Valuing Natural Resource and Environmental Amenities: Can Economic Valuation Be Made Defensible?

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The topic of this address may seem somewhat remote from the traditional interests of agricultural economists. Yet it has been of central concern to those of us involved in natural resource and environmental issues. Given the direction in which agricultural economics is currently moving, it may soon prove to be as perplexing a problem in that arena as well.

If not before, then certainly since, Just, Hueth and Schmitz's book appeared on our shelves, the field of applied welfare economics has become an integral part of the economics of public policy. For many years the notion of normative analysis in economics was the subject of great controversy. Today we have something of a consensus on defensible empirical practices for measuring welfare changes associated with policy actions in the context of private markets. In the words of Just, et al. "these many (recent) advances fit together to constitute a complete methodology for applied economic welfare analysis."

This methodology applies to welfare measures associated with price changes for market goods. Yet many of the normative issues which economists have, of late, been asked to address involve nonmarket commodities. They require valuations of public goods and as such cannot draw directly on the recently established consensus in applied welfare analysis.

The problems which I refer to here are most commonly associated with environmental and resource economics. What is the value of improving air quality? or reducing thermal pollution? or of regulating hazardous waste disposal? But agricultural economists cannot be too complacent. Increasingly, problems at the interface of agriculture and resource economics have been more visible—witness the importance in watersheds such as the Chesapeake Bay of conflicts between the marine environment and agriculture production.

Despite the near consensus which currently exists in market oriented welfare theory, economists are far from embracing a complete methodology for valuing public, non-market goods. It hardly seems necessary to document this contention. We need only consider some of the many recent conferences which have attempted to resolve difficulties and increase consensus on these issues, (e.g., Southern Natural Resource Economics Committee, 1983; EPA Workshop on the State of the Art in Contingent Valuation, 1984). In essence "Nonmarket valuation has a long way yet to go before all the problems will be solved and its acceptance by economists will be unequivocal (SNREC, p. 4)."

The Current Direction on Nonmarket Benefit Evaluation

While there exist a number of approaches and a myriad of empirical attempts at valuing nonmarket goods, this work has one element in common. The valuation exercise is an attempt to bring these incommensurables—these nonmarket goods—into policy considerations on a comparable footing with private marketed goods. Market prices are seen as the standard of accuracy, as the appropriate measure of benefits. Expressed in its most extreme form by Sherwin Rosen at a recent EPA sponsored conference on contingent valuation, many see the task of the nonmarket benefit analyst as determining what the market would have done had it had a chance to operate and

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then applying standard welfare theory. These thoughts of market analogies pervade the literature. The most commonly debated issue has been the effectiveness of available methods for achieving this end.

To be fair, some economists and many non-economists have questioned the relevancy of the market analogy for public good valuation. Arguments by philosophers include reference to a social ethic and contend that societies may have collective values independent of individual preferences. Not so well articulated are our own concerns about how people think about public goods and how they relate public goods to private expenditures. To what extent can a public good be translated into an effect on an individual such that an individual's willingness to pay is a meaningful concept?

Such questions may easily cause us despair since they seem, at this time, unresolvable. Those of us working in nonmarket benefit evaluation have tended to take the position well articulated by John Stoll and others at the recent SNREC nonmarket valuation workshop:

> The extent to which one may view current attempts as desirable will be discussed here. It is taken for granted that, regardless of one's viewpoint, this process of moving extramarket items into the realm of markets is occurring . . . [O]ur viewpoint is that like it or not, nonmarket valuation will continue to be used in the future and likely to a greater extent than in the past (p. 4).

In what follows, I would like to offer a different kind of justification for pursuing our work, despite the controversy and doubts which surround it. My story provides not only a justification for continued work but an argument for the kind of work which will bring us closer to the resolution of the larger problem.

The remainder of the paper has two parts: the second part is an overview of some substantive work we are undertaking in the area of valuation of environmental improvements at the University of Maryland. However the first is somewhat philosophical in nature. I will not attempt to argue for some specific approach to valuing nonmarket benefits, nor assert that a complete methodology for benefit valuation exists. I will not even argue that economists necessarily are on the right track or that they have any business trying to answer some of these valuation questions at all. Instead I would like to offer some perspective on why we are in this apparent quagmire and what precedent there might be for spending our time and efforts trying to make some progress here.

