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Contingent Valuation and Actual Behavior: Predicting Connections to New Water Systems in the State of Kerala, India

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In 1988, families in Kerala State in India were surveyed to ascertain their willingness to pay for household connections to a piped water supply system. In 1991 the families in these communities were surveyed again and their actual decisions recorded. This article explores the validity of the findings of the 1988 study on the basis of actual behavior. It looks at the question of benefit revelation: did people behave as they said they would? And it looks at the question of benefit transfer: did people in one site behave as they were predicted to behave, on the basis of the predictions of a behavioral model for a different site? The data were also used to analyze the policy relevance of behavioral modeling.

The ability to put a value on environmental resources is a core problem in environmentally sustainable development in industrial countries (Carson and Mitchell 1993 and Mitchell and Carson 1989) and developing countries alike (Serageldin and Steer 1994). During the past twenty years there has been a vigorous and contentious debate about the relative merits of various approaches (Brookshire and others 1982; Arrow and others 1993). The "indirect approach" draws conclusions from actual behavior; the "direct approach," or the contingent valuation method, draws conclusions from responses to hypothetical questions. The "benefit-transfer" issue in environmental economics, which is concerned with transferring valuations from one population to estimate how a second population would value the same resource, further complicates the debate (Pearce 1993).

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These approaches have been applied to other areas, including water supply policy in developing countries (World Bank 1992). The fundamental issue in water supply policy is predicting the response of consumers to a service to which they have not previously had access or to characteristics that they have not previously experienced. Piped water supply, higher prices, and improved reliability are examples (see Briscoe and others 1990; World Bank Water Demand Research Team 1993).

Mirroring the debate in the environmental economics literature, two fundamental approaches are used to analyze this fundamental issue. The indirect approach involves observing actual behavior, modeling this behavior, and then deriving the willingness to pay for water connections from the value of time spent fetching water or from housing values (see North and Griffin 1993, for example). The direct (contingent valuation) approach, by contrast, involves taking a survey (through a carefully designed and administered questionnaire) of households' willingness to pay specified prices for hypothetical services.

Our first concern is with the validity of the direct approach in predicting actual behavior. In environmental economics, because contingent valuation methods are used to value public goods or environmental amenities, it is not possible to validate the hypothetical responses of the interviewed population through actual market behavior. For a water system, however, it is possible to test the results of a contingent valuation survey by comparing the responses given when the water system was hypothetical to the actual behavior once the water system becomes available.

Our second concern is to test the benefit-transfer hypothesis as it applies to direct valuation approaches. Contingent valuation methods are subject to hypothetical bias, strategic bias, and compliance bias. Hypothetical bias can be reduced if the sample is well aware of the nature of the good; strategic bias can be reduced if the sample has little or nothing to gain by undervaluing the good. Compliance bias can be reduced through careful development of the survey, training, and supervision of the fieldwork. These problems can be reduced, at least hypothetically, by using the benefit-transfer approach, in which the behavior of a group that already has the service is projected onto a second group to predict the second group's willingness to pay for the good or service in question. This approach may be of interest for three reasons: the second group has little knowledge of the good, the second group may behave strategically, and the technique requires little additional data collection. The contingent valuation survey in Kerala was carried out in pairs of communities that differed primarily in their having or not having a water system available. We can therefore test the accuracy of benefit-transfer predictions from the villages where water systems already existed in relation to what we will call "benefit-revelation" predictions based on the contingent valuation responses of the villages where no water system existed.

Our third concern is to test the relative accuracy of simple tabulations of willingness to pay from a contingent valuation survey and predictions from a behavioral model using the same data. Simple tabulations may provide all the information we need to decide whether people will hook up to a new water system under a planned tariff structure. Behavioral models are also important, however, because they allow us to simulate the impacts of changes in policy variables, estimate price elasticities, quantify changes in welfare, and control for nonpolicy variables that also affect behavior. Yet such models may be misspecified, the functional form may be inappropriate, or the assumed distribution of the errors may be wrong. We are able in this analysis to test actual behavior against both simple tabulated responses and predicted responses based on a behavioral model.

