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Why is Meaningful Collaboration Between Ecologists and Economists So Difficult?

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From a policy perspective, the collaboration of ecologists and economists seems clearly desirable. At the very least, if a policy goal involves implicit or explicit choices of ecological "end points," estimates of the cost of attaining these will be relevant to the original decision and to subsequent reviews of the policy's performance. For example, consider the on-going heated debate about the wisdom, or lack thereof, of the goals and methods of the Endangered Species Act. But even the Clean Water Act involves implicit ecological choices, as its goals include making all U.S. water bodies of "fishable" and "swimmable" quality.

A more ambitious, but also more controversial goal for collaboration would be the assignment of monetary values to the ecologically defined end points, based on what the citizens directly and indirectly affected would be, or say they would be, willing to pay to see them achieved.[1] And at the end of this road lies cost-benefit analysis (CBA) of possible or existing policies, which can be used either as a hurdle (as in requiring that benefits exceed costs for a program, policy, or project to be acceptable) or as a basis for choice among competing alternatives (as in choose the alternative with the greatest excess of benefits over costs).[2]

It may be slightly less well known, and even less widely understood, that economists have gotten themselves in trouble with prescriptions for policy instrument designs that cannot perform as promised through their failure to take into account the complexity of the natural world's actions in translating pollution discharges into ambient environmental quality levels. For example, see Russell and Vaughan's (2003) discussion of whether and under what conditions, particular versions of charge or tradable discharge permit schemes can deliver the economist's touchstone, static economic efficiency (here lowest costs for given ambient quality results).

This much is hat for most readers who come to this issue from work in the environmental policy arena. But those same people are also likely to be familiar with one or more hugely contentious, if not actually failed, efforts to organize the sort of meaningful collaboration that is, on its face, highly desirable. The purpose of this short paper, based on a longer version completed as part of a project supported by the U.S. Army Corps of Engineers (Baumann and Bourge 2005), is to draw attention to what we believe to be a very large stumbling block present in the earliest stages of the road to such collaboration, a block built up of fundamentally mismatched understandings of how to think about the "value" of actions that purport to change ecological outcomes.

By way of background, we briefly describe three experiences with attempted collaboration, experiences spanning about 35 years in three quite different settings. (In the first two, one of us was an active participant; in the third, we were outside observers with access to the interpretations of participants.) These tales provide clues as to the detailed composition of the block in the road; at least as we interpret them. And in spelling out the lessons we find in them, we define a problem for the future. We look for a way around the block and into the future, but, to anticipate, we certainly do not claim to have found anything remotely qualifying as a magic bullet. Indeed, we seriously doubt one exists. Rather, we think real progress will depend on the existence of support for long-term collaboration, with all its frustrations, on fairly narrow, applied problems,

in which contexts the fundamental disagreements can be finessed long enough to get started. We do *not* advocate support for blue-ribbon committees of the eminent in the two disciplines, the products of which are likely to be expressions of opposition to sin and support for virtue, but not operational prescriptions for getting beyond the mismatched understandings.

Tales from the Trenches of Collaborative Efforts

The following brief descriptions of difficulties experienced in actual efforts at eco/eco collaboration illustrate several different guises in which the stumbling block to success can appear.

In which Monetary Valuation, even of Costs, is Seen as Wrong-headed when the Subject is the Choice of Environmental Policy

In the early 1970s, Russell was working with an environmental engineer at Resources for the Future on a large, for the time, mathematical optimization model for regional pollution control. The goal was to illustrate the practical implications of the conservation of mass and energy in production and in pollution treatment processes, and was inspired by Robert Ayres and Allen Kneese (1969). The model included air and water pollution links in the industrially-diverse and densely populated Delaware Estuary region of NJ, PA and DE. The optimization exercise was the minimization of the costs of meeting any given (feasible) set of ambient air and water quality standards, while the underlying cost models reflected possible transfers of pollution mass from one form to another and one medium to another, as in the burning of wastewater treatment plant sludge, or the dumping into water bodies of particulates removed from stack gases.

The first version of the model had for its water quality side a very simple, traditional version of the BOD/DO relation in the river. But the overseers of the project, Kneese and Blair Bower, felt that success in reaching the overall goal would require more extensive sets both of pollutants and of ambient quality measures. They pushed for the addition of an ecologist who would be able to build a model accepting at least a generalized "toxic," as well as nitrogen and phosphorous nutrients, and producing at least predictions of fish and algae as quality measures.