Often our own specific endeavors so command our attention, that we are oblivious to the fact that similar struggles have taken place and are taking place time and again in our own and other intellectual disciplines. What we are talking about here is the development of our "science"; and what we need is a much broader perspective in which to consider that development.

The Development of Theories

Some time ago, Thomas Kuhn wrote a very insightful treatise entitled the *Structure of Scientific Revolutions*. Many who took courses in the history of economic thought in the 1970's were exposed to his ideas. Kuhn's thoughts may be considered passé by historians and philosophers today, yet Kuhn's description of the process of development of scientific theory seems germane to the development process in which we find ourselves.

Kuhn's story of development bears close resemblance to what we have seen go on in many fields of economics. Underlying Kuhn's hypothesis is that science does not develop in a linear fashion through accumulation. In the early stages of a science or a subfield of a science, there exists competition among a number of distinct views all somewhat arbitrary in their formulation. Each builds on a different basis with no common standard and chooses different supporting observations and experiments. Eventually a set of theories, in Kuhn's now familiar terminology—a paradigm, emerges which provides focus to future work.

The role of the paradigm is crucial. The paradigm is a far more general concept than the way in which we use the word "model." It reflects a view of the world, the set of fundamental concepts and theories which all additional work takes as given. Consensus over a set of theories; i.e. acceptance of a paradigm, provides the profession with fundamental concepts which are universally accepted and need not be continually reiterated. This allows, and in fact encourages, research to become more focused, more refined, and more detailed and leads to the construction of elaborate instrumentation (be it mechanical or

mathematical), as well as the development of esoteric vocabulary and skills.

Normal science, the progress of research, can proceed only in the context of a paradigm. It involves a series of puzzle solving exercises, where the paradigm serves to suggest the puzzles potentially solvable and the rules by which solutions will be judged. Once a paradigm is in place, the puzzle solving process is the normal progress of science and consists of the articulation of the paradigm; i.e. the testing of theoretical predictions against observations.

No paradigm ever explains completely and perfectly observations on nature (or in our case, economic behavior). When predictions of theory fail to match observation, it is difficult to determine whether the fault lies with the scientist's application, the precision of measurement, or the paradigm itself. Professions resist the rejection of paradigms, however, unless anomalies to the paradigm become so numerous and persistent that a crisis develops within the profession.

Interestingly it is the intensive work within the context of a paradigm, the continual refinement of concepts and improvements in measurement precision, which provides persuasive evidence to retain or reject the paradigm. The sense of crisis which stems from unresolvable problems provides the essential tension which leads to renewed debates over the legitimacy of methods, of standards of solution, and of problem definition itself. The existing paradigm is likely to undergo considerable modification during such crises in an attempt to encompass the new set of problems and resolve at least some of the anomalies.

Previously accepted concepts will be deemed invalid and rejected, only if another paradigm becomes available. The criteria for choice is not limited to the relative abilities of paradigms to predict observations, for none do this perfectly. Their aesthetic appeal as well as an arbitrary assessment of their abilities to handle future anomalies come into play.

The essential point, however, is that the business of science can only be gone about in the context of a paradigm. This body of accepted thought provides the necessary structure, fundamental concepts, rules, and standards of judgment, without which research becomes confusion.

Kuhn's view of scientific development lends insight into the historical development of economic theories and concepts. It also provides a means of placing in perspective our endeavors in the valuation of public goods and an idea as to how we might fruitfully proceed.

Whether we wish to view it as a preparadigm stage or a crisis in the neoclassical paradigm, the development of what has become "traditional" welfare economics (i.e. welfare measurement in private markets) provides a case in point. Welfare economics has a long history of controversy, beginning with loosely defined and imprecisely measured concepts of rent and consumer surplus extending as far back as Ricardo and Dupuit. The establishment of these concepts as foundations of a theory of economic welfare was a long and uphill battle involving attacks by new welfare economists on the old welfare economics and the development of the compensation principle. For a very long period the state of welfare economics was one of crisis, with applied economists pursuing empirical studies which theoreticians could only condemn. Over time, and with the help of economists such as Willig, Hausman, Just et al., Hanemann, and others, some theoretical justification for feasible empirical practices has emerged in the form of what I will call the "willingness to pay" paradigm.