In this article we analyze two surveys of willingness to pay for improved water services in Kerala State in India. The first survey was done in 1988; a follow-up survey of the same households was conducted in 1991. Section I recaps the findings of the 1988 study, and section II describes the 1991 follow-up survey. The article then explores the validity of the findings of the 1988 study on the basis of actual behavior. Section III looks at results for the scarce-water environment and section IV for the saline-water environment. Section V presents conclusions.

I. RECAP OF THE 1988 STUDY

The 1988 study of the willingness to pay for water in villages in northern areas of the Indian state of Kerala was part of a multicountry study of willingness to pay for water in rural areas of developing countries (World Bank Water Demand Research Team 1993). Singh and others (1993) reported the results of the 1988 contingent valuation study. The original study conducted contingent valuation surveys among families living in three types of traditional drinking water environments—abundant water, scarce water, and salt-water intrusion (the latter is referred to as saline water in text). Pairs of communities were selected within each of the three water environments. Each pair included a site-A community and a site-B community.

Households in the site-A communities had piped water service already available. Within the site-A communities, two types of households were surveyed: those who had already decided to connect at the existing connection costs and tariffs and those who had decided not to connect. Connectors were asked whether they would continue to connect, for a range of hypothetical tariffs higher than the current tariff. Nonconnectors were asked whether they would connect, for a range of hypothetical tariffs and connection costs.

Households in the site-B communities had no piped system but could expect to have one installed soon. Families were asked whether they would connect for a variety of hypothetical connection costs and monthly charges. All households in both sites in each type of water environment were asked about their willingness to pay if the reliability of the water system were improved.[1] Thus, the im-

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[1] Reliability was characterized as follows: "Now I would like you to tell me what you would do if the service through the piped water system was greatly improved. Imagine that the water supply was available every day for most of the day, that the flow in the taps was always good, and that the water was clean."

pact on the choice of connecting of three characteristics of water systems—connection charge, tariff, and reliability—was tested through the contingent valuation method.

In the original survey there were 1,150 households distributed approximately evenly across the three types of water environments, including the entire population of connectors in the three A sites, a sample of nonconnectors in the A sites, and a sample of potential connectors in the B sites. Table 1 provides some basic information about the survey sites and shows how willingness to pay varied across connectors and nonconnectors in the A sites and overall for the B sites.

The exact questions posed during the interviews and the econometric issues that had to be resolved are discussed in detail in Singh and others (1993: 1,932–35) and will not be reproduced here. Table 2 summarizes how well the 1988 survey followed best practice in designing contingent valuation surveys, or what has become known as the "Seven Pillars of NOAA" (National Oceanic and Atmospheric Administration) (Portney 1994). These rules are designed to overcome some of the known problems with the technique. Generally, the survey meets or exceeds the desired specifications, but the true test is whether the hypothetical responses adequately predict subsequent behavior.

Table 1. *Location and Types of Survey Sites, with Sample Size and Maximum Willingness to Pay, Kerala, India*

	A sites: improved water source available		*B sites: no improved water source available*
Site characteristic	*Connectors*	*Nonconnectors*	
Water abundant			
Location	Ezhuvathuruthy	Ezhuvathuruthy	Nannamukku
All households	66	819	1,497
Household sample	66	100	200
Water scarce			
Location	Elapully	Elapully	Elapully
All households	86	723	876
Household sample	86	100	200
Water abundant but with saline intrusion			
Location	Ezhuvathuruthy	Ezhuvathuruthy	Vallikkunnu
All households	98	768	1,313
Household sample	98	100	200
Total household sample	250	300	600
Average maximum monthly tariff bid (rupees)	19.3	8.7	5.5
Average maximum connection charge bid (rupees)	n.a.	355	267
Average maximum bid for improved service (rupees)	25.0	9.7	n.a.

n.a. Not applicable because the bidding game was not conducted in that site.
Note: The exchange rate in 1988 was about 14 rupees per U.S. dollar.
Source: Authors' calculations.

