

THIRTY YEARS OF WATER AND ENVIRONMENTAL ECONOMICS

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The plan for these essays, supplied by our editor, suggests beginning with a paragraph on how each author got in to the water field. He says that in his case it was “a fluke.” So it was in mine. I went to graduate school aiming to become a development economist, based on what I had seen and learned in nine months or so living in Uganda and working as an Research Assistant (R.A.) for an economist who was doing a book on the East African economy as it then was. To make a long, and to me painful, story short, I had after my second year determined that I would not be able to find a thesis advisor from among the several Harvard development economists. In the midst of the resulting depression I was asked to lunch by Henry Jacoby, then running the Harvard Water Program for Bob Dorfman and Harold Thomas. He was looking for a graduate student on whom to lavish some support. It took about five minutes for me to see the wisdom of abandoning development for water/environment.

In the fall of the next academic year, I was sent out to Worcester, Massachusetts, to meet with Bob Kates and his geography grad students at Clark who were, with support from Resources For the Future (RFF), studying the very severe northeast drought of the mid 1960s. Apparently our editor, in his then-role as head of RFF’s water program, had strongly suggested the addition of at least an economics graduate student to the project team. This led in turn to:

- A dissertation on “Drought and Water Supply Planning,” jointly authored with Kates and company and accepted by Bob Dorfman with the comment that interdisciplinary work would be the wave of the future. (Right for me but, in my experience, wrong for the profession.)
- A one-year appointment at RFF’s water program to make a book out of the dissertation (Russell, Arey, and Kates, 1970).
- An acquaintance with Blair Bower, who, with Allen Kneese invited me to stay another year or two and transfer to their Quality of the Environment division.

- Ultimately to 17+ years of very satisfying work at RFF, some in water *per se* (I could never escape the drought study completely, nor did I really want to) but most in the environment more broadly.

RFF was a great place to be, especially in the early days, as environmental economics was being created by Blair, Allen, and John Krutilla through their own work; the work of collaborators such as our editors Bob Ayres, Bob Davis, and Bob Haveman, Tony Fisher, and Kerry Smith; and that of grantees at universities across the U.S. I would attribute to the experience of those years a career-long interest in the preservation vs. development debate broadly; and the U.S. penchant for damming every river in sight, narrowly. I also became involved in a running debate with Allen on the choice among environmental policy instruments, especially the scope for using economic incentives. Later I had a chance to get into benefit estimation through a project for The Environmental Protection Agency (EPA) involving the adaptation of “indirect” methods to the problem of freshwater recreational fishing benefits. More or less simultaneously, as division director, I was able to help support Robert Mitchell and Richard Carson as they crunched out their book on the “contingent valuation” method (Mitchell and Carson, 1989). In the process I became at least literate in the area, though not an active practitioner until very recently.

Now, that last paragraph was not just a blatant violation of the one paragraph rule for reminiscence. Read it rather as a segue into what will pass for substance, because what I want to note briefly here are three “triumphs”:

- In the world of water and dams, I think one can read the history of the last 30 years as the triumph of cost-benefit analysis (BCA) or at least of common sense.
- On the question of policy instruments it seems to me that economic incentives, at least in the form of marketable permits and at least in the U.S., have triumphed. There is another, to me more troubling, triumph as well; in development and environment one

is hardly allowed to question the wisdom of recommending emission charges to developing countries.

- In benefit estimation, direct survey techniques seem to be close to triumphing over the traditional indirect, or revealed preference methods, as the methods of practical choice for actual project analysis.

COST BENEFIT ANALYSIS AND DAMMING THE WEST

It seemed that the challenge of developing analytical tools with which to address the Hell's Canyon dam proposal pushed John Krutilla to make operational the notions contained in his justly famous paper, "Conservation Reconsidered" (Krutilla, 1967). This threat to the last great undammed canyon in the U.S. may be seen as the high water mark, if you'll pardon the expression, of the dam building excesses of the 20th century. Dams are beautiful and seem further to be powerful symbols or celebrations of human capability, especially in the face of the awesome power of nature in the untamed west. So powerful were they as symbols that the underlying reality was ignored or lost. The massive transfers from the rest of the nation that principally helped a relatively small number of large-scale farmers grow crops also growable (and grown) in other, naturally watered, parts of the country were not the focus of policy statements or project analyses.

If the water resources field generally was the incubator of serious methodological improvement in cost-benefit analysis, then the great western dam building period can be seen as the power source at the heart of that incubator. As methods were refined it became clearer and clearer that, from a national efficiency standpoint, these dams were disasters. Such results were, of course, not enough in themselves to stop further dams (not all so dramatic as Hell's Canyon). But it seems in retrospect that the steady drumbeat of negative evaluations was important in giving politicians cover for objecting - whether they were at another level moved by the income transfers or later by awakening environmental concerns.

Therefore, I give credit to the developers of CBA, the consultants to and writers of the Green Book, the so-called Senate Document No. 97, and those to be found in Kneese and Smith (1966), for wearing down the political foundations of these monuments to hubris. The resulting analyses set the stage for Jimmy Carter's "Hit List," and for the adoption of local contribution rules that drove the stake through the heart of the enterprise.

ECONOMIC INCENTIVE POLICY INSTRUMENTS

When I came to RFF, Allen Kneese and Blair Bower were making the case for emission charges. They pointed to the operating system in the Ruhr area of Germany (then West Germany) and encouraged a variety of efforts at implementation in the U.S., including one involving Rick Freeman in Maine and another in Vermont that was pushed by John Hansen, a young lawyer who spent a year at RFF. There was even a flurry of national level excitement about a tax on SO₂. I think it is fair to say that nothing ever came of these political efforts and, on the more purely economic side, expressions of skepticism started to be published. Lost, for the time being, was the dual notion of marketable permits to discharge pollution, a suggestion identified with Dales and Crocker (Dales, 1968; Crocker, 1966).

