# Measuring the Economic Value of Outdoor Recreation and Other Environmental Amenities: Discussion

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At the outset, let me say I regret very much not being able to attend these meetings. Furthermore, when I found out that I would be unable to attend, I was reluctant to attempt to discuss the very interesting and timely papers presented by Hueth and Strong and by McConnell and Bockstael. However, my comments are being forwarded to you for whatever use you may wish to make of them.

Starting with the paper by Professors McConnell and Bockstael, I very much agree with their first statement that "Problems of aggregation plague applications of microeconomics." It is interesting that similar problems were encountered in agricultural economics in trying to estimate regional and national supply for farm products by aggregating the individual supply functions of various kinds of "representative" farm firms. As I recall, the last of these efforts was conducted during the 1960s or early 1970s by means of large regional linear programming models of representative farms. It is my impression that not all the problems involved with this approach were satisfactorily resolved. At least I am not aware of much being published along these lines during the past few years.

I think that I am in general agreement with many of the conclusions reached by McConnell and Bockstael. They note in an excellent statement that recreation economics is a product of two legacies, one of which is from Hotelling and Clawson and is derived from the analogy of markets and the use of average behavior to gain plausible measures of quantities demanded at various distances. Then, from the inferred quantity-price relationships, somewhat plausible estimates of value have been inferred. However, these estimates of value based upon Marshallian consumer surplus may, at least in some cases, differ dramatically from willingness to pay compensation and from willingness to accept compensation, according to recent research by Hueth. On the other hand, McConnell and Bockstael correctly note the important legacy ". . . derived from axioms of optimizing behavior and attempts to develop exact welfare measures based on individual behavior. The aggregation issue is one of several cases where the conflict between the two legacies must be worked out."

Most of us would also agree with McConnell and Bockstael that individual behavioral parameters would be the appropriate parameters to use for welfare measurement. Furthermore, I would not deny that it is somewhat difficult to reconcile the traditional travel cost method with models of individual choice, and especially so if one is working only with strictly averaged or aggregated data and cannot go back to the individual observations that generated the aggregated data, as McConnell and Bockstael appear to assume. Actually, it should be noted that the $X_{ij}$ values in (1) for the individual observations may also be available for some data sources, such as where one conducts a survey with a sample drawn from a list of all eligible participants, like license holders.

My main difficulty in following part of the analysis by McConnell and Bockstael begins where they assume that the participation rate, $\pi = n_i/P_i$, is constant for all i. It seems to me that they then argue that if the participation rate is constant across all zones, then there is no problem in accurately estimating the parameters of individual behavior and, presumably, the consumers' surplus. However, I found it difficult to understand the basis for their statement on that "This expression (for the consumers' surplus) is accurate only so long as $\pi$ is constant." It appears that this statement may be true if we accept their earlier statement that we would usually not know

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the participation rate. However, I find it quite puzzling as to why we would not know the participation rate. (Perhaps one might not know the participation rate for certain kinds of secondary data sources, but where we have conducted our own surveys of a sample drawn from the eligible participants, the participation rate can be legitimately estimated.) In fact, it would seem that one of the biggest advantages of the traditional zonal travel cost model is that it implicitly takes account of the participation rate in order to specify the dependent variable as trips (or other measures of activity) per capita.

To clarify and illustrate the preceding remarks, consider the following simple case. Suppose that the "true" individual demand function for a given recreational activity is

$$(1) \qquad q_i = 6 - 1.0\, TC_i + \alpha_i$$

where $\alpha_i$ denotes a random "intensity" variable which represents the difference in intensity of demand among the various recreationists. If $E(\alpha_i) = 0$, then the mean individual demand function would be $E(q_i) = 6 - 1.0\, TC_i$. It is important to note that $\alpha_i$ is not an error term, but rather is a variable that denotes the difference in intensity or strength of demand among various individuals. For illustrative purposes it is convenient to assume that $\alpha_i$ is a discrete variable that takes certain values that are distributed symmetrically about zero with the following probabilities:

$$(2) \quad E(\alpha_i) = \frac{1}{16}[-4 + 4(-2) + 6(0) + 4(2) + 4] = 0.$$

The variance of $\alpha_i$ would then be

$$(3) \quad E(\alpha_i - 0)^2 = \frac{1}{16}[(-4)^2 + 4(-2)^2 + 6(0) + 4(2^2) + (4)^2] = 4.$$

The distribution of $\alpha_i$ implied by (2) is the same as the distribution of the sum that could be obtained from flipping four unbiased coins where a tail would be assigned a value of $-1.0$ and a head a value of $+1.0$. Thus, the probability would be 1/16 of obtaining four tails equal to a sum of $-4$, and similarly for four heads giving a sum of $+4$. The probability of obtaining three tails and one head equal to a sum of $-2$ would be 4/16, with the same probability for three heads and one tail giving a sum of $+2$. Finally, the probability of obtaining exactly two heads and two tails with zero sum is 6/16. All the above probabilities follow from the binomial expansion, as shown in some probability or statistics textbooks.