Table 2. *Comparison of National Oceanic and Atmospheric Administration (NOAA) Guidelines for Contingent Valuation Surveys with Guidelines Used in the Kerala 1988 Survey*

NOAA guidelines	*Procedures used in the Kerala survey*
1. Interview in person rather than on the telephone.	Interviewed household head personally.
2. Question about a future, hypothetical occurrence rather than a historical event.	Survey asked about willingness to pay for a new or improved water system or a change in tariff policy, not for an existing service.
3. Referendum format in which respondent "votes" on a benefit with a known price (as opposed to open-ended questions).	Bidding game format used: interviewer suggested prices for monthly tariff or connection cost; respondent answered yes or no to each quote.
4. Interviewer begins with a scenario accurately describing the benefit or program.	Interviewer described the exact nature of the good or service to be provided: existing quality of piped water services into the home at various monthly tariffs and connection charges, plus improved quality of service at various monthly tariffs.
5. Survey reminds that payment for the new benefit reduces other consumption.	Survey had no reminders, but questions about other consumption and assets preceded the contingent valuation questions.
6. Survey reminds that substitutes exist for the hypothetical benefit in question.	In-depth questioning about cost, distance, and other characteristics of the household's sources of water (and volume of consumption) preceded the contingent valuation questions. No specific reminder was given during the contingent valuation questions, but it was clear to the respondent that the context of the survey was general use of various sources of water.
7. Follow-up questions to make sure respondent understands the choices made.	No follow-up questions, but interviewer evaluated the quality of response. There was a follow-up survey to ascertain actual behavior after the water system was put in place, and respondents were asked to explain divergence between hypothetical (1988) and actual behavior (1991).

Source: Portney (1994) and authors' review of the Kerala survey and procedures.

In the earlier analysis (Singh and others 1993), we estimated an econometric model that controlled for individual, household, and community characteristics that could affect the responses to the three policy variables in which we were interested: monthly tariff, connection cost, and reliability. The purpose of estimating the full model was to isolate the impact of the policy variables on the choice to connect and then to perform simulations of how changes in these policy variables would affect the number of water connections demanded and, consequently, consumer welfare.

Table 3 contains the full model estimated in Singh and others (1993), along with coefficient estimates and *t*-statistics. Monthly tariff was an important determinant of whether respondents were willing to pay for yard connections. For each 1 percent increase in the monthly tariff, the probability of choosing a yard tap fell by almost 1.5 percent. However, connection cost was an even greater impediment limiting connections, probably because of the very high implicit interest rates prevailing. Whereas a ten-rupee increase in the mean monthly tariff quoted in the bidding game would cause connection probabilities to fall by approximately 27 percent, a ten-rupee increase in the implicit monthly connection cost would cause connection probabilities to fall by approximately 82 percent.[2]

Another finding is that household characteristics matter. The decision to connect was positively and strongly affected by nonpolicy variables: higher levels of income, assets, and schooling. Water scarcity also matters. People in scarce-water areas were much more likely to connect, everything else being equal, than those in areas where water is abundant. Improved reliability turns out to be important only for current connectors. Those who were already connected indicated that they would be willing to pay substantially more for more-reliable service. More-reliable service did not, however, affect the probability that nonconnectors would decide to connect to the system.

The findings indicate that large potential welfare gains would be generated by a more liberal connection and pricing policy. Because of the problem of connection costs, we estimated that consumer surplus could be increased by at least 450 percent by amortizing the connection cost and folding it into the monthly tariff. We estimated that connections in the population would rise from the current 250 to about 2,500 under this policy regime.

II. THE 1991 FOLLOW-UP SURVEY

In the period following the 1988 contingent valuation study, improved water services were made available in the scarce- and saline-water areas. Site-B households in these two areas had to decide whether to connect to the improved system. The original site-B families in these two water environments were resurveyed in 1991 to determine whether they had connected.