To make a long story short, such a person was hired. While the desired aquatic ecological model was eventually built (Kelly, 1975), producing somewhat peculiar results regarding fish, the whole effort failed in the policy arena (Spofford, Russell and Kelly 1976). This was because the Clean Water Act (CWA) of 1972 shifted attention from ambient water quality to treatment process definition. But the point here is not the policy failures. Rather, it is to set the stage for stressing that ecologists thought, and repeatedly said, that the idea behind the effort was foolish, if not actually pernicious. For him, the answer to the question, What should pollution control policy look like? was crystal clear and had nothing to do with costs. That answer was to minimize the impact of man, to aim for zero discharge, probably not coincidentally the stated ultimate goal of the CWA. Anything else would leave us with "polluted," unnatural, ecosystems, undesirable on their faces, whatever their multifaceted functioning might be predicted to look like in detail.

In which "Valuation" and "Value" are Seen to be Very Tricky Words

In the early 1990s EPA was spending substantial sums on the collection of masses of data about many technical indicators of ecosystem functioning in a variety of settings, from grasslands to wetlands, from seashores to mountains. This "Ecological Monitoring and Assessment Program" (EMAP) was intended to provide a foundation for better understanding and, ultimately, predictive modeling ability. But the indicators being studied were at the micro level and designated by names that only scientists could begin to relate to. To broaden the potential appeal of the program within and outside of the agency, including among those who held the congressional purse strings, EPA funded at least one project aimed at translating the indicators into more widely understandable terms. A team of ecologists, sociologists, and anthropologists from Oak Ridge National Laboratories (ORNL) joined with an economist, Russell, and a survey methodologist from Vanderbilt to undertake that task. While the basic challenge was translating language and somehow aggregating micro measures, it was also necessary to decide in advance how we would be able to tell if those changes "worked" in the sense that lay people could relate to and understand what the new indicators implied for the ecological setting being

described. (The project setting would be Appalachian forests, chosen because accessibility from ORNL would be easy if on-the-ground testing turned out to be necessary.)

It was this second challenge that got the group mired in discussions about the meaning of "value" and "valuation." The economist suggested that a way to test understanding would be to ask a random sample of lay individuals to judge among three alternative forests, described using the newly constructed indicators. These forests would be presented to each respondent in pairs, and the respondent would be asked to say which s/he preferred, or, equivalently to the economist, to say which s/he would be willing to pay more to visit for a day of recreation.

If the result for a person were a transitive ordering of the three forests, as in A>B, B>C, and A > C, then the indicators had been understood and "related to." If the result were cyclic (A>B, B>C, C > A) it was proposed that we interpret it as evidence of a failure to communicate the meaning of the indicators. This did not go down well at all with the ecologists (or the sociologists and anthropologists, for that matter). It was not the possibility, found in laboratory experiments, of "preference reversals," which show willingness to pay and "preference" as being less tightly linked than economists assume (see Grether and Plott 1979), that bothered them. It was the idea that willingness to pay would be so casually equated to "value."

In the course of many exhausting and sometimes acrimonious sessions it became clear that "value" for most of the team referred to "principle," as in "freedom" is a basic "value" of U.S. democracy, rather than to something for which it is meaningful, or at least not offensive, to talk about paying. They could accept that food, clothing, and transportation are priced but not that a functioning ecosystem could or should be. One of the arguments made to support this position involved the assertion that the "value" of the forest should include all the "life-support" services it provided to humanity, such as CO_2 sequestration, and flood control, in addition to the aesthetics that make for a pleasant day's outing. But how could the respondents possibly have a clue about what those "values" might be?

This dispute about testing technique was never settled because EPA backed out of its commitment to the project with more than half the original life left to run. (A paper summarizing the slender results obtained to that point is Shiller et al. 2001.) The economist came away from the experience with an appreciation that "value" and "valuation' are seriously tricky words for ecologist/economist teams to come to grips with. But he never could see that the insistence on taking into account all the service flows from nature made sense when valuing alternative forests that differed from each other in only modest degree.

