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The Personal Journey of a Resource Economist

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Abstract

This perspective article begins with speculation about my early interest in conservation at age six and traces my personal development until I became an assistant professor. My contribution to the beginning and development of nonmarket valuation, including an early publication on the stated preference method, is included. All but one of the discussed articles was about a nonmarket context. I also explore my research on endangered species in general and the spotted owl and black rhinoceros in particular. The arc of interest represented in my publications embraces biodiversity. For example, one article covers a metapopulation model, whereas others discuss the bioeconomics of antibiotics and an early treatment of uncertainty in a public utility setting. My reconsideration of the analytical and empirical resource scarcity literature in the field is distinctive, while the necessity to work in an interdisciplinary setting is shown as transparent.

INTRODUCTION AND LIFE THEMES

A strong memory I have is of my grandmother admonishing me at about the age of 10 to be careful how long I took a shower. We were on a well in very rural New Hampshire, and it could run dry. My grandmother was nondirective, very rarely telling me what to do. So, this one-time advice sank in—and perhaps her remark pointed me in the direction of becoming a resource economist.

One other boyhood memory strikes me as relevant to this essay. I worked on a farm the summer before my eighth grade. It was there that I conceived the idea of raising chickens as a plausible path to making a decent living. Percy Woodward, the farmer, was a true New Englander and exceptionally demanding. At 13, I accompanied him to a freight car half full of 100-pound bags of grain. Like him, I was expected to carry them to his truck at a swift pace. Always competitive and driven, I expected he would remark favorably on my performance. But he said nothing. Nor was this experience a sample size of one. I was also responsible for feeding and watering 5,000 chickens daily, among other chores. At this point, I made a major decision: I could not trust the judgment of others in authority. Henceforth, I would make up my own mind. "Have doubt" is the general take-home lesson here: Instead of assuming that what you read, hear, or are told is true, consider the possibility that it is not and challenge yourself to find where it is vulnerable. As I would much later learn, this is a crucial approach that Karl Popper (1959), in *The Logic of Scientific Discovery*, urges of scientists: Work to falsify hypotheses; check your hypotheses for flaws rather than trying to prove them right.

Some youthful experiences—the fervent Christian Science of my family, moving around a lot, and an acute awareness of being poorer than others at a prep school I attended briefly—imbued me with a certain outsider mentality. Lack of money and the struggles of my single mother to support us on a meager salary also sparked a strong interest in money and how to make it.

My mother and her siblings were athletic and interested in many things—sports, jazz, opera, travel, painting, and the outdoors—which continually tickled my curiosity. My mother also used reason in dealing with me. She would only let me have my way—for example, to take the car, at age 16, to ski in Vermont—if I could make a good, cogent argument for why I should be allowed to do so. Her approach caused me to think about what I was doing and to learn how to reason well.

I've always had a tendency to speak out and act on what I believe, even in high school. Later on, this propensity would lead me, as a graduate student, to argue against my department chair before a California legislative committee on the issue of setting a minimum wage for farm labor.[1] Likewise, when I was an assistant professor without tenure, I appeared before a university investigatory committee and presented a case against my University of Washington (UW) chair, who tried to fire another assistant professor on grounds, as I argued, that violated the professor's civil rights. He lost the case and soon after lost his chairman position.

A large dose of randomness entered into my decisions, for example, about college and the choice of U.C. Berkeley for graduate school. It might be more accurate to say that what strong self-confidence and a burning need to think and act independently can do is to attune one to seeing and seizing opportunities that arise.

After high school, I took what now is called a gap year. Part of the time I spent working in Florida, where I had biked from Boston with two friends. After a few months of 110-hour weeks working as a bellhop and parking cars, it became clear to me that simple hard work wasn't an agreeable path to either intellectual stimulation or wealth. One day, when talking to a rich guy

[1]Whereas the chair used a fixed coefficients linear program, with no factor substitution, to demonstrate how farmers would be made worse off if farm wages were increased, I obviously argued that farmers substitute when relative input prices change, thereby reducing my chairman's biased estimates of the owners' loss. Nevertheless, the minimum wage legislation passed. A few months later, the tomato harvester developed at U.C. Davis was introduced.

vacationing at one of the hotels, I asked him what I could do for a living if I liked to read the *Wall Street Journal*. "Business or economics," he said. Clearly, I needed to go back to college and was lucky enough to come across Antioch (Ohio) in Lovejoy's Guide to American Colleges (Straughn & Lovejoy 1955), which was organized alphabetically by state.

Antioch dramatically changed my energy, politics, religion, horizons, and purpose in life. The work-study program was an ideal way to pay for a good education while learning about new aspects of life. Arriving at Antioch, I still had in mind to be a chicken farmer—it was a feasible occupation that solved the how-to-earn-money problem. But on my various co-op jobs throughout my college career, I was exposed to new experiences and became aware of unexpected possibilities. The motto of the college was "more than books."

Most Antioch job periods were 10 weeks long. Working for UPS in New York City to earn money, I learned about the city, went to museums and plays, and gained an essential introduction to a wider culture. Being competitive, I liked trying to match stock certificates and names faster than anyone at Marine Midland Bank on Wall Street, but the work itself was boring. So, I considered being a stock broker. In a job at Merrill Lynch, Pierce, Fenner, & Beane in Boston, where I was an errand boy or "iggy," I was placed in the hub of research and information and found it very exciting. But in looking for that job I had met a manager at Putnam's who subsequently invited me to lunch. In the course of conversation, he made clear that, to do research, a stockbroker had to have family wealth. Otherwise, one was just a glorified salesman. That avenue was closed.

While working on water projects in Washington, DC for the Economic Research Service in the US Department of Agriculture on yet another job, I somehow heard about Resources for the Future very soon after it began. I walked over there and applied for a 6-month job for my final Antioch work term. The crux of my experience was working for and getting to know John Krutilla rather well. He was interested in what later became natural resource economics—dams, saving wild rivers (because he liked to fish), etc. I also met Allen Kneese and Blair Bower, who were concerned about water quality—what was later called environmental economics. There were no titles to these fields in the 1960s.