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[2] Under reasonable assumptions about the interest rate and amortization schedule, the impact of the tariff and connection charges on the probability of connecting can be equalized, suggesting that respondents treated the tariff as a recurrent cost and the connection charge as a capital cost. The monthly charge for the connection cost in this example is calculated as if the connection cost were financed for six years at an annual real interest rate of 5 percent. A ten-rupee increase in the average monthly cost would thus correspond to an increase in the total connection charge from the bidding game average of Rs219 to Rs842. In fact, rural Indians are likely to face much stiffer credit market conditions, so this example is for most people probably a substantial understatement of the full connection costs when high interest rates are taken into account.

Table 3. *Probit Estimates of the Probability of Choosing a Yard Tap (1988 Survey)*

Variable	*Coefficient*
Constant	−0.3009 (−1.28)
Tariff	−0.0605 (−20.31)
Connection charge	−0.0010 (−13.21)
Improved service	−0.0582 (−1.11)
Distance to current source (meters)	0.00002 (0.06)
Queue at current source (minutes)	0.0028 (1.54)
Per capita income (rupees)	0.00002 (1.93)
Household has electricity	0.3345 (3.83)
Number of rooms in dwelling	0.0861 (3.50)
Household has females in government service	−0.0997 (−0.57)
Household has males in government service	0.1664 (1.78)
Hindu household	−0.1908 (−2.07)
Female-headed household	0.0569 (0.66)
Female respondent	−0.2749 (−3.70)
Some primary school	0.5092 (3.39)
Primary school complete	0.6293 (4.45)
Middle school complete	0.9608 (6.51)
Secondary school complete	1.1325 (8.09)
More than secondary school	1.2898 (7.99)
Scarce-water area	0.3474 (3.54)
Saline-water area	−0.2315 (−2.19)
A-site nonconnecting household	−0.3070 (−2.45)
B-site village household	−0.4921 (−3.87)
Pseudo R^2	0.28
Sample Size[a]	9,720
Households	1,150
χ^2 (22)	3,272

Note: Dependent variable: whether respondent would choose to connect at each price quoted. Estimates are weighted by the population of the sampling unit. Standard errors are corrected using a method explained in Singh and others (1993). The estimating equation is significant at better than the 0.00001 level for a likelihood ratio test (χ^2). The omitted site dummy is A-site connecting household. *t*-statistics are in parentheses.

a. The sample size of 9,720 results from multiple observations for each household in the sample. See Singh and others (1993) for a complete explanation.

Source: Authors' calculations.

Data Issues

In the 1991 follow-up survey, an attempt was made to contact all of the respondents in the original sample in the two villages. The first issue to address in any follow-up survey is sample attrition. We lost 25 of 200 households (12.5 percent) from the sample in the scarce-water site and 59 of 200 households in the saline-water site (29.5 percent) between 1988 and 1991. The loss of households appears to have been random in the scarce-water site in that it left the original income distribution virtually intact. In the saline-water site, attrition was concentrated among the poorer households.[3]

"Connectors" in this analysis are defined as those who either were already connected to the new system at the time of the follow-up survey, had applied for a connection, or had made a decision to connect once applications were called for by the water authority. Both connectors and nonconnectors were asked a short series of questions concerning their decision and concerning exogenous changes that had come about since the original survey.

We predicted that a household would connect if the maximum bid for connection in 1988 was higher than the actual connection cost in 1991. If the household was connected at the time of the interview in 1991, the respondent was asked how much the connection actually cost. If the household was not connected, the interviewer estimated the cost of connecting for that household on the basis of its distance from the distribution line.[4]

Judging the Validity of Predictions

What constitutes a success or failure in making a prediction based on the 1988 survey? In this section we lay out the criteria to be used in judging the success of the experiment.

The first and most important criterion, as in any sampling procedure, is whether we predict the correct proportion of connectors, notwithstanding whether we predict the exact behavior of each household. We would like to get as close as possible to the correct proportion. If we err, we prefer to err on the side of underpredicting the proportion of connectors because the concern with the direct approach is that its inherent biases tend toward overvaluation. Furthermore, we would not want to recommend installation of a water system on the basis of estimated demand and revenue forecasts that are too optimistic.