In Which the Argument Arises that, if Monetary Valuation is Inevitable, Start with Total Value

Roughly a decade after the problem of "valuation" set the indicator translation research team to wrangling, a conceptually related dispute between ecologists and economists broke up a panel convened by EPA to discuss and recommend methods of deriving estimates of the ecologically-based benefits attributable to air pollution control under the Clean Air Act (CAA). The economist's vision for such an effort has already been noted, albeit very briefly: the physical, chemical, and biological effects of the CAA controls would be determined from existing technical literature and the judgment of experts. The specifically ecological implications of these "forcing functions" would be teased out using predictive models, presumably the newest intellectual descendants of the ecosystem models of the 1960s and 1970s. Then economists would devise ways to estimate individuals' willingness to pay for the changes found. They would use either "indirect" methods that look for links between the changes and people's behavior in markets, or "direct" methods that involve asking people what they would be willing to pay to "purchase" those changes (see Freeman 2003).

Thus the economist's vision looks only at the predicted change in ecosystem functioning and the resulting flow of services. For the most part, it implicitly assumes that policies such as the CAA involve only marginal changes to the underlying conditions within which the ecosystems operate and thus will lead to only small changes in service flows. But what if, after a seemingly small human intervention, the ecosystem undergoes large, fundamental, changes? Ecologists tend to argue that we are sufficiently ignorant of the system dynamics for all ecosystems that we can almost never rule out such a result.

The ecologist's vision for getting at the value of ecological system changes should, on this reasoning, start from the total value of the system, either as it exists if it is at risk from the intervention, or as it might exist if the action contemplated seems to promise only improvement. And why draw lines around "an ecosystem"? If everything is really connected to everything else, such lines are not only arbitrary but also possibly dangerous. Rather the starting place should be the total value of the world's ecosystems, as estimated by Costanza and collaborators (Costanza et al. 1997).

It was apparently a disagreement over whether or not to start the EPA panel's work with the Costanza total value, and try to pro rate changes in that value to reflect the limited operational scale of the CAA, that pushed the panel ecologists and economists apart. The economists criticized the prescription as, on its face, unhelpful because they could see no way that this total value was relevant to what had apparently been marginal changes in functioning attributable to the CAA. They also criticized in some detail the methods used by the Costanza team in arriving at the total value. But the ecologists felt their concern about uncertainty, reflecting ignorance, was being shunted aside by fellow panelists who were not trained to appreciate the depth of that uncertainty—and who were, in any case, famous (or infamous) for always being ready to assume the existence of the proverbial can opener. The long and short of the tale is that the ecologists walked out, and the rump of the panel, mainly the economists, was in a position to impose its vision by default. But the economists were left without the ability to get the ecologists' insights and judgments concerning the available predictive models of the systems putatively affected by the CAA, and without that guidance it would be hard for policy makers to know how realistic a vision had been presented. (In writing this "tale" we have relied heavily on a USEPA [2001] report and on first hand accounts provided by V. Kerry Smith and A. Myrick Freeman.)

Linking the Experiences: Uncertainty, Risk Aversion, and Views of "Value"

We believe it is useful to attempt to tease out and make explicit the links among the recounted experiences; in our view they tell us more when seen as pieces of a single puzzle than when simply recounted ad seriatim. Thus, at the time of the first dispute, when the ecologist, hired exactly for his modeling skills, dismissed the entire cost-effectiveness modeling project as a waste of time, the economist's reaction was, in turn, to dismiss that gesture as mere politics. It seemed that the call for automatic zero tolerance for human impact on nature through pollution was just an example of a new Ph.D. imitating the then-current antics of Commoner, Watt, and Erlich, who were much in the public eye. But, seen in combination with the other experiences, the initial refusal to play the game can be seen as quite likely an implicit acknowledgement of ignorance, and the choice of extreme risk aversion in the face of that ignorance.[3]

In the second tale, to the economist the arguments over "value" seemed to be just hugely counterproductive distractions from the task of designing a research methodology. The claim that asking people to "value" days of recreation in different forests was in any way akin to asking them to put a price on "freedom" seemed merely perverse. Further, the fall back position, that any purely recreational "value" would potentially be misleading because it ignored differences among the forests in their ability to provided basic life-support services to humanity generally, while possibly correct in principle, seemed over the top as a practical matter. After all, how much *could* these forests actually differ in the latter regard? But, if one starts from the belief that tinkering with ecosystems, even with the best of intentions, *can* lead to disaster, then emphasizing the total value of what is at stake, or even implicitly making that total value infinite by analogizing it to matters of fundamental principle, may be seen as rational, if extreme, risk aversion. The *fact* that the research team was not going to be tinkering with any actual forests, and thus would not be risking actual disaster, was apparently easy to lose sight of in the hothouse atmosphere of the infrequent team meetings.