When I started college, the notion of graduate school had never entered my mind, but during my last year, most of my friends were applying. The idea of going off to explore the subjects that by now engrossed me was irresistible.

Accepted at both the University of Chicago and U.C. Berkeley, I chose economics at Berkeley merely because of the attraction of the West Coast. Wow! What a political and professional difference that was, I was to learn. I'll skip the politics, but I went to graduate school interested in development rather than natural resources because I naively thought that, in agricultural economics, you learned to drive tractors and feed cows better.

Coming from a small, friendly, community-oriented college, I was dropped into a huge university and a department where professors' offices had no names on the doors. You courageously met your professor in the central office "bull pen" if needed. There was a rigid format for the courses taken in the first year in graduate economics. Always independent minded, I didn't want to do it that way. So, I took economic development in my first year. It was a big mistake; I simply didn't have adequate preparation.

Another experience I had of my own limitations was when Frank Hahn, an extraordinary mind, visited U.C. Berkeley. He taught the basic theory courses and assigned us to write a paper on consumer's surplus (CS). What an irrelevant idea, I thought. Why couldn't I write about something more substantive? How wrong I was, and how fortunate I was to be exposed to a concept that was to become the workhorse of applied economics, in particular, nonmarket valuation to which I was to become a modest contributor. [Later, when I responded with CS to a question in a subsequent graduate agricultural economics course, the illustrious professor told us that CS had no place in economics because of what Samuelson had once written. See Samuelson (1961, p. 195).] What was true? It took me a few more papers to decide.

One good thing during my first year in graduate school was that agricultural economics[2] students enrolled in the graduate economic theory courses. Clearly, I realized, they were not just learning to drive tractors over there, but learning, as I was, about Hessian bordered matrices and other arcana. I changed departments at the end of my first year in grad school.

In the more welcoming agricultural economics department, faculty and students personally interacted and played bridge together during the coffee hours. This was a much more human atmosphere.

It wasn't just a push from economics, or an attraction to agricultural economics, however, that led me to change departments. It turned out that for me, the application of economics to real-life problems was also critically important and is reflected in many of the papers I wrote after my PhD.

I had a crisis of conscience. How could I take a job teaching economics when the basic assumptions made no realistic sense to me? The discipline of neoclassical economics was premised on the assumption that people are rational, that everyone has access to the same information at the same price, that individuals have perfect expectations and maximize (a trivial notion of utility for starters), and that the microeconomic world is perfectly competitive. Production functions are only constant returns to scale. I believed this framework to be false. To teach this material, therefore, was hypocritical. So, after my PhD, I dropped out for a year and went to Europe with my partner, at least in part to sort out my academic future.

In graduate school I had audited two courses on the philosophy of science: one taught by Karl Popper, who fortuitously was visiting Berkeley, and the other by Paul Feyerabend, his former student.[3] During that year away, I read a great deal of material on the philosophy of science in order to resolve my personal conflict with economics. It's surprising how little I remember now of this fairly momentous moment. But the gist of it was a realization that a field has to be judged by a dynamic path, not at one point in time. Put too simply, one could formulate a model with few variables, study it thoroughly, and then expand the variables in the model over time. Or, one could start with a more realistic model. I found no evidence that one of these paths was epistemologically superior to the other. Since it was an intellectual standoff, accepting the familiar economic methodology seemed reasonable. I remember one quote from Popper (1959, p. 59): "Scientific theories . . . are nets cast to catch what we call 'the world': to explain, and to master it. We endeavor to make the mesh ever finer and finer." Making the mesh finer and finer is the dynamic element, where it is incumbent upon the field's practitioners to progressively relax the original simplifying assumptions in order to encompass more of the world. Here, my pragmatic self clung to hope that improved economic models would become more credible over time, and I stayed in the field.

How has economics evolved since 1964? My biased opinion is that the profession has made substantive progress. For example, Joseph Stiglitz, Michael Spence, and George Akerlof won the Nobel Prize in 2001 for the topic of imperfect or asymmetric information. Stiglitz argued that only under exceptional conditions are markets efficient. Just this past year, Richard Thaler won the Nobel Prize for developing behavioral economics, where people behave predictably

[2] Willig (1976) pointed out that in *Foundations of Economic Analysis*, Samuelson (1947) concluded that CS is a worse than useless concept "because it confuses." It was natural then for Morey (1984) to follow on with "confuser surplus," as the profession struggled with the concept.

[3] For an entertaining methodological aside, have a look at Milton Friedman's "The methodology of positive economics," (1953) or just skip to Samuelson's response (1963) calling Friedman's ideas "the F twist." Friedman argues that the unrealism of assumptions is irrelevant. A good theory is one that predicts well. In one class, Feyerabend commented about the high correlation between birth rates in the geographic divisions in Germany and the number of stork nests.

irrationally, in ways contradicting the standard economic theory. Other Nobel Prize winners identified as behavioral economists include Akerlof (1971) above, Robert Shiller (2013), and Elinor Ostrom (2009, economic governance of the commons). It is natural to include Daniel Kahneman, a psychologist who won the Nobel Prize in economics in 2002, in this group. Kahneman's work violated traditional ideas for decision making under uncertainty and further paved the way for behavioral economics. And then there was Thomas Schelling and Robert Aumann, receiving the Nobel Prize for writing about conflict and cooperation, which were surely topics outside and differently focused than the material I learned in my theory courses. Another example of progress is Paul Krugman's Nobel Prize in 2008 for reintroducing economies of scale after more than a half century's lapse of their inclusion in the profession. He freed the profession of a serious straightjacket of constant returns to scale. A final example of substantial progress toward making economic theory more empirically relevant is Arrow's (1962) article on learning by doing, cited over 13,000 times.