But, as the Clean Air Act's provisions for punishing areas not attaining the new national ambient air quality standards (KNACKS) began to threaten to bite and wreak political havoc, people inside EPA dreamed up the "bubble," "offset," and bankable emission credits. Taken together, these amount to a marketable permit system, albeit one that was heavily fenced about with restrictions designed to reassure bureaucrats worried about "losing control." As a practical matter, the cobbled together system allowed new businesses to open in nonattainment areas. As important for the longer run, people on all sides of the debate began to notice that the allowed trades led to cost savings.

The potential of permit trading caught the eye of would-be brokers for such trade and even impressed some in the environmental community. Here I would give major credit to the Environmental Defense Fund (EDF), always the friendliest of the groups toward economic arguments.

The inclusion of an SO₂ permit trading system in the Clean Air Act amendments of 1990 may have been the key to getting something done at last about acid rain. It seems to have broken the log jam caused by the Eastern vs. Western coal fight and the costly requirement for limestone injection and scrubbing that resulted (Ackerman et al., 1981). (That the price of permits quickly fell was a reminder of the dangers of taking seriously politically motivated policy cost estimates.)

The apparent success of the SO₂ trading system seems to have made tradable-permit believers out of even the Gore camp in the current administration. They have put a global system of tradable carbon emission permits on the international climate-change agenda. The promise of this

system, confirmed insofar as one can believe the cost models (and insofar as one believes the reductions will actually be made), is that it will make global CO₂ emission reductions hugely cheaper by allowing the U.S., Europe, Japan, and Australia to buy reductions in the poorer nations where massive energy and transport inefficiencies are rife. The political problem now is that the Europeans object on what appears to be moral ground; the U.S. would not suffer enough for past and current profligacy.

The success of tradable permit systems and the recent discovery of the potential “double dividend” of any pollution control instrument that brings in government revenue, seem together to have created a widespread wave of enthusiasm for the application of such instruments in the developing country context. “Solve all your problems at once and suffer no pain in the process,” is only a slight exaggeration of the message. Twenty years after I used to argue with Allen about just how desirable emission charges could really be claimed to be, I find myself once more in the role of a somewhat lonely skeptic about the wisdom of a wave of enthusiasm that tends to sweep away objections.

DIRECT, SURVEY METHODS OF BENEFIT ESTIMATION

Perhaps the most dramatic sea change of the past 30 years has involved benefit (or damage) estimation methodology. When I was a graduate student, Samuelson’s dictum that people would answer strategically if asked about their WTP for a public good was accepted as the last word on the subject. Bob Davis’ early work notwithstanding, the possibility that it would be useful to go after WTP directly via interviews was not taken at all seriously by the profession.

What has happened to turn things around, so that journal editors complain of being flooded by papers about what has come to be called “contingent valuation” (CV)? I would give a large share of the credit (or blame if you happen to think badly of the approach) to Alan Carlin at EPA. Alan managed to find and protect the money that supported most of the early efforts, concentrated in Ralph d’Arge’s group at Wyoming. These efforts began identifying problems for such techniques and even suggesting possible solutions. Perhaps most importantly, they failed to find evidence of pervasive strategizing in the responses. (This work also produced the CV tag, which was devised to avoid the word “survey” which was a red flag to OMB reviewers.) Such reviews resulted in demands to expand samples, chosen in sophisticated ways

which in turn implied much higher expense than the EPA project budgets allowed.

At roughly the same time, Peter Bohm was doing his famous TV show experiment in Stockholm (Bohm, 1972). This also showed immunity of respondents to the efforts of the survey designer to provoke under and over statement (“free riding” and “overbidding”), which is to say strategic responses. But Peter would and does stress that it shows a tendency to state substantially higher amounts for WTP in situations described as not involving any payment.

Other researchers took up the challenges as well, including Robert Mitchell, then at RFF, who had experience in survey work. He, in turn, brought in Richard Carson; and the two of them eventually parleyed an EPA (Carlin) cooperative agreement into the 1989 “bible” for the field.

The largest single intellectual event over the next decade was the publication of the NOAA “Blue Ribbon Panel” report in 1993 (NOAA, 1993) that gave a stamp of fundamental approval – subject to caveats about how to go about it – and may finally have finished off the Samuelson objection on the basis of the number of Nobelists involved in the panel. This, in turn, was part of the afterglow from the huge practical event, the grounding of and spill from the Exxon Valdez in Alaska. This produced enormous amounts of money for studies that were themselves undertaken with the aim of supporting or undercutting claims for damage payments from Exxon.

Ten years later, it is not a stretch to say that the direct methods are triumphant, at least in the sense that they have become the dominant benefit estimation technique. For example, when benefit estimation is attempted for water quality improvement projects being proposed for Inter-American Development Bank loans, the method of choice is referendum-style CV. (“Would you be WTP X for the described water quality improvements?”) Indeed, dealing with the problems of, and suggesting and refining alternatives to, the “traditional” CV approach are the dominant intellectual challenges these days in environmental economics journals.

CONCLUSION

All in all, the last three decades have been an extraordinary period to have been working in water and environmental economics. There have been huge changes in both the practical and intellectual worlds and these, in turn, have been driven by exciting confluences of people

and events. The only depressing feature of it all is that the rate of output of the field has so increased that no one, except perhaps Kerry Smith, can read fast and long enough to begin to keep up. Specialization becomes more and more necessary for those who want to make a contribution. Those of us who started in the '60s were lucky enough to be able to move around easily in what was then a fairly empty start-up field.

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