The second criterion, aimed directly at the heart of evaluating the technique, is whether specific households behave as they said they would. There are three elements here. The first is gross accuracy, or the proportion of the surviving sample for which actual behavior was correctly predicted. The second is the specificity of connector predictions or the proportion of connecting households predicted correctly. The third is the sensitivity of predictions for nonconnectors or the proportion of nonconnecting households predicted correctly.

The statistical epidemiological literature distinguishes between specificity and sensitivity (see Kleinbaum, Kupper, and Morgenstern 1982). Specificity has to do with the problem of false positives, that is, the number of families that were predicted to connect but that actually did not connect. Sensitivity has to do with the problem of false negatives, that is, the number of families that were predicted not to connect but that actually did connect.

Comparing Benefit Revelation, Benefit Transfer, and a Behavioral Model

We apply the above procedures to assess whether the behavior of families at the B sites could best be predicted by benefit-revelation or benefit-transfer methods. There are, accordingly, two research questions. First, did people at the B sites behave as they said they would? We call this the benefit-revelation question. Second, did people at the B sites behave as we had predicted on the basis of the predictions of a behavioral model for the A sites (after substituting the characteristics of households in the B sites)? We call this the benefit-transfer question. Prima facie, it is not obvious which of the two strategies implicit in the above questions would be most promising.

To partially assess the validity of the inferences drawn from the behavioral model (Table 3) estimated using the 1988 data (Singh and others 1993), we also compare predictions from it with actual behavior. Is the actual behavior of the families in the B sites in 1991 predicted accurately by a behavioral model based on the responses of those families in 1988? We call this the behavioral modeling question.

III. Results for the Scarce-Water Site

This section analyzes the results for the scarce-water site. The results are also robust for the saline-water site, but we do not place equivalent weight on these findings, for reasons to be explained in the next section.

Benefit Revelation

The fundamental question that the follow-up survey was designed to address is the simplest one: did people behave as they said they would? It was necessary to exclude those families for whom predictions could not be made because they bid at the maximum (Rs700) in 1988, and their actual connection costs in 1991 (which varied from house to house, depending on the distance to the public water line) exceeded this maximum bid.[5] This situation applied to 13 of the 161

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[3] The exact comparison of the two samples is available from the authors.

[4] The connection cost in 1991 was deflated to 1988 rupees so that the numbers could be compared in real terms. We used the average inflation rate from 1980–90 as reported in World Bank (1992), 7.9 percent. This long-term average rate was used to avoid year-to-year fluctuations for this investment and in recognition of the imprecision of annual estimates.

[5] For this group, we do not really observe a maximum bid because we know only that it was at least Rs700. Thus we cannot make a comparison with the connection cost faced in 1991.

Table 4. *Comparison of Actual and Predicted Behavior of B-Site Households in Water-Scarce Areas*
(number of households)

	Actual behavior		
Predicted behavior	*Did connect*	*Did not connect*	*Total*
Benefit revelation[a]			
Will connect	15	6	21
Will not connect	7	120	127
Total	22	126	148
Benefit transfer, all A sites[b]			
Will connect	27	76	103
Will not connect	1	65	66
Total	28	141	169
Benefit transfer, scarce-water A site[c]			
Will connect	28	100	128
Will not connect	0	41	41
Total	28	141	169
Behavioral modeling[d]			
Will connect	10	13	23
Will not connect	18	128	146
Total	28	141	169

a. Prediction based on 1988 survey of B-site households in the scarce-water area.
b. Prediction based on 1988 survey of A-site households in all three water areas.
c. Prediction based on 1988 survey of A-site households in the scarce-water area.
d. Prediction based on probit model of 1988 B-site households in the scarce-water area.
Source: Authors' calculations.

families (7 of which actually connected).[6] The results for the remaining 148 respondents are presented in table 4 (under "benefit revelation").