With the recognition that public policies frequently produce benefits and losses outside of markets comes a new controversy and an attempt to stretch the existing "willingness to pay" paradigm to cover new ground. To many established economists, the problem seems straightforward: the valuation of nonmarket benefits through benefit cost analysis, under ideal procedures for extracting value measures, is assumed to provide the same answer that the market mechanism would provide. The major difficulties lie in defining those ideal procedures which seem so to elude our grasp. Others question whether these measures exist, or are meaningful, in the context in which we wish to use them—i.e., can the willingness to pay paradigm really be stretched and modified to resolve the anomalies which public good valuation present.

This subfield of economics—valuation of public goods—is in a period of crisis in its development, not unlike periods of crisis which have arisen in other areas of economics and in the "harder" sciences. Kuhn described these periods as marked by debates over legitimate methods, over relevant experiments and over standards by which results can be

judged—a description which fits closely the current activities in nonmarket valuation. In these periods of crisis, Kuhn argues, many speculative and unarticulated theories develop which eventually point the way to discovery. Either large scale changes occur in the existing paradigm or a new paradigm emerges.

There are two messages for us in Kuhn's theories about scientific development. The first is that attempts to verify or falsify paradigms conclusively will not succeed and are a waste of our time. No paradigm matches nature perfectly or completely, and we can never know what is "right." Scientific methods allow us to test a paradigm's predictions against observations of nature but not to test two paradigms against one another. Also failure to match predictions and observations is not conclusive, but merely generates anomalies which may or may not be resolved eventually with better and more precise measurements.

The second message, and in my mind most important, is that meaningful research cannot exist outside the context of a paradigm. "Measurements taken without a paradigm seldom lead to conclusions at all." In fact it is precisely in the context of the progress of research allowed by the paradigm, that sufficient arguments develop to reject the existing paradigm in favor of another. Normal science encourages more and more refined and precise analysis which either establishes a closer match between theory and observation or provides more evidence that such a match does not exist.

The implication of Kuhn's thesis is clear. The only way we can hope to make any progress in research is to work within the existing paradigm—whether or not we are convinced that the paradigm will in the end be the most useful. Can standard welfare economics—the "willingness to pay" paradigm—be stretched to resolve the public good valuation problem? The only way to determine this is to explore nonmarket valuation problems in a rigorous welfare theoretic framework. If time and again the anomalies cannot be resolved, even with increasingly careful modelling and precise measurement, then the balance will tip in favor of seeking a new paradigm. But it is only in the context of some carefully conceived theoretical structure that progress can be made; "truth emerges more readily from error than from confusion (Kuhn, 1969)."

In the next section I present evidence of anomalies in existing methods for valuing public goods and some attempts at resolving these anomalies. I think they provide some basis for encouragement, and I hope that my opening remarks do the same. My intent is to place in perspective the state of our labors in public good valuation. We have no reason to be ashamed of our failure to provide decisive answer's to these difficult questions—such crises arise in even the most reputable of sciences. In some ways we have less reason to be ashamed than other scientists. While parallels in development clearly exist between the hard sciences and economics, one factor distinguishes us. Scientists, Kuhn argues, pursue research which their existing paradigms suggest are potentially solvable. The paradigm insulates the profession from socially important problems which are not reducible to puzzle solving form. Economists, and other social scientists, have no such insulation and find themselves forced by public appeal into problems for which they may not yet have the appropriate puzzle-solving tools.

Making Nonmarket Benefit Measurement More Defensible

There is a good deal of confusion in nonmarket benefit analysis today—a fair amount of skepticism, rival theories and approaches, and a lack of consensus on means by which results can be judged. There is, in Kuhn's words, something of a crisis. Which, if any, techniques are right? Can existing techniques be improved? or are they hopeless? Are we trying to value things that economists shouldn't be valuing?

To reiterate, the only test we have as economists, the only test any scientist has, is the comparison of the predictions of theories with observations on the subjects under study. A profession develops by improving the rigor and precision with which theories can predict and observations can be measured. We come to reject a paradigm—a way of looking at the problem—only when sufficient discrepancies or anomalies arise during this process.