At this point it should be noted that preceding Equation (1) could conceivably be generated by McConnell and Bockstael's Equation (1), where $\alpha_i$ represents the influence of their $X_i$ variables, except for travel cost, which is explicitly included in my Equation (1). Of course, in my Equation (1), I have not included their random error term, $\epsilon_{ij}$, but I believe that such an error term could be added to my equation without greatly changing the results, assuming that $\epsilon_{ij}$ is not excessively large relative to the effect of all the $X_i$ variables in their Equation (1).

At any rate, with the assumed individual demand function of (1), then consider the simplest possible travel cost-distance zone data shown in Table 1, generated from Equations (1) and (2). Note that for distance zone #1, the expected number of trips per recreationist is $6 - 1 = 5$, but some recreationists take more and some take less, depending upon their "intensity of demand," i.e., depending upon their $\alpha_i$ value. For example, the first line of numbers in Table 1 corresponds to $\alpha_i = -4$. Therefore, the number of visits per participant would be $6 - 1 - 4 = 1$. Since there would be only one respondent, on the average, for this $\alpha_i = -4$, multiplying 1 times 1 times the sample blow-up factor of 100 gives the estimated total number of visits of 100 for the first line. For the second line of numbers in Table 1, corresponding to $\alpha_i = -2$, the total visits per participant would be $6 - 1 - 2 = 3$. Since, from Equation (2), there would be four sample observations for $\alpha_i = -2$ on the average, the estimated total number of visits in the next to last column would be 3 times 4 times the expansion factor of 100 equals 1,200. The other numbers for the main distance zone #1 were generated in the same way.

For zone #1, no potential participants were eliminated since the lowest intensity of demand and travel cost do not drive the $q_i$ value to be equal to or less than zero. But for main distance zone #2 where the travel cost increases to 4, the respondent in the first line of zone #2 of Table 1 with $\alpha_i = -4$ would have predicted trips of $q_i = 6 - 4 - 4 = -2$. Since trips must be greater than or equal to zero, zero trips would be indicated by such a respondent. For the second line of zone #2 with $\alpha_i = -2$, exactly zero trips would again be

**Table 1. Observations generated for three distance zones where the true individual demand functions are assumed to be $q_i = 6 - 1.0\ TC_i + \alpha_i$, $E(\alpha_i) = \frac{1}{16}[1(-4) + 4(-2) + 6(0) + 4(2) + 1(4)]$**

| Main Distance Zone | Main Zone Population | Intensity of Demand | Average Travel Cost per Visit | Predicted Total Visits per Participant | Number of Respondents | Estimated Total Number of Visits[a] | Zone Average Visits per Capita |
|---|---|---|---|---|---|---|---|
| | | −4 | $1 | 1 | 1 | 100 | |
| | | −2 | 1 | 3 | 4 | 1,200 | |
| 1 | 1,600 | 0 | 1 | 5 | 6 | 3,000 | 5.0 |
| | | 2 | 1 | 7 | 4 | 2,800 | |
| | | 4 | 1 | 9 | 1 | 900 | |
| | | −4 | 4 | 0 | 1 | 0 | |
| | | −2 | 4 | 0 | 4 | 0 | |
| 2 | 1,600 | 0 | 4 | 2 | 6 | 1,200 | 2.125 |
| | | 2 | 4 | 4 | 4 | 1,600 | |
| | | 4 | 4 | 6 | 1 | 600 | |
| | | −4 | 7 | 0 | 1 | 0 | |
| | | −2 | 7 | 0 | 4 | 0 | |
| 3 | 1,600 | 0 | 7 | 0 | 6 | 0 | 0.4375 |
| | | 2 | 7 | 1 | 4 | 400 | |
| | | 4 | 7 | 3 | 1 | 300 | |

[a] Assumes a random sampling of one percent from the general population and corresponding expansion factor of 100.

reported since $q_i = 6 - 4 - 2 = 0$. Thus, the total trips for these four respondents would be zero. For distance zone #3 of Table 1 with travel costs of $7 per visit, only the five respondents represented by the last two lines of numbers in Table 1 would have sufficiently high intensities of demand with $\alpha_i = 2$ and $\alpha_i = 4$ to take one or more trips. Thus, the sample number of participants drops from 16 to 11 to 5 in going from the nearest zone to the more distant zones, where all zones are assumed to have equal populations of 1,600.