Table 5. *Summary Statistics on the Accuracy of Predictions of Behavior of B-Site Households in Water-Scarce Areas*
(percentage)

Sample statistic	*Value*
Benefit revelation[a]	
Gross accuracy	91
Specificity of connector prediction	71
Sensitivity of prediction for nonconnectors	94
Benefit transfer, all A sites[b]	
Gross accuracy	54
Specificity of connector prediction	26
Sensitivity of prediction for nonconnectors	98
Benefit transfer, scarce-water A site[c]	
Gross accuracy	41
Specificity of connector prediction	22
Sensitivity of prediction for nonconnectors	100
Behavioral modeling[d]	
Gross accuracy	82
Specificity of connector prediction	43
Sensitivity of prediction for nonconnectors	88

Note: Gross accuracy is the percentage of the surviving sample for which actual behavior was correctly predicted. Specificity of connector prediction is the percentage of those predicted to connect and who actually did. The sensitivity of prediction for nonconnectors is the percentage of those predicted not to connect who actually did not.
a. Prediction based on 1988 survey of B-site households in the scarce-water area.
b. Prediction based on 1988 survey of A-site households in all three water areas.
c. Prediction based on 1988 survey of A-site households in the scarce-water area.
d. Prediction based on probit model of 1988 B-site households in the scarce-water area.
Source: Authors' calculations.

The results indicate that 14.9 percent of the respondents (22/148) did connect. This is not statistically different (at the 5 percent significance level) from our prediction that 14.2 percent (21/148) of the families would connect. The gross accuracy of the predictions based on benefit revelation was 91 percent, as shown in table 5. That is, the behavior of 91 percent [(15 + 120)/148] of the families was consistent with the intentions they declared in the 1988 contingent valuation survey. The specificity of connector predictions, the percentage of those predicted to connect who actually did connect, was 71 percent (15/21). The sensitivity of nonconnector predictions, the percentage of those predicted not to connect who actually did not, was 94 percent (120/127). Thus, simple tabulations of respondents' answers to the contingent valuation survey in 1988 were remarkably accurate in predicting both the overall proportion of the sample connecting and the household-specific choices once the households were given the opportunity of connecting to the piped water supply system in 1991.

In addition to determining whether families connected or not, the follow-up survey asked why. A variety of responses were given during these open-ended discussions. Only one emerged with any consistency—three of the thirteen respondents whose behavior was inconsistent with their 1988 response cited "changed economic circumstances." Two of the seven "unpredicted connectors" fell into this group, as did one of the six "unpredicted nonconnectors." For the nonconnectors, more than 75 percent indicated that "inability to pay the connection cost" was the primary reason for not connecting to the system, just as we had predicted from the analysis of the 1988 data.

Did reliability affect connection decisions in the sample in 1991? We predicted, on the basis of the 1988 results, that households already connected were concerned with reliability, but it was not an important consideration for those who were not already connected to a water system. The results of the ex post

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[6] After losing 25 households because of attrition, we lost another 12 households that had missing values for connection cost in 1991 and 2 with missing values for the 1988 contingent valuation questions, which left us with 161 households for analysis in the 1991 sample.

investigations confirm this finding. Only a small proportion of nonconnectors (13 percent) replied that an inadequate quantity of water from the system was the main reason for not connecting, and no respondents mentioned service quality as a decisive reason for not connecting. However, those who had connected in 1991 unanimously expressed dissatisfaction with the reliability of the system. Every connector complained about the quantity of water available from the system during the dry season, and all but one of the connectors found the quantity inadequate even in the monsoon season.