Back in the 1950s and 1960s, a lot of intellectual furor arose over what the character of economics should be. In particular, there was a schism about the role mathematics should play in the discipline. U.C. Berkeley's Department of Agricultural and Resource Economics was heavily committed to mathematical economics in coursework and publications. Many of the graduate students in my cohort additionally obtained master's degrees in statistics. I was interested in natural resource economics, and this group, led by Siegfried von Ciriacy-Wantrup, was in the institutional economics camp.

A major chance event, however, was to change my intellectual direction in a fundamental way. When I submitted a rough draft of my PhD dissertation (Brown 1964) on the California Water Plan (CWP) to Julius Margolis, a member of my dissertation committee, he first said that I should be glad that he was old because this was a terrible dissertation, "but I don't care anymore." Then, a bit later he offered me a consulting job and arranged for me to talk with C.B. (Bart) McGuire (full professor at U.C. Berkeley without a PhD!) about a research project that involved determining the optimal water price to charge seven irrigation districts, which were members of the Kern County Water Agency in California, some of whom had access to groundwater. It is fair to say that no economist knew more about the CWP than I did. Bart laid out a discrete time intertemporal maximization model. My job was to empiricize it for each of the different irrigation districts. It was a cutting-edge model, and we did not know of any other applied model resembling the mathematical formulation of this one. It was my first paper, coauthored with Bart, later published in *Water Resources Research* (Brown & McGuire 1967) and republished several times.

Two key points stood out in my mind about this article at the time. First, was this research on an important topic? Yes. And could I have solved the problem with the institutional training I had? No. I'd made a training error. Lots of remedial work needed to be done. Second, a natural policy to come out of this formulation was an optimal pump tax for groundwater extraction. It turned out that California law had provisions for a groundwater pump tax. In another setting, a water law professor once told me that it was like "walking on the ceiling" to read what economists wrote. So in principle, I was relieved to discover that our model was institutionally feasible.

SOME IDEAS I'VE HAD

Being hired at UW was largely due to chance. Because James Crutchfield had a deep habit of overcommitting himself, I was hired in 1965 when research funds were pouring in to study water topics in the state of Washington, and no economist on the faculty was thought suitable. Crutchfield welcomed me with my interdisciplinary interests and mentioned a number of faculty interested in resource issues throughout the university with whom I might talk.

Salmon: Willingness to Pay and Willingness to Sell

During one conversation, Steve Mathews, a biologist in the Washington Department of Fisheries, spoke about the importance of sport salmon fisheries. For example, how much expenditure to enhance salmon migration could be justified when dams blocking salmonid passage are constructed? Our discussion led to a joint research project on the topic.

Robert Davis (1963) had developed the contingent value method in which, loosely speaking, one asks sports salmon fishermen, in our case, about their willingness to pay (WTP) for the right to fish. [I should have known about this concept earlier on, because Ciriacy-Wantrup, (1947, 1952) with whom I had been a research assistant, had written about using "the 'direct interview method' to measure values associated with natural resources. . ." (Mitchell & Carson 1989).]

Our research was published in Brown & Mathews (1970), a Washington Department of Fisheries research bulletin. Despite the obscure source, I include the article here because we were the first researchers to ask contingent valuation nonmarket WTP questions after the Davis (1963) contribution (see Mitchell & Carson 1989, appendix A). We were also the first to ask respondents to imagine that, if they had a right to fish, what would they sell it for (willingness to sell, or WTS). This was the first article, to my knowledge, that discovered the empirical difference between WTP and WTS. The reported WTS was much higher than the reported WTP. The wealth effect of owning a hypothetical salmon fishing right cannot cause a significant difference between WTP and WTS responses when the budget for the good is modest (see Willig 1976). The prevailing theoretical view at the time was that WTS should approximately equal WTP. At the time, I concluded that our research design must be faulty, so I never submitted the paper for review and publication in an economics journal. Yet as readers know, virtually all articles that were subsequently published on the subject have found this same discrepancy. A published page from an unknown source in my files, summarizing 15 contingent valuation method experiments during the period 1974–1984, concludes that WTS estimates are three to five times greater than estimates of WTP.

For many years, this branch of research, regardless of its status in the economics profession, has grown and taken up a very substantial fraction of space in our field's journals; for example, it comprises more than one-third of one journal's several issues that I randomly checked a few years ago.

The Price of Public Services Under Uncertainty

When I was a graduate student in California, I enjoyed skiing, climbing, hiking, and camping in California state parks. California parks had two qualities of parks that I'll call "low-class" and "high-class." The latter had hot water, showers, and other amenities lacking in the low-class parks. Entry prices to the parks did not and still do not cover the cost of the parks. Since I always chose the low-class parks, given my financial status, I thought it would be productive to show that high-class parks were more heavily subsidized. California had exceptional data on park participation, including data on excess demand from each park because of full capacity. Primitive campsites cost less than the cost of high-class sites, but the difference in occupancy rate resulted in a lower per-site subsidy for the high-class, more expensive sites. I am embarrassed to say today that I did not submit the paper because of my political bias.

However, thinking through that analysis (and data) raised a really interesting problem. Surely, park attendance in the summer is not the same good as park attendance in the dead of winter. To put it simply: When did the peak period stop and the off-peak period begin? This quandary led me to write my first article that was countable for tenure, with my colleague Bruce Johnson, on

"The price and output of public services under uncertainty," published in the *American Economic Review* (*AER*) (Brown & Johnson 1969).

By my reckoning, when I entered this article for publication, our department at UW was the third-best, right-wing economics department in the United States. Coming from U.C. Berkeley, I did not fit in. The challenge for me was to get tenure in an inhospitable political environment. I asked my department chair how many articles I had to publish in the top 10 journals. I do not remember his exact hurdle, but I did get tenure with his support, because I met his criterion with articles in *AER*, *Review of Economics and Statistics*, and *Journal of Political Economics*, plus two replies to the *AER* article with Bruce Johnson.