The only way in which we will gain any insight into whether we are on the right track is to define and estimate our welfare measures in the context of existing economic paradigms as clearly, precisely and rigorously as possible and then to see whether insurmountable anomalies arise. I make no argument that we

are on the right track, but that half-hearted, ad hoc, misguided and confused experiments can prove nothing and will not further our development.

The application of scientific method immediately raises difficulties in the area of nonmarket benefit analysis. Our approaches provide estimates of welfare for which we have no direct observations for comparison. But this is not so uncommon in science where frequently it is impossible to observe the object of study directly but only its effect. Our inability to observe welfare directly only suggests that we should define welfare measures on models of behavior which *can* be observed. What I will be discussing for most of the paper will be welfare measures which are derived from just such models of economic behavior. Here we have a well established paradigm in economics, can we stretch it—or better yet, how far can we stretch it, to address the valuation of public goods?

Starting, as they do, from models of economic behavior, one would think that welfare measures derived from models of observable behavior in markets related to environmental goods (e.g., recreational demand models) would be a popular approach. Certainly, the travel cost approach, a specific variant of more general models of economic behavior, has produced many benefit estimates in its long life. Yet this approach's credibility has been challenged of late on two counts.

First, policy makers argue that many amenities of interest cannot be associated closely enough with a market or with observable behavior to allow for the use of related market methods. This criticism has some very important implications. On the pragmatic side, it is useful to note the result of the recent EPA sponsored conference on contingent valuation. Contingent valuation, the only alternative method in contention, has been pronounced quite reliable as long as the good to be valued is closely related to a market experience. What is more germane to my arguments here is that when valuation is unrelated to observable behavior, it is impossible to test the predictions of theories against observations—and as a consequence we can have no confidence in those predictions. In fact, it is unclear that economic valuation has any meaning in a context where there exists no related observable economic behavior. We are reminded of Kuhn's warning "measurements undertaken without a paradigm seldom lead to any conclusions at all."

The second criticism of market related valuation approaches is that the same valuation problem can generate a vast array of radically different benefit estimates. How can one trust a method which appears capable of generating a number of very different answers to the same problem? Table 1 provides an example. Using one data set of 647 individual sportfishermen, we attempted to estimate an average measure of benefits associated with a season's access to sportfishing in the Chesapeake Bay. The example is not of particular importance for similar results can easily be obtained on other data sets. The essential point—and one which bears out the criticisms of this approach—is that from the exact same data set we have generated twenty-four estimates, all of which are perfectly reasonable and consistent with methods which have

Table 1. Benefit Estimates from Travel Cost Models
(Average benefit for sample of Chesapeake sportfishermen)

	Linear Function Time valued at wage	Semi-Log Function Time valued at wage	Linear Function Separate money and time costs	Semi-Log Function Separate money and time costs
Compensating Variation:				
Omitted Variables	$8977	$5735	$5331	$2580
Errors in Measurement	4694	3536	2898	1691
Ordinary Consumer Surplus:				
Omitted Variables	7571	4993	5053	2554
Errors in Measurement	4224	3383	2840	1685
Equivalent Variation:				
Omitted Variables	6691	4531	4922	2529
Errors in Measurement	4023	3247	2811	1678

been used in the past. The largest estimate is more than four times that of the smallest, yielding total benefit estimates for those individuals in the sample ranging from one to six million dollars per year.

The advantage of working within a paradigm—in this case, "willingness to pay" based on a model of individual behavior—is that we can, at least, identify the sources of the discrepancies in Table 1. The discrepancies are associated with different assumptions about behavior or about the decision environment. And these assumptions, with luck, can be tested. Methods which directly produce willingness to pay estimates provide no such means of resolving discrepancies.

The discrepancies in the estimates reported in Table 1 arise from four different sources. One is functional form. We have employed two commonly used functional specifications—the linear and the semi-log—both of which yield reasonable results and expected signs on coefficients. The second source of difference is definitional. For each functional form we have calculated the ordinary consumer surplus measure but have also derived compensating and equivalent variation measures. This is achieved (à la Hausman and Hanemann) by integrating back to an indirect utility function and solving for the expenditure function.