In the third situation, real ecosystems *were* at stake. The CAA exists and has real effects on the ground. The danger for the risk averse might well be seen to be the possibility of undervaluing its accomplishments. That would more likely be avoided if the base from which benefits were measured were destruction rather than simply a slightly less desirable state. This line of thinking may or may not have

been explicitly behind the insistence of the ecologists that the panel's methodological recommendations start from the Costanza et al. total value number, but it does provide room for cross-disciplinary discussion. As it happened, discussion faltered and then stopped more or less completely.

This interpretation of the "tales" amounts to the definition of a theme that helps in understanding the difficulty of eco/eco collaboration. We suggest that the outline of this theme runs as follows: Ecologists are aware of linkage, complexity, and ignorance as they look at the natural world. They find it difficult, even impossible, to ignore the chance that ecological disaster will result from human intervention in that world, even if its proponents might characterize the purpose of the intervention as "pro nature." Thus, total ecosystem value is never irrelevant: the end point of an intervention *could be* disaster, and the potential damages, if monetized, enormous. Decoupling of parts of the affected system, or the assumption that small interventions make only for small changes in the system involved, do not come easily.

Economists, on the other hand, especially micro economists who dominate applied policy fields, including environmental and resource economics, are trained from their earliest courses that assuming decoupling (though that word is not generally used) is the best way to make progress in the analysis of policy problems and potential solutions. Assuming given prices or quantities (or both) in every market but the one being studied comes as naturally as breathing. Economic "collapse" (almost) never need be considered in the set of possible outcomes resulting from a candidate policy. Small changes in rules or incentives structures may be assumed to produce correspondingly small changes, both in size and in reach, in the outcomes observed in the world. And those small changes are what the valuation problem is (almost) always about. From this background, economists ask ecologists for help with predictions of the (small) changes in natural world outcomes, proposing to use their disciplinary tools to supply values for the changes. They are taken aback, to say the least, when they are instead lectured on the importance of the life support services of ecosystems and the real meaning of "value." But they often do not have the time (in the setting constructed for the proposed collaboration) or background to allow them to understand whence come these lectures. The dialogue all too often never gets down to the real issues of ignorance, risk of disaster, and how to do policy analysis in such a setting.

Can Anything Be Done About Mismatched Concepts, Concerns, and Conclusions?

While we believe that the above analysis helps explain the difficulties ecologists and economists have experienced in attempts at collaboration, we do not see that it leads to a simple prescription for changing things. Neither discipline can tell the other it is wrong and expect to be heard, let alone to have an effect. The bases of the "models" that underpin habits of analysis lie deep in decades of methodological development and graduate education. Understanding may lead to tolerance, but collaboration is not a product of tolerance so much as of shared concepts that include the nature of the problems being addressed and the tools for addressing them. Ecologists and economists see different problems when they look at the interactions of humans and the environment, and they are taught to apply quite different assumptions to the analysis of these interactions. We do, however, want to suggest one negative and one positive, if limited, conclusion bearing on the question: Can anything be done?

The negative conclusion concerns the notion that a "safe minimum standard" (SMS) approach can be helpful as a basis for eco/eco dialogue. In the 1990s, the SMS idea was espoused by individuals interested in furthering that dialogue (see Norton 1995) and analyzed sympathetically by card-carrying economists (see Toman 1994). The idea was (and is) that the SMS approach to policy decisions should come into play when the stakes arguably involve long-run irreversibility and large spatial impact. For quickly and easily reversible, small-scale decisions, the ordinary economic approach that looks for "efficient" solutions via cost-benefit or cost-effectiveness analysis (CBA or CEA) would be acceptable. Unfortunately, if the reasoning presented above is correct, the argument between ecologists and economists in nearly any policy setting is likely to be about exactly the issues of how irreversible and how large scale the impacts of the decision are likely to be...about the nature and extent of the risk being faced. If one follows Toman and Norton by conceiving of the SMS vs. CBA decision as involving placing the problem setting at issue within a box, the

axes of which are degree of irreversibility and extent of impact, and then observing where the problem is located relative to a pre-decided frontier within the box, the terms of the arguments as traced above may be changed but the substance will be the same. Ecologists are very likely to want to place most problems further out on the impact and irreversibility scales than are economists. Maybe such a shift in terms would be a healthy thing, but we are deeply skeptical that it will lead to real progress.[4]