Ducks: Empiricizing Intertemporal Maximization with a Renewable Resource

In graduate school, I became more familiar with the plight of migratory farm labor. I was involved a bit with organizing farm labor. At one point in this involvement, I read or heard that there was more legislation for protecting migratory birds than for migratory labor. This subsequently led me to look into the question of alternative flyways for birds, including the Pacific Flyway, which extends from Alaska to Patagonia. It was a rich opportunity to combine my work in nonmarket valuation with an intertemporal model of population dynamics, which became the first published empirical bioeconomic model in resource economics.

The mallard duck species breeds and produces juveniles in Canada in the late spring, then migrates south in the fall where the birds are harvested in different states, which each have hunting restrictions. The ducks winter over in Mexico and return to Canada in the spring. There is an international externality between Canada and the United States, others between the states, and yet another between Mexico and the United States.

I informally contacted John Krutilla at Resources for the Future, described my research proposal, and requested some financial support for the cost of the survey—I recall $5,000—to which he immediately agreed. It's striking what can be accomplished informally, it turns out, with minimal transaction costs on both sides, if there is trust and the availability of modest funds. As mentioned earlier, back in 1958 when I had a work-study job at Resources for the Future, I interacted a good deal with John. The intellectual bonds that Crutchfield had built with the School of Aquatic and Fisheries Science at UW, and those I had furthered as well, gave my graduate student Judd Hammack and me immediate access to Gerald Paulik.

Gerald introduced us to fishery population dynamics. We/he assumed that ducks would behave as fish, at least as a starting point. However, revisions were essential. This was the first time the Beverton–Holt model had been used in a resource economics model because of the necessity to introduce age structure and much more carefully describe the production of young. Ducks require ponds (May/June) to successfully raise surviving juveniles, so we had to transform a constant in the fishery production function to a function.

Thanks to our interdisciplinary fisheries research, our population dynamics equation was better than the model/analysis being used by the mallard population specialists in the US Department of the Interior. Ours provided a superior fit to the data. This was in no thanks to us, but rather to interdisciplinary research. We had two mortality coefficients: one for humans and the other for nature. Interior had one, implying that when human-based mortality increases, natural mortality automatically decreases pari passu.

The intellectual contribution here is that natural resource economic models must faithfully capture the real world, at least or if only synoptically. Along with the physical population dynamics, we also had developed and estimated the nonmarket valuation (the fifth study according to Mitchell and Carson) of how much hunters were willing to pay for a marginal duck. Summarizing the empirical results very briefly, the amount of kill per state among the states proportionally was not so far off, but we concluded that the harvestable stock should be greatly increased by increasing the population.

Canada was subsidizing the destruction of ponds for more grain acreage for grain export. But ponds had a positive shadow value in Canada for the production of mallards to shoot by hunters in the United States. So, one could compare the duck value of a pond with its wheat value. The institutionally satisfying company Ducks Unlimited, with a recent annual budget of $US224 million, claims it is the world's leader in wetlands conservation. It reassuringly provides the necessary bridge between the viewer and hunter benefits in the United States and the opportunity cost of preserving or adding ponds in Canada.

Our book, *Waterfowl and Wetlands: Toward Bioeconomic Analysis* (Hammack & Brown 1974), was named the Publication of Enduring Quality by the Association of Environmental and Resource Economists in 2007.

Biodiversity with a Growth Model

Several decades ago, when the idea of biodiversity was warming up, Timothy Swanson, then at the University of Cambridge, organized a seminar on biodiversity, which I attended. At that time, ideas about biodiversity, particularly from an economic perspective, were quite fluid, to put it kindly. Less kindly, I couldn't understand what the papers were about. So, I decided to think about the subject from a more formal perspective. I had recently read parts of *The Theory of Island Biogeography* by MacArthur & Wilson (1967). Both authors were trustworthy natural scientists. They posited the function

$$S = A^{\mathrm{a}},$$

where S is species, A is area, and $\mathrm{a} > 0$ is a constant.

That seemed a good place to begin, because it joined land—a factor of production of "normal" market goods—with species, a nonmarket good. This model, I could see, was going beyond my comparative advantage, so I asked Robert Rowthorn, of Cambridge, if he was interested in the topic. In our economic growth model (Rowthorn & Brown 1999), people enjoy species and other goods directly, and both depend on land. In this model, because species are a nonmarket capital good, one solution is to alter the discount rate to achieve the desired socially optimal mix of species and other goods. The intellectual challenge here is to find a congenial physical production function that joins the novel concept of species with a recognizable economic variable, land. This was achieved with the MacArthur and Wilson equation. What was the moral of this story? Read beyond economics.

Renewable and Nonrenewable Resistance: Antibiotics

When I was in graduate school, I learned that one of the negative spillovers of the Vietnam War was the evolution of resistance of gonorrhea, so prevalent among servicemen, to the treatment of penicillin. Resistance had risen so much that other drugs like tetracycline and fluoroquinolones had to replace penicillin. I wondered if resistance, or what I thought of as its reflection and effectiveness, was a renewable or nonrenewable resource. I read a few technical articles about this topic. If it was a renewable resource, as many antibiotics are (Rowthorn & Brown 2002), or even if it wasn't, this was a socially important topic for resource economists.

In the case of renewable effectiveness, the objective was to determine the rate at which the stock should be drawn down before it was withdrawn, replaced by another antibiotic, and allowed to recover to some optimal reintroduction point. For nonrenewables such as Gentamicin and Tobramycin, what should be its pace of effectiveness decline (Laxminarayan & Brown 2001)?

Another article, "Resistance economics: social cost and the evolution of antibiotics," written with David Layton (Brown & Layton 1996, p. 355), particularly satisfied me. We concluded that: "Put provocatively to emphasize the point, when both humans and animals use antibiotics (for economic weight maximization), we are equating the marginal economic value of improving life a bit more with extra pounds of beef; i.e., the exchange rate between life and meat."