A third cause for discrepancies in estimates is due to the specification of time costs in the estimated function. In one case time costs are introduced directly; in the other, time costs are translated into money costs at the wage rate. The final source of discrepancy is the treatment of the error. Depending on the interpretation of the individual's disturbance term, different welfare measures will result. Other discrepancies could be generated by considering, for example, different aggregation schemes as well.

Looking back at Table 1, it is interesting to note that the one source of discrepancy which has received the most attention in the literature is the least important in this example. Willig has warned us to expect small differences between ordinary surplus (OS) and compensating (CV) and equivalent (EV) variation when income elasticities are small, and this example is consistent with his findings. For an income elasticity estimated in the range of .3 to .5, we find at the very most a 10% difference in CV, EV and OS estimates and for some models less than a 1% difference (Figure 1 provides graphs of the compensated and ordinary curves for the two functional forms). Yet for other considerations: functional form, error specification, and the treatment of time, discrepancies range from 40 to 100%.

It is hard to have confidence in estimates which vary by as much as 100%. The intent of the work that we are pursuing at the University of Maryland is to reduce the apparent arbitrariness in these estimates, lending credence, if possible to this approach. The spirit of this work is to reduce some of the apparent arbitrariness in estimates by tests on observable behavior in the context of models of economic behavior.

The issue of the treatment of time is a good example, because the varying specifications of time in the demand function have so often been arbitrary. In our examples, vast differences in benefit measures occur when we compare the two treatments of time cost. Both approaches, introducing time directly in the demand function and valuing time as some function of the wage rate, have been applied in the literature.

We argue (see Bockstael, Strand and Hanemann, 1984) that the correct way to incorporate time costs into recreational demand models depends on the individual's opportunities and the constraints the individual faces in the labor market. The practice of arbitrarily valuing trip time at the wage rate for all individuals is shown to be misleading when a realistic set of labor market constraints is introduced. Individuals may not always be in the position of making marginal decisions with respect to work time, and thus the time cost of leisure activities is often not reflected by the wage rate.

This is not to say that time has a zero opportunity cost for such individuals. Other activities vie for the scarce resource. What it does mean is that time and money constraints cannot be collapsed into one constraint. The individual cannot, at the margin, trade time for money and money for time at a fixed rate.

Graphs such as those in Figure 2 illustrate the point. In panel A, the budget constraint, i.e. the trade-off between money and recreational time, is a straight line reflecting a fixed wage rate, w. In this graph, E is non-wage income and T is total available hours. Participants in the labor force are found at points in the open interval (BC) on the budget line, equating their marginal rates of substitution between leisure and goods to the wage rate.

