In support of observational studies

Agrawal *et al.* (*Front Ecol Environ* 2007; **5[3]**: 145–52), in an NSF-organized panel, take on a formidable task by attempting to identify gaps in population and community ecology, and they outline several interesting research directions in the process. In the spirit of open dialogue, however, I ask that NSF and the research community consider a radically different direction for ecology, one that resurrects the ghosts of old naturalists and yet is fully grounded in contemporary ecological problems and facilitated by modern technology.

This approach places observational science as ecology's driving force, and implicitly includes human influences as a factor to be accounted for in all ecological studies, be they observational, experimental, or theoretical. While the panel acknowledges in an appendix to the Agrawal paper the role of climate change and human drivers, and suggests that observational studies should "complement and expand on" experimental work, I argue that there is both the opportunity and the societal urgency to make observation-driven ecology our highest priority.

The failure to make observational science the center of ecology for the past 40 years has manifested itself in several disturbing ways. For instance, the same issue of *Frontiers* in which the Agrawal *et al.* paper appears reports that, lacking long-term studies on population dynamics, we must resort to a captive "Amphibian Ark" to study and understand amphibians, which are rapidly disappearing from the wild (Dispatches; *Front Ecol Environ* 2007; **5[3]**: 117). Likewise, we have so few comprehensive field surveys of ecological communities that ecologists are left to dig through scattered records of naturalists, traditional cultures, and even gamblers to piece together the effects of recent climate warming on these systems (Sagarin and Micheli 2001; Nijhuis 2005). Yet, even as we face an appalling lack of relevant observations on the biophysical environment, we have even fewer data on human social and economic dynamics in ecological systems. In the spirit of the panel's call to understand interactions, we cannot continue to function as if socio-economic behaviors of humans – which often lead to the strongest relative forcings in ecological systems – are outside the purview of population and community ecology.

It is because of this ubiquitous influence that observational studies take on a renewed urgency in ecology. The panel lauds the "fruitful" 40-year transition from thinking of communities as structured by competition to our more holistic current understanding. Although this theoretical and experimental pathway has been crucial to understanding some ecological mechanisms, such a pace of progress is far too slow to address human impacts on ecological systems. By contrast, the relatively recent resurgence of observational approaches has already delivered startling successes at all levels of ecology, from basic to applied research. This has been facilitated by new technologies unimaginable to early naturalists, such as satellite tags available for a range of taxa that have provided stepwise advances in our understanding of wild populations and how to conserve them (Ropert-Coudert and Wilson 2005). Likewise, molecular population genetics gives us a window with which to see both the present and past for populations of critical ecological concern, as demonstrated in work on historic whale populations (Roman and Palumbi 2003). Even lacking these technological marvels, historical observational studies, which are a relatively small percentage of published ecological research, have been disproportionately critical to identifying the "fingerprints" of climate change on populations and communities (Root *et al.* 2003).

In returning observational approaches to the prominent role they played in early ecology, we must get over individual and institutional biases that suggest observational natural history lacks the rigor of experimental and modeling approaches (Francis and Hare 1994). Indeed, given the opportunity of new technologies and the challenge of broad-scale ecological problems, observational studies will often provide the only data relevant and extensive enough to falsify alternative hypotheses. I argue that an aggressive push toward supporting observational studies of human influences on ecological systems at NSF will not only fill innumerable gaps in our knowledge and make societal problems more tractable for ecologists, but will also re-energize the spirit of pure discovery that brought many of us to this field to begin with.

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The authors reply

We agree with Sagarin that observational studies are important. At some spatial and temporal scales, this may be the only possible approach. Advances in methods such as remote sensing, phylogenetic analyses, and statistical techniques have greatly expanded the utility of observational studies. However, important misconceptions have arisen from studies based solely on observation and correlation. For example, it took a large-scale manipulative experiment, following hundreds of years of observations and correlations, to come to some resolution on the causes of snowshoe hare population cycles (Krebs *et al.* 1995). Similarly, the current surge in research on the causes and consequences of invasive species is revealing a level of mismatch between experiments and previous observations (Colautti *et al.* 2004; Parker *et al.* 2006).

Experiments can provide a mechanistic basis for strong scientific inference, as well as improved and reliable predictive power that can be applied to management decisions. As we commented in the original article, the two approaches are not mutually exclusive and the simultaneous application of both observations and experiments frequently provides more insight than either one alone. Even for the case of studying large-scale anthropogenic change, it is clear to us that the most powerful approaches integrate observations, experiments, and modeling (Carpenter 1998). Although we agree with Sagarin that, currently, the "pace of ecology is far too slow to address human impacts on ecological systems", there is little reason to believe that returning to primarily observational approaches will serve ecology well.