A Metapopulation Model

Just before I took a sabbatical at Stanford University, I attended an interdisciplinary workshop in Stockholm, then introduced myself to Jonathan Roughgarden, a well-known biologist at Stanford, and asked about his research interests. This led to a productive collaboration (Brown & Roughgarden 1997) involving the addition of a spatial ingredient to the research. One of Roughgarden's specific interests—barnacles—has a two-phase life cycle, as do high-valued species, such as scallops, clams, crabs, lobsters, and shrimp. Adults contribute to the first or larval stage, or "larval pool," beyond the shore to form, in effect, a commons. This pool then naturally distributes recruits to local sites along the coast, where they develop into adults. In principle, some form of private property is feasible along the coast, so a rental rate on the adult harvest is conceivable and is conceptually in order.

The paper with Roughgarden included the first metapopulation model and one of the first spatial models published in renewable natural resource economics, Sanchirico & Wilen (1999, p. 130). Further, there is the novelty of increasing returns, as discussed below. Its implications are that some biological dynamic models do not obey economic model structures. Larvae are produced under increasing returns by adults, so some shore sites should be the source of the adults that produce larvae. The flip side is that sites with a comparative advantage in harvest should produce harvest. The striking conclusion is that the most profitable harvesting strategy involves harvesting at only one site. The adults at all the other sites will travel offshore and contribute to larvae production to capture the increasing returns. If this result is shocking, that is an encouragement to improve the biological dimensions of the theory.

The reality seems to be that bioeconomic dynamics often violate economists' working assumptions. When nonconvexities (that are not mere curiosities) inhere in major ecosystems, where equilibrium is not unique and one equilibrium is superior to another, then the design of a public management policy has to be more complex than simply arranging for a suitable market pricing structure (Brown et al. 2011, Dasgupta & Mäler 2004).

Hedonic Valuation of a Nonmarket Resource-Open Space

In the early 1970s, I was aware that a lot of research funds were available for research on coastal zone management. Because I knew that parts of the Boston waterfront had been converted to living spaces, I wished the Seattle waterfront could become more people and amenity friendly. I wanted to use hedonic analysis to show that it would pay to open up the aging commercial fisheries shoreline to multiple purposes for people with different tastes.

Griliches (1961) had identified hedonic analysis as a way to value the individual characteristics of a composite product, but I worried that we might encounter statistical problems due to limited sample size and too much heterogeneity, so we chose a simpler problem. If the empirical model worked for a homogeneous sample space, then we could tackle the more complex waterfront space.

Basically, what is the WTP for incremental space to view an open waterfront? Fortunately, Seattle has many lakes. Around each lake is a variable width or setback, which represents the distance from the house to a lake, together with many other characteristics. It was, in part, a standard hedonics model, but it valued novel, nonmarket characteristics. The good news is that the research was published. The bad news for me was that the coefficients in the regression were too fragile. If one introduced another variable like distance to downtown, then the key values of the variables jumped. So, regrettably, we did not pursue further that "heartfelt" objective.

There was an interesting conceptual element in this problem setting. As one increases the width of the open space around a lake, the value of that characteristic enhances the value of the houses in the neighborhood of the lake. However, the opportunity cost of expansion is the loss of developed property values (taxes) foregone from not building on open space, a value that can be factored into the analysis at the county level.

Natural Resource Scarcity: Why Barnett and Morse Were Wrong

The subject of growing natural resource scarcity has interested economists since Malthus and earned us our reputation as a "dismal science." The topic has been of much more general interest. Some may recall the famous bet between Julian Simon and Paul Ehrlich (and in addition, two U.C. Berkeley physicists, J. Harte and J. Holdren) in 1980. Simon challenged anyone to bet, against him, that prices would not rise for the price trend of commodities, which were believed to be an important index of resource scarcity. The combatants settled on $200 each for five commodities: chromium, copper, nickel, tin, and tungsten. Ten years later, three of the five metals had declined, and Ehrlich et al. paid up.

In 1963, Barnett & Morse published *Scarcity and Growth: The Economics of Natural Resource Availability* (1963). The book received widespread citations (1,540 at last count) and was uncritically supported by well-known authors such as Mishan (1973) and Blackman & Baumol (2008). My first course in natural resource economics required that I write a review of *Scarcity and Growth*. It did not sit well with me then, for reasons I no longer remember. My suspicions nagged me for years, however, until I had the opportunity to revisit the concept of scarcity with my colleague, Barry Field (Brown & Field 1978). Barnett & Morse (1963) set forth three measures of scarcity. The first is natural resource extraction cost, defined as labor (L) cost per unit of extractive industry output (Q) or L/Q.[4] Beware! This is called a cost, but in fact, it is merely a physical measure. The second measure is real product price for goods such as cotton, frozen fish, or lumber. A rise in product price indicates an increasing natural resource scarcity of the natural resource input. Third is the rental rate, the amount an owner of a natural resource would charge one who plans to extract or harvest a unit of the natural resource.

Barnett & Morse's (1963) reasoning behind the first measure, L/Q, is that as a natural resource grows scarcer in a Ricardian world, a lower-quality resource is extracted, and it takes more labor (and/or capital) to do so. Therefore, L/Q should increase with increasing scarcity, Barnett and Morse argued. They found that the unit cost for a variety of resources fell, and at an increasing rate, from 1870 to 1950, thus refuting the hypothesis of increasing scarcity. Forestry in the United States was the major exception. The authors concluded that the unit cost index is faulty if there is technological or social change. It is more basically faulty because L/Q is a physical, not an economic, measure. More technically, assume a constant elasticity of substitution production function

$$Q = (aL^{-c} + bR^{-c})^{-1/c}, \quad 1/(1+c) = s \quad c > -1, \qquad 1.$$

where L is a composite labor-capital input, R is a natural resource input, and s is the elasticity of substitution. Manipulating the equation so that the ratio of the marginal product for each

[4]Barnett & Morse (1963) also introduce $(aL + bK)/Q$ where K is reproducible capital.

factor equals the factor price ratio in competitive factor markets and making that substitution into Equation 1 yields

$$L/Q = \{a + b[ra/(wb)]^{cs}\}(1/c), \quad 2.$$

where w represents the price of the labor-capital input, and r is the price for the natural resource.