Alternatively the individual may be "ra-

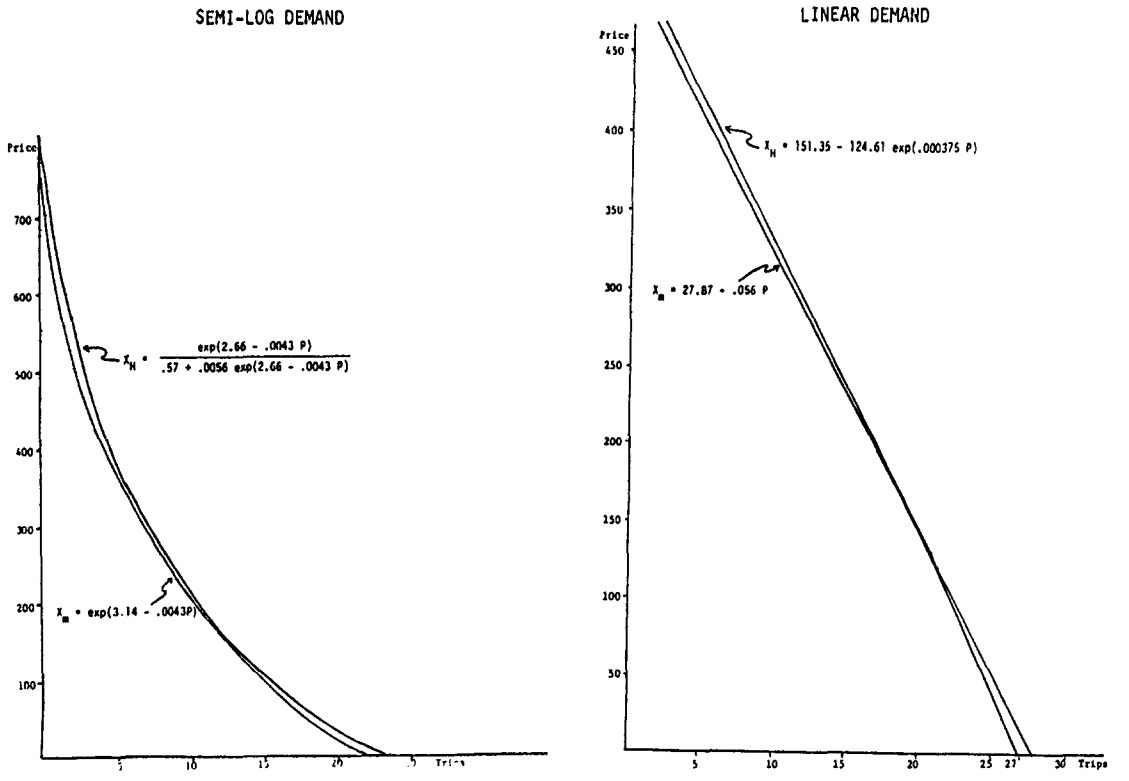


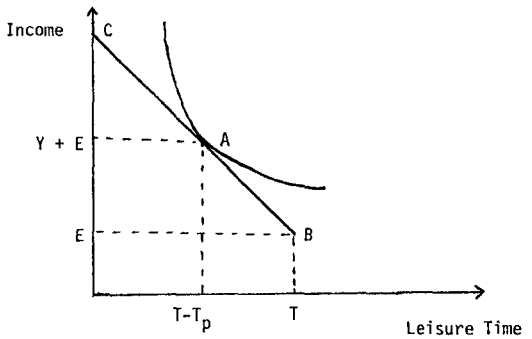
Figure 1. Semi-log and Linear Demand Functions

tioned" with respect to labor supply in a "take-it-or-leave-it" fashion. He may be forced to choose between a given (minimum) number of work hours (say 40 hrs/week) or none at all. Panel B depicts an individual whose primary job requires T_p hours within a total time constraint of T hours per week. The relevant wage rate at this primary job is w_p and is depicted as the slope of the implied line segment between A and B. This individual may be able to earn more wage income by moonlighting at a job with a lower wage rate (depicted by the slope of the segment between A and C). His relevant budget line is segment AC and point B. Depending on his preference for goods and leisure, he may choose not to work and be at B; he may work a fixed work week at A; or he may take a second job and be along the segment AC.

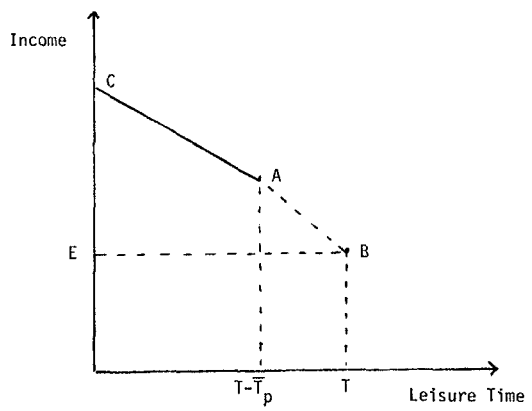
Consideration of more realistic employment constraints such as these have implications for model specification. Only those individuals who choose to work jobs with flexible work hours (such as second jobs or part-time jobs) can adjust their marginal rates of substitution of goods for leisure to the wage rate. All others can be found at corner solutions where no such equimarginal conditions hold.

For individuals at interior solutions, time and income constraints can be collapsed and straightforward welfare analysis pursued. For those at corner solutions, two constraints are relevant and the decision problem becomes more complex. We show in our paper how to estimate recreational demand functions and derive welfare estimates for samples composed of people in different types of labor market situations. Data requirements for this approach are not especially great. In addition to trips, travel cost, income, etc., one need only determine for each individual whether he could have worked at his existing (marginal) wage rate had he not been recreating.