So, what can we offer by way of positive suggestions? Only this: We believe that progress can be made on the applied level, through the establishment of multi-disciplinary teams that stay together for long periods and work, at least in the beginning, on fairly narrowly defined problems. Time is essential for getting beyond the arguments from conflicting principles. Narrow problems will help to focus attention on real systems and options rather than generalized "ecosystems" with abstract properties and ill-defined human "interventions." Unfortunately, there are few institutions around that have the money and mandate to offer real support for such efforts, where "support" would ideally include not just salary but the currency of academic respectability, such as some version of tenure. In the U.S., we have been told that the Institute for Water Research (IWR), part of the Army Corps of Engineers, offers this setting, though whether the academic link from IWR to academe is sufficiently strong to give young scholars career flexibility is unclear. Russell has spent almost a year at the Beijer Institute, part of the Swedish Royal Academy of Sciences, and can testify that it *is* such a place. (For example, a major ongoing project there has involved ecologists and economists in studies of the problem of nutrient discharge to the Baltic Sea, with the consequent problems for aesthetics, recreation and ecosystem structure.) U.S. universities are never likely to find it easy to play in this game because of the dominance of traditional departments and schools in the administration of the reward structure. Tenure in traditional departments rewards publication in "core" journals of the discipline, not collaborative papers in interdisciplinary journals, or, worse yet, for an economist, papers in ecology journals, or vice versa.[5] Perhaps the existence of graduate programs in "ecological economics" (as at Rennselaer Polytechnic Institute) and of an eponymous journal can be taken as evidence that, in the longer run, the new discipline embracing eco/eco collaboration will successfully be established in academe. This certainly cannot be ruled out, but at this point, assuming it is going to happen would seem to be a risky strategy for the young person who wants both to collaborate across the eco/eco line and hopes for a successful academic career.

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Notes

1. This notion is something of a lightning rod for controversy—philosophical, theoretical, and empirical. For an introduction to the issues see Argyle 1986; Fischhoff 1991; Lane 1993; Slovic 1995; and Sagoff 2004.

2. That CBA is also controversial is hardly news. Bromley, 1990, does a very fine job of explaining why, even from a narrow economic perspective, it must be taken with a large grain of salt. In a broader view, its addition of (presumably self-regarding) individual valuations flies in the face of the intrinsically communal nature of most environmental decisions (Blamey et al. 1995).

3. It is by no means clear that the young ecologist involved would, or could, have provided this interpretation if pressed. In the beginning, it seemed that he believed it would be straightforward to use the modeling tools he had been taught to capture the estuary's aquatic system operation. Only later did he, and his colleagues, discover how hard this was. But the Delaware was a very dirty river then, and the news was full of foaming and even flaming rivers elsewhere, so opting for zero discharge may have seemed a sensible way to put pressure on those responsible, roughly what the 1972 CWA actually did.

4. It is worth pointing out that the work of Ready and Bishop (1991) puts in doubt the value of the SMS, even as a risk averse approach to policy decisions. They show that whether the SMS is, in fact, going to strike interested parties as truly risk averse in a species extinction decision setting depends on the nature of human ignorance about the species in question when the decision whether or not to set an SMS must be made. The common variety of ignorance about the vast majority of species would lead to rejection of the application of the SMS because in the absence of knowledge we would have no basis for valuing

them, in contrast to the "development" alternative, the choice of which would lead to their loss. The finding that ignorance vitiates the apparent protective role of the SMS ought to make us veryuncomfortable.

5. Since we began sharing drafts of the chapter upon which this brief paper is based, we have had pointed out to us more than once, as evidence that we are flogging a dead horse, the existence of papers in *Science*, *Nature* and other journals, jointly authored by some of the great names in both economics and ecology. We may be perverse, but we are inclined to believe the horse is still breathing, even kicking. When you are famous enough, you can do what you want; and when you are mellow enough and face a broad enough charge, you can easily finesse the problems that trip up lesser mortals trying to build an operational model or produce an actual monetized estimate of ecological damage or improvement. These joint statements are good things, but they do not substitute for a strategy of fostering collaboration in the trenches where the real work is and will be done.