Imagine two outputs with the same parameters captured in each case by Equation 2 but differing in the elasticity of substitution. When the price of the natural resource, r, relative to w rises in each case, then the interpretation of Equation 2 is that the unit cost, L/Q, registers the greatest increase where substitution is the easiest This is a perverse conclusion, because $d(L/Q)/ds > 0$.

The unit cost of extractive output alarms us with dramatic increasing scarcity, when in fact, technology embedded in the elasticity of substitution has made it easier to decrease the use of natural resources. For example, as steamships became better substitutes for sailing craft, followers of the unit cost measure of scarcity would have grown even more strident in their demand to preserve the tall pole timber for masts.[5]

It is important to distinguish between product price and extractive price (rental rate). Others, including Nordhaus (1974), have used product price as a scarcity measure of the input. Clearly, other factors, including the cost of intermediate inputs through time, intervene to mask scarcity elements of the natural resource. Surely, relative prices are the appropriate measure of scarcity, but what should be the numeraire? Nordhaus's price relative to the wage rate fell for all 11 minerals he studied. When we computed the price of minerals relative to the price of capital, as measured by Jorgenson & Griliches (1967), four of the indices showed significantly increasing scarcity. The other seven mineral prices fell relatively more slowly when the price of capital replaced the wage rate as the numeraire.

The conceptually ideal measure of economic scarcity is the price or rental rate for the resource, when it is sold to the extractive sector or its exchange value in situ. According to Barnett & Morse (1963), rent is not a measure of scarcity "in a world of depletion . . . [or] of socio-technical change." When there are "changes in interest rates, relative demand and expectations concerning future resource availability, . . . advances in rent on unhomogeneous resources are an ambiguous indicator of increases in scarcity."[6] Barnett and Morse must have forgotten Debreu's extremely careful definition of a factor or a good necessary for an axiomatic definition of economic equilibrium (Debreu 1972). If any of its characteristics change, it's a different good. The reference quoted above, to unhomogeneous resources, lacks relevant substance in this discussion.

We brought out more sharply the distinction between the product price and resource price/rental rate by a careful and formal discussion of the intervening variables in time, space, and other characteristics separating product price from resource price, which have to remain constant over time for the substitution to be valid.

There are a few more important points to be made about the distinction between rental rate and unit cost. Unit cost captures a static view about value. Rental rates are forward looking in that resource owners and buyers take into account prospects for future discovery and future costs of extraction when settling on a rental rate.

Resource rental rates are usually proprietary information, so empirical studies are infrequent. Fortunately, the US Forest Service owned substantial stocks of Douglas fir stumpage that it auctioned off over time, thereby making resource price data public. From 1910 to 1970, the stumpage price-rental value varied relative to Douglas fir lumber price by a factor of 12. Much

[5]In an old reference now lost to memory, Tony Scott cited this example.

[6]Marshall (1959) argued that "all rents are scarcity rents."

more informative is the inverse relation of the stumpage price to *L/Q*. Not only did the stumpage price rise dramatically from 1910 to 1970—increasing scarcity—while the unit cost, *L/Q* fell dramatically—declining scarcity—but every time the unit cost fell, the stumpage price rose, and vice versa.

Barnett & Morse (1963) is one of the most highly cited publications in resource economics, probably ranking in the top 30 or even better in terms of citations. But it is intellectually vacuous, as I believe Barry Field and I thoroughly demonstrated. Having lived through this period, I wonder why bad economics prevailed? I wonder if it is because the contemporary economics profession simply believed, as an article of faith, that if there is a scarcity stimulus, there automatically will be a successful mitigating innovating response.

The editor handling our manuscript questioned in a neutral way why the ideas in this paper warranted publication in a mainstream journal. We pointed out that the whole fabric of natural resource economics has a strong public component, e.g., one-third of the nation's land is owned and publicly managed. We wrote that the stewards of natural resources do not bear directly the rewards or costs of correctly identifying an accurate measure of resources or resource scarcity in particular. Poor indices can be chosen by policy makers and remedial antidotes may be misplaced, poorly timed, or unnecessary and wasteful.

Endangered Species

A natural focal point for research about natural resources is the Endangered Species Act. Jason Shogren and I wrote a paper entitled, "Economics of the Endangered Species Act" (Brown & Shogren 1998). The title is a bit of a misnomer, as the 1973 US Congress specified that economic criteria would not be included in either the listing or the designation of proposed critical habitat to preserve the species.

The US Supreme Court scrutinized the legislative history and concluded that the trend toward extinction should be halted and reversed, whatever the cost. However, budget constraints enable only a few species to be listed as threatened each year. Private landowners have an incentive to destroy the habitat of species—and do so—before they are listed in order to avoid restrictions on habitat use for gainful purposes. There is a prescription against ranking species for listing, but in fact, the species preferences of employees of the Endangered Species Program (US Fish and Wildlife Service) are reflected in the species listed. Birds are listed more frequently than reptiles. Recovery expenditures are not driven by science but by species in conflict with construction or other forms of economic activity. Approximately one percent of the species listed have been removed because of satisfactory recovery. We made recommendations for improved performance, such as providing rewards to landowners for good stewardship of actual and prospective habitat and species thereon.

Two articles estimated the demand and supply of distinct endangered species. One covered the northern spotted owl, with a focus on the probability of survival, and the other discussed black rhinos, with what was at the time a very unusual proposal to decrease the market price of horn below the opportunity cost of poacher's entry.