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LIHD versus HILD biofuels

Low-input high-diversity (LIHD) biofuels appear more ecologically sound than many alternative biofuel systems (LIHD biofuels: toward a sustainable future*; Front Ecol Environ* 2007; **5[3]**: 115). However, biofuels as a group have a very limited niche in energy supplies because they are extremely intensive in use of land (habitat) and, in most cases, water (for which we are already beyond sustainable use).

Land use is intensive because yields are tightly limited by the physiology of photosynthesis. Maximum dry biomass yields with high inputs average about 30 tons per ha in the continental US, in *mesic* areas. With future improvement of cellulosic fermentation of biomass, conversion of 90% of the energy might be attained, giving about 513 GJ ha^{-1} annually. We must debit at least one-third of this total for energy inputs in growing and harvesting biomass; over and above harvesting activities, energy is used in fertilizer production, as nutrients must be resupplied when biomass is consumed off-site. Then, we must reduce energy yields by another one-third for losses in processing. We are then at 228 GJ ha^{-1}, equivalent to about 8300 L of hydrocarbon fuel per ha. Compare this to US liquid fuel use of 1 trillion liters annually and one sees a need for 117 million hectares of land, more than eight times the area of the state of Illinois. With "industrial-style" HILD (high-intensity low-diversity) biofuels, this is new land and lost habitat. It is also clearly an impossibility. With LIHD, as proposed by Wallace and Palmer, the land needed is about double that for HILD, given the lower yields, but it need not be new, and LIHD may even help maintain plant species diversity in some systems. An assessment of how much land would be necessary for LIHD is sorely needed.

Water use per unit biomass produced is also constrained by the physiology of gas exchange, to a practical minimum of about 300 g of water per gram of harvestable biomass, even for C_4 plants. Using biomass for all transportation fuels would demand about 300 trillion liters of water. With LIHD, much of this could be rainfall, but, again, the potential area for optimal (biodiversity-conserving) LIHD needs to be assessed. For HILD, much of the water would be from irrigation, given that expansion of biofuel cropping would largely be onto irrigated land. I have estimated that HILD irrigation demands would equal about six times our total (unsustainable) irrigation demands for current food and fiber crops (http://biologyweb.nmsu.edu/vince/biofuels_DW.html). The ethics of using cropland in a food-short world must also be considered, even with the qualifiers that exporting US crops is a mixed blessing for food-short areas (eg the local agricultural economy is suppressed). HILD must surely be ruled out as a viable option, and the potential role of LIHD must be quantified soon.

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The authors reply

The current biofuels debate is being dominated by talk of HILD, and a serious consideration of LIHD is needed. We fully agree that using only biomass (HILD or LIHD) to support current demand for transportation fuel is an impossibility; societal will to reduce consumption is vital.

For better or worse, we take it as given that there will be increasing pressure to exploit land for HILD, given the current political momentum of the issue. If ecologists do not play a role in the debate soon, LIHD will be ignored. There is an irony here: much spontaneously growing biomass is already "harvested" but not utilized. For example, the Federal Highway

Administration estimates that there are about 10 million acres of roadside vegetation in the US, approximately the area of Massachusetts and Connecticut combined. Add to this vast areas of powerline right-of-ways and fire breaks. Exploiting such areas, even if productivity is low, would not threaten the food supply.

The feasibility of LIHD may depend on more than just productivity, nutrient-use efficiency, or water-use efficiency. Imagine the rural landowner (such as retirees, urban refugees, and absentee landlords) for whom land management is not a top priority, but who would like to see some economic gains. If they are interested in HILD, they will need to research the crop, buy patented seed, plow, irrigate, fertilize, regularly inspect the crop, research diseases, spray and pray, and eventually harvest – or they can contract the work out, with associated headaches and costs. If they are interested in LIHD, they need to do little more than harvest, and can spend more time enjoying a beautiful and diverse landscape.

Utilizing hay meadows to fuel transportation is not new: horses, relying on grassy fuel, have transported people and goods for millennia. Some remnants of the once-vast system of hay meadows still exist throughout the world. In parts of Moravia, for example, hay meadows have been mowed (with a few lapses) annually for five centuries. This management pattern allows these areas to have high concentrations of rare plant and animal species. However, the decline of horsepower for transportation since the Industrial Revolution has lead to the disappearance of most of these valuable systems. Mowing them for biofuels may help reverse this trend.

Similarly, there are abandoned grasslands worldwide being encroached upon by woody plants and invasive exotics, which may create problems for conservation, allergies, fire danger, and hydrology. Sustainable harvest may be a profitable alternative to abandonment.

While biofuels alone will clearly not solve our current transportation dilemma, they may at least give us an opportunity to develop creative solutions to vexing vegetation management issues. We agree with Gutschick that "an assessment of how much land would be necessary for LIHD is sorely needed", but we would also like to turn the tables a bit, and claim that "an assessment of how much land would benefit from LIHD is sorely needed".

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