In our paper we show, using simulation analysis, the biases which are introduced by naively assuming everyone in a sample faces and makes the same type of labor market decisions when in fact they do not. These biases can be substantial as indicated by the results in Table 1.



PANEL A.



PANEL B.

Figure 2. Income-leisure constraints

A second source of discrepancy in the estimates produced in Table 1 is associated with the interpretation of the error term in the recreational demand model. While different approaches can be uncovered in the literature, to my knowledge no one has previously explored in a systematic way the implications of the source of error (see Bockstael and Strand; Strand and Bockstael).

In Figure 3 is depicted a linear demand function estimated from the sample of Chesapeake Bay sportfishermen. After having estimated this function, how does one calculate each individual's ordinary surplus or the aggregate surplus for the sample? Suppose individual 1 is associated with the price quantity point (X_1, P_1). Is the appropriate surplus measure ΔABP_1 or $\Delta A'B'P_1$? Of course the difference between (X_1, P_1) and the regression line is due to the individual's "disturbance" term. We normally ignore these disturbance terms, as long as they

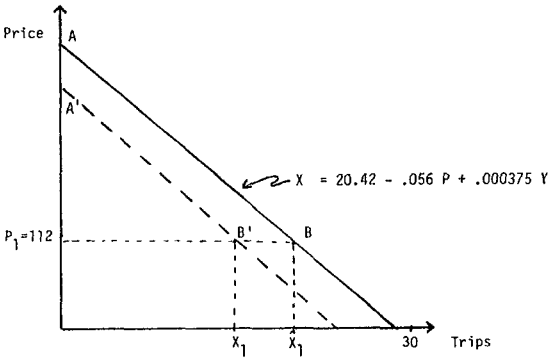


Figure 3. Effect of error source on consumer surplus calculations

are consistent with Gauss-Markov assumptions. The estimated coefficients and the predictions of the dependent variable will be unbiased irrespective of the cause of the disturbance, as long as it is normally distributed with zero mean, constant variance, and it is uncorrelated over observations or with other independent variables.

Two sources of "error" which can give rise to the disturbance term and still maintain consistency with Gauss-Markov assumptions are a) omitted variables (i.e. other uncorrelated explanatory variables missing from the equation) and b) errors in the measurement of the dependent variable. In any actual application both are likely to come into play, particularly in recreational demand analysis where recall error may be substantial.

In a recent paper (Bockstael and Strand, 1984), we show that while the source of error makes no particular difference in estimation and prediction, it has a potentially dramatic effect on benefit estimates. Theoretical derivation shows that different expected values of estimated consumer surplus from the same sample will result from the two assumptions about the source of error. The omitted variables assumption, the one commonly used in travel cost analysis, leads to larger values of consumer surplus than the measurement error assumption. The difference between them is directly related to the variance of the error, the sample size, the demand inelasticity, and the variance of the estimated price responsiveness. The results reported in Table 1 bear this out.

What implications do these results have for the researcher active in measuring benefits? Perhaps the most obvious result is that if the research attributes all of the error to omitted

variables (i.e. draws his demand curve through the observed (X_1, P_1)) when at least some of the error is due to measurement error, he may be vastly overestimating consumer surplus. The second implication is that improved estimates of consumer surplus can result if one can a) reduce the variance of the error in the regression and b) provide information as to the source of the error. Survey designs which reduce measurement error by limiting recall information will be helpful on both counts. The extent to which we can rule out measurement error, or alternatively test for it, will aid us in choosing between the vastly different benefit measures in Table 1.

While I have not discussed how we might resolve the discrepancies associated with different functional forms, work is in progress on this and several other issues. I hope, nonetheless, I have given you a sense of the spirit of our research. First and foremost, we would like to improve methods for valuing these very important goods about which society must make difficult decisions. Our intent is to improve their credibility. We are, however, open to the prospect that we may fail to resolve many of the anomalies associated with valuing public goods. Even if our methods are in error, if our attempts are carefully pursued in the context of testable theories, this area of public policy can only gain from our experiences.

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