How would the results in Table 1 change if the $\alpha_i$ values were distributed differently; e.g., if the $\alpha_i$ were distributed normally with mean zero and variance equal to four? Actually, a similar kind of result should be obtained for most symmetric distributions with similar means and variances.

How accurate would various estimates of consumer surplus be? The accuracy is easily checked by first computing the individual consumer surpluses from the assumed true demand function, $q_i = 6 - 1.0\ TC_i + \alpha_i$. For the first line of numbers in Table 1, the true individual demand function is $q_i = 2 - TC_i$, and it represents one sample observation. The corresponding consumer surplus is equal to $1/2(1)(1) = 0.5$. Blowing it up by the expansion factor of 100, a true consumer surplus of $50 is obtained. Similarly, for the second line of numbers in Table 1, the true demand function is $q_i = 4 - TC_i$, implying three trips by this type of participant. A true consumer surplus equal to $1/2(3)(3) = \$4.5$ per participant is computed. However, since there are four observed recreationists of this type in line #2 of Table 1, the total consumer surplus represented by line #2 would be $400(\$4.5) = \$1{,}800$. Following this same procedure for the rest of Table 1, a total true consumers' surplus of $30,050 is obtained.

It is interesting to estimate the error in estimating consumer surplus from the traditional zone average travel cost model. Using the last column in Table 1 (the zone average visits per capita) as the dependent variable, the zone average travel cost estimate of the demand function was

$$y_i = 5.5625 - 0.760417\ TC_i. \tag{4}$$

Using (4), the traditional estimate of consumer surplus for zone #1 was $15.1627 times $1{,}600 \doteq \$24{,}260$. For zone #2, estimated consumer surplus was $4.1784 times $1{,}600 \doteq \$6{,}685$, and only about $60 for zone #3. Thus, a total consumer surplus of about $31,005 would be estimated by the zone average travel cost model, fairly close to the true consumer surplus of $30,050. Thus, the error of estimation was only about three percent, much better than I would have expected from McConnell and Bockstael's statement, ". . . To use OLS in the traditional way on aggregates of zones, we must be assured that the participation rate is constant across zones."

Of course, the preceding exercise from Equation (1) and Table 1 does not prove that the zonal average travel cost model will always give accurate estimates, but it does cast some reasonable doubt on the conclusion that the participation must be constant across zones for the travel cost method to give reliable results. The main point of my exercise with Equation (1) and Table 1 is that the consumer surplus estimates based upon the traditional zone average travel cost model were surprisingly accurate under fairly realistic conditions. Of course, a more extensive Monte Carlo type simulation from individual observations generated with more of the specific features of McConnell and Bockstael's Equation (1) could and should be conducted to give a better evaluation of the relative accuracy of estimation by the travel cost method under various specified conditions as compared to other alternative estimators, such as those proposed by McConnell and Bockstael.

Although it is only one case, in defense of my preceding exercise with Equation (1) and Table 1, it was never selected or designed to show good accuracy for the travel cost method, but rather was designed to illustrate why demand estimates based upon unadjusted individual observations should not be used when the participation rate declines with increasing distance, the most common case of nonconstant participation rates. (I believe that Professor Hueth would attest that he was all too familiar with the controversy that motivated me to develop the preceding exercise.)

In addition to the fine contribution by McConnell and Bockstael, I was also most favorably impressed by the excellent paper by Hueth and Strong. One very nice aspect of their paper was that they were not more critical of some of the earlier work by Sorhus and myself in trying to use a two-stage procedure to relate consumer surplus per river to fish catch per river. One serious limitation of our crude procedure was that it tended to impute all of the consumer surplus to the fish caught, in contrast to a more sophisticated model like the household production function. Although John Loomis and I have recently experimented with including fish catch in a regional travel cost model, it is too early to predict how well this approach will work in estimating the marginal value of fish. Suffice to say that we have discovered some specification pitfalls that should be avoided!

In conclusion, it is difficult to find justification for disagreeing with Hueth and Strong's evaluation of the TC, HTC, and HP models. Being somewhat more empirically oriented, I might be inclined at this point in time to place more emphasis on the relative data needs of the three models. Since it is cruder, the TC method appears to require less sophisticated data. However, as more experience with the HTC and HP approaches is acquired, this one present advantage of the TC model may greatly diminish in the future.

## References

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