with research that is falsely justified on the basis of its alleged practical value. Moreover, I believe that it is unethical yet all too common to fund and perform basic research instead of, or even at the expense of, urgently needed conservation and restoration activities. To take just one notorious example, much of the hundreds of millions of dollars specifically earmarked for the restoration of Alaska's Prince William Sound following 1989's devastating oil spill by the *Exxon Valdez* was ultimately usurped by an ever-expanding scientific "cottage industry." As one long-term Alaskan (Holleman 1999) lamented: "But it [science and the surrounding scientific infrastructure] is not restoration. They are not working in the best interest of the wildlife …. Yes, we know more about these animals being counted and darted, poked and prodded. But what good is that knowledge? They aren't more protected from oil spills; their lives aren't better, safer."

In Hawaii, I gradually abandoned my original goal of making my research practically relevant. Frankly, in addition to the difficulty of meaningfully applying my results to actual restoration programs as discussed above, I grew tired of the (often valid) criticism from the academics and bureaucrats on the one hand ("Your research does not address issues of sufficiently broad interest/intellectual depth to warrant funding/publication"; "I'm not paying your salary to manage someone else's weed problems!") and the land managers and conservationists on the other hand ("Your research is too abstract, reductionist, and irrelevant to apply"; "When will you stop intellectualizing and quantifying our problems and start actually doing something about them before it's too late!").

At Kaupulehu, I satisfied my desire to directly contribute to our on-site restoration program by simply helping with the physical labor whenever I could and by designing some of my experiments so that they produced useful "by-products" (surplus endangered plants suitable for transplanting, irrigation infrastructure in strategic areas, etc.). Ironically, this strategy seemed to appease both sides—suddenly I seemed to be doing both real science and real restoration! Moreover, the more I focused on abstract, academic research questions that were intellectually and physically separated from our actual restoration program, the less I had to contend with on-the-ground logistical and "managers versus scientists" cultural conflicts.

Today, if I was in charge of restoring the Kaupulehu region of the island of Hawaii as quickly and efficiently as possible, I would create an "adopt an acre" program in which each volunteer group would receive their own parcel of degraded dry forest. Beyond some common sense guidelines (e.g., no cutting down of endangered native trees or planting invasive alien species), for once there would be no pretense of testing more general hypotheses or implementing rigorous data collection and monitoring protocols. (Every land management agency I know of in Hawaii is overflowing with decades of pseudoscientific "quantitative baseline data" that have and continue to require enormous resources to collect and process, yet remain largely uninterpretable and ignored). However, groups would be encouraged to perform informal, intelligent tinkering–type experiments, take pictures, and record some qualitative notes, and of course any group (or outside scientists) would be free to devote as much time to more rigorous methodologies and extensive data collection protocols as they wished.

At the end of each year, a democratically elected governing board would evaluate the progress of each group and increase or decrease their acreage for the following year in accordance with their previous performance, general value to the overall restoration program and community, and on-the-ground results. I believe that the community involvement, diversity of approaches, healthy competition, and public accountability resulting from this model would foster more and better restoration at Kaupulehu and greater public involvement, understanding, and support for restoration and conservation in general.

I know that some of my colleagues would find the uneven checkerboard of "restored" plots resulting from this approach to be inauthentic, ecologically compromised, and/or unethical. Yet I would argue that first, within the Hawaiian islands in particular and an ever-increasing portion of the rest of the world in general, due to the overwhelming effects of factors such as alien species invasions, functional and actual native species extinctions, and climate change, even the most rigorous and cautious restoration programs cannot bring back historically "authentic" ecosystems (Harris et al. 2006). Second, as ecologically imperfect as these acres might be, they would represent a vast improvement over the monocultures of noxious, fire-promoting alien grasses and shrubs they would replace. Even if we agreed on the debatable idea that the worth of restored areas is directly correlated to their ecological purity and degree of scientific documentation, the reality is that in this and many other degraded ecosystems, little if any land can actually be restored without the use of blunt tools and pragmatic ideologies. (I have found a strong inverse correlation between peoples' ecological purity and the spatial extent of restoration they have personally achieved.) Third, because even the most pristine ecosystems on Earth are now significantly affected by humans (Vitousek et al. 1997) and our "wildernesses" may be more accurately categorized as "wildland gardens" (Janzen 1998), I would argue that the most appropriate ethical question regarding degraded and degrading ecosystems is not whether we have the right to play God and "fake nature" (sensu Katz 1996; Eliot 1997) but rather the relative risks and consequences of doing nothing, doing a little, and doing as much as we can.

There are obviously times and places in which our scientific square grids fit the ecological and human landscape reasonably well, and thus a formal scientific approach may be an effective or even the most effective path to successful ecological restoration. Some might also argue that our young discipline of restoration ecology will eventually blossom into a mature science with as much unifying theory and practical power as modern physics, and thus one fine day, our square grids will fit most if not all the real world. Indeed, I know many scientists and nonscientist alike who believe in the universal supremacy of science with a fervor that resembles religious fundamentalists. Ironically, however, like religious beliefs, this hypothesis is actually based on faith, not science, because there is no way to empirically test and falsify it.

It seems at least equally plausible to me that in contrast to the relative simplicity of understanding and manipulating nature's inanimate physical forces, the infinitely greater complexity of interacting living species and the messy realities of the human world may combine to ultimately limit the practical relevance of the science of restoration ecology. Perhaps there will always be some if not many cases where our square grids simply do not fit the real world; thus, the best we can do is develop more organic and holistic grids, lend our support to other ways of knowing and doing, and/or get out of the way! Objectively and rigorously investigating when, where, and why a formal scientific approach may or may not be an appropriate and effective methodology for achieving on-the-ground restoration might be the best thing we could do to promote better science, better ecological restoration, and better relationships between these two cultures.

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