A controversial paper I wrote with David Layton (Brown & Layton 2001) bears on managing black rhinos, an endangered species highly valued for their horns. There has been a ban on trading black and white rhino horn for many years. During this time, the black rhino population fell from 65,000 in 1970 to about 2,500 in 1998, and it has fallen dramatically since then. The economics discipline has explained theoretically and documented empirically the grave difficulty encountered when legislative fiat is used to control demand.

Due to poaching, black rhinos are now locally extinct across substantial areas of Africa. African rhino horn has no close substitutes. Both black and white horn is used as a traditional medicine to treat persistent fever in China and Korea. Much horn has been used for making dagger handles in Yemen, and for cultural reasons, the material has few substitutes. Demand is very inelastic, and the price has mushroomed, making poaching extremely attractive. Anti-poaching efforts have been inadequate for the task of preserving rhinos.

David Layton and I proposed to save the black rhino population by removing the ban on trade in rhino horn. The model involves the key African governments suddenly driving the price of white rhino horn down by putting their substantial inventories of horn strategically on the market until the price is below the reservation price, which represents the marginal cost of poaching. Speculative stocks are held with the expectation that the population will increasingly begin to look like a nonrenewable resource and that the price should rise.

By bringing large public inventories to the market, we argued, the speculators' price expectations will fail, and they will adjust their inventories accordingly, further putting downward pressure on price. Moreover, the neat element in the model is to introduce cropping of white rhino horn in an optimal way, because the horn is in fact a renewable resource. Although white rhinos are distinct from black rhino biologically, they are identical economically. The white rhino population is substantial, so it can readily support cropping.

Because horn is a renewable resource, like trees, it might be thought that the Faustmann approach would be used in the real world to determine the optimal time to harvest the horn. However, this is not true. Under legal trade with cropping, rhinos would not be scarce. The optimal rotation time occurs when the cropping cost is minimized and when the horn is approximately at its maximum size.

We empiricized the model. As stocks build up, dehorning can be decreased because of natural mortality occurring to increased stocks. Using prices at that time, for example, more than one-third of the million-dollar profit from rhino horn sales can be earned by African countries by cropping less than 20% of the white rhino population. With insignificant poaching pressure, the black rhino population could grow to 11,000 in ten years' time.

When I presented our ideas to the African Rhino Specialist Group—all biologists—about two decades ago, they thought them weird and summarily dismissed our proposal without any consideration. However, during the ensuing years, I have read of a few biologists proposing the Brown and Layton idea for preserving the black rhino.

What is the Marginal Cost of Increasing the Survival Probability of an Endangered Species—the Northern Spotted Owl?

My reasons for writing the spotted owl paper (Montgomery et al. 1994) are twofold. First, preserving the spotted owl, listed as threatened under the Endangered Species Act, was an enormous policy issue in the Pacific Northwest, and ultimately involved President Clinton mediating a solution in a meeting held in Portland, Oregon. So, it is natural for an economist to wonder how one could contribute to the controversy. Much too simply, the spotted owl's habitat is old-growth timber, which is very valuable. The owl's range comprises parts of Washington, Oregon, and northwestern California. In order to "save" the spotted owl, millions of acres of old-growth Douglas fir with high-opportunity cost would have to remain vested. Second, because species survival is uncertain, the appropriate conceptual unit is not "saving" per se, but the likelihood of survival and how certain we want to be about survival. That is a pretty unique idea to measure. How we did it was novel, and unprecedented, I think.

Biologists carefully refrained from using likelihood of survival in the analysis of various conservation proposals. To get at survival indirectly, we sent an inquiry to a number of owl biologists, requesting that they draw the cumulative distribution of the owl population exceeding 500 pairs at the end of 150 years as a function of the number of nesting owl pairs. This would give us a cumulative distribution to work with.

The physical marginal product in terms of board feet for each of hundreds of timber districts was estimated, taking into account some complex spatial dimensions, such as clustering habitat areas to support clustering owl pairs. The Endangered Species Act requires habitat to be preserved throughout the species' range. So, both high and low marginal productivity areas had to be included when estimating the marginal cost function. A well-known Timber Assessment Market Model (TAMM) was then used to estimate the dollar welfare loss in stumpage and all related markets for a 50-year period.

Regarding the mid-level protection proposal, to go from 90% to 91% protection involved an estimated cost of $1.4 billion and an estimated $2.6 billion per percentage point to achieve 95%.

In a later unpublished National Science Foundation–supported research project, David Layton and I created and empiricized the third stated preference study, preceded by the work of Adamowicz et al. (1994). Although the paper was presented at the American Economic Association meeting in 1995, it was not submitted because the results were too fragile owing to small sample size. Nevertheless, we learned that respondents did not value the northern spotted owl very highly, in part, because the owl is rarely observed. However, its habitat, the ancient Douglas fir, which is viewed and enjoyed in a variety of recreation activities, is enormously valued for its majesty. The estimated value of old growth was in the neighborhood of 70% of the total value. As a result, the value of the joint product supported the mid-level federal recommendation that was adopted for habitat protection to preserve the northern spotted owl.

How Important Are Marginal Changes in Ecosystems due to Global Climate Change?

A policy group for the private electric utilities in the United States invited me to investigate how people across the country valued climate change. I would prefer to present only the highlights of the resulting publication (Layton & Brown 2000), but some background is essential for understanding it. The research was a stated preference study. A critical issue in the narrative was whether climate change will or will not occur as a result of nature. Layton and I argued that, because the research was being conducted on behalf of the electric utilities, they should be interested in customer behavior in response to the most likely account of the causes of climate change. That would involve human contributions to the change. After all, that is the level of dissatisfaction to which the utilities will have to respond. One or more individuals on the advisory group proposed that we use a natural causes story that we all knew would have produced a lower cost estimate.

The leading nonmarket researcher we hired to conduct our focus groups tore up our set of key questions—which supposed scientists being uncertain about the timing of climate change—and substituted his own. He argued that people cannot respond well to questions set in an uncertain background. Shortly into the first focus group meeting, a woman said, approximately, "Well, I just read an article in the *Readers Digest* that said uncertainty surrounds climate change." We went back to our initial proposal. Uncertainty allowed us to motivate natural variations in the four menus that each respondent faced, in which the costs and physical changes due to global climate change on forests were varied.

This is a natural lead-in to the next challenge. A famous member of the advisory group said that the research design was faulty, because respondents get tired when filling out as many as four or five alternative situations in which prices vary or other elements in each menu change. Thus, their answers will be skewed depending on the order in which alternatives are shown. In response, we simply doubled the sample, presenting one-half the respondents with menus 1, 2, 3, 4, and the other half saw menus with 4, 3, 2, 1. There was no statistical difference in the responses. It surely pays to have a generous budget!

The setting for our study occurred on the Front Range in Colorado, where there is an abrupt demarcation between a prairie grassland system and mountain forests to the west. We supposed, with scientific support, that the density of trees decreases with elevation gain and would more rapidly decrease with time. Respondents objected to our primitive characterization of changed environments due to climate change, so we hired a professional computer specialist who masterly prepared subtle "photos" of the changed environment, at different elevations and different times, due to different climate changes. Again, a larger budget allowed for greater complexity as a couple of examples show. A friend, who was senior in the environmental section of the Weyerhaeuser timber company, warned me that one will get quite different responses if one uses "forest," "timber," or "lumber." She was right.

We supposed in one scenario early on that scientists could offset climate change by increased fertilizer of the natural trees. We were quite surprised that members in the focus group strenuously objected to "messing with God's plan."

Out went that option. In our random parameters multinomial probit model, the question design allowed us to check whether respondents were rationally consistent. For example, WTP increased monotonically as the amount of forest loss went from 6,000 feet in elevation to 7,900 feet. As economists would expect, the estimated marginal cost function is nonlinear.

Behavioral Economics and the Future

In 2000, I wrote an essay on renewable natural resources economics in the *Journal of Economic Literature* (Brown 2000), in which I strongly recommended increased attention be devoted to spatial natural resource economics. Without claiming causation, it is encouraging to see much greater attention paid to the spatial element in resource economics models during these last 17 years. Now I recommend incorporating behavioral economic themes in resource economic models in the future. To this end, Dan Hagan and I held a workshop in 2010 on that subject (Brown & Hagan 2010). That's just a mere drop in a potential bucket of research opportunities that exist today.

Interdisciplinary Economics

There is more essential work to be done within the framework of interdisciplinary economics. When we prefix economics with natural resource or environmental, we are obliged to go beyond our field and become informed by other disciplines. On the production side, in the interest of reality we need production functions. For example, Hammack and I had to learn about fishery population dynamics from a fishery biologist. In estimating the value of an endangered species, the northern spotted owl, I had to obtain the functional relation between the probability of its extinction and the size of the population. The article incorporating biodiversity in an optimum growth model with Rowthorn turned on the log-linear species area function set forth by the biologists MacArthur & Wilson (1967).

On the demand side, to capture how people behave, we typically lack markets that provide their money metric values for characteristics of the resource or environment important to them. We are well aware of the major contributions the field of psychology has made in the design of nonmarket survey designs and more generally to behavioral economics in this regard.

One has to be prepared to venture out of one's comfort zone and to be rebuffed due the close-mindedness of potential co-operants who fail to see the opportunity for challenging work. It takes colleagues who have a predisposition to be open and respectful of the other's persuasions and discipline or points of view. Such collaborations have been infrequent but exist. Having worked in several failed meetings and committees, I am impressed with the unusual success from the skill and care with which David Policansky put together special committees for the National Academy of Sciences to study politically sensitive problems. Representatives from business and environmental interests, universities, and federal agencies had to produce a substantive report with unanimous support.

FURTHER THOUGHTS

Early in this review, I spoke about the exceptionally strong assumptions underlying the neoclassical model if an efficient solution is to occur. I should have included the assumption about perfectly contingent future markets emphasized by Debreu (1972), because most of the papers I have published are concerned with a missing market and nonmarket valuation.

The publication by Hammack & Brown (1974) was an early example on optimal control with nonmarket value estimates of waterfowl values and was very prominently featured on graduate class reading lists, I've belatedly learned.

If I were young and seeking attractive researchable areas, I would look for a problem involving two interdependent resources, e.g., where valuable swept leaves from growing trees draw down the stock of nitrogen in the ground. Similarly, when economists expand their horizon of natural resource problems involving biological functions, I have a strong suspicion that nonconvexities will be ubiquitous and be a fruitful area for exploration, particularly including policy analysis (see Dasgupta & Mäler 2004).

The cost of saving an endangered species can become impressively expensive. Economists can also usefully contribute to this sensitive policy issue.

My journey from wanting to be a chicken farmer when I entered graduate school to getting an honorary PhD degree from Gothenburg University in Sweden included the luxury of being paid to generate challenging problems and come up with the publishable, interesting story. To use a cricket term for which I don't have an American equivalent, I've had some good innings.

This essay would be very incomplete if I did not mention one of the most stimulating opportunities of my career. Around 2000, I had a sabbatical coming up. Quite fortuitously, the environmental economics program in Gothenburg was looking for a substitute for Thomas Sterner, who was going to be away for a year. It was a congenial match that continued over a period of years, involving some teaching and guiding a number of graduate students at various stages of their dissertation while that program was getting under way, as well as my participation in several fascinating African research projects. I must also honor a few colleagues, scholars, and friends with whom I have had many thought-provoking conversations, but with whom I have not published. They include but are not limited to Partha Dasgupta, Martin Weitzmann, Robert Halvorsen, Thomas Sterner, Anthony Scott, William Schulze, Ronald Johnson, Alan Haynie, Mike Ward, Giulio Pontecorvo, and my wife, Victoria Farr Brown.

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