Benefit Transfer

Many reasonable people are justifiably suspicious of answers to hypothetical questions by subjects who have a strong interest in the outcome of the research. Extrapolation based on information gathered from a similar group that is not subject to these problems may reduce these biases and thus be a more reasonable basis for predicting actual behavior. The strategy of benefit transfer depends on the validity of models that can allow us to extrapolate from behavior or valuation of benefits in one area to populations of known characteristics in other areas. This method is still in its infancy (Pearce 1994; Desvousges, Naughton, and Parsons 1992; and Boyle and Bergstrom 1992). In the United States there has been only one legal proceeding in which contingent valuation estimates from one study were used to estimate values in another site, and the court refused to accept the "transferred values" as legitimate evidence (Brookshire and Neill 1992).

Three guidelines have been developed for use in research on benefit transfer. First, the study site should be very similar to the policy site. Second, the policy change or project at the study site should be very similar to that proposed at the policy site. And third, the valuation procedures used at the study site should be analytically sound and carefully conducted (Pearce 1994). The Kerala study follows these guidelines exactly.

To address the benefit-transfer research question, the contingent valuation survey of households at the A sites was used to estimate a model explaining the probability of connection for them. As described in Singh and others (1993), respondents' choices were modeled using a random utility framework, in which the probability that a family chooses to connect is determined by household characteristics, characteristics of the improved supply system, and characteristics of alternative water supplies. Table 3 contains estimates for the full model using all observations for all A and B sites; appendix table A-1 contains analogous estimates for the three subsamples used in this section: all A sites, the scarce-water A site, and the scarce-water B site. Under reasonable assumptions on the distribution of errors, probit estimates are unbiased. They are not efficient, however, because each household recorded repeated bids. The standard errors have been corrected for within-household correlations among groups of observations (described in detail in Singh and others 1993).

Before proceeding, it is important to explain what these estimating equations do and do not do. The coefficients are consistent estimates of the true effect in the population of each variable on the decision to hook up. Given the high *t*-statistics for the important variables, we can be fairly certain that if other samples were drawn from these populations, we would get roughly the same coefficient estimates. However, the pseudo R^2 statistics at the bottom of tables 3 and A-1 indicate that only about one-third of the variation in the dependent variable—whether to connect—is explained by the model. Although this statistic is relatively high for cross-sectional models, it suggests that the specified economic and social variables determine only a limited fraction of behavior.

Using this model for one site to predict responses at another site might be problematic for several reasons. For example, some variables might be out of range. The coefficients are valid only for the range of variables occurring in the sample observed in the first site. Another possibility is that the predictable component of behavior may be overwhelmed by unobservable and site-specific effects and random effects. Still another possibility is that comparison across time periods without new information might be problematic, in that some of the observed variables in the equation may have changed, but the changes cannot be observed. One example is income. In predicting behavior in 1991, we bumped up every household's income by the real growth rate of the economy, but doing so fails to capture relative changes in real income across the households.

Using models of behavior at the A sites to predict behavior at the scarce-water B site gives completely inaccurate predictions. This is true whether households in all the A sites or only the households in the scarce-water A site are used (table 4). We assume that the results using the scarce-water A site would be the most accurate, as this site is the closest sample to the scarce-water B site. Using the benefit-transfer approach, we predicted that a very high proportion of the B-site households would connect—76 percent (128/169). From table 4, only 16.6 percent (28/169) of the families connected. The difference is statistically significant. Gross accuracy is 41 percent, specificity of connector predictions is 22 percent, and sensitivity of nonconnector predictions is 100 percent. In short, the performance of the benefit-revelation method (based on the responses to contingent valuation questions at the B site) is vastly superior to that of the benefit-transfer method (based on modeling behavior at the A site and transferring the results to the B site).

We do not have to look far for reasons why the benefit-transfer approach works so poorly in this case. The basic reason is not model specification (in the next subsection we demonstrate that using data from the B site to estimate the coefficients in the model tends to overpredict connectors slightly, but otherwise performs well in explaining behavior), but the much lower bids in the B site in 1988. The descriptive statistics at the bottom of table 1 show that the average connection bid in the B sites in 1988 (Rs267) was only 75 percent of the average for the nonconnecting A sites (Rs355). The average maximum bid for the monthly tariff in the B sites was Rs5.5, only 28 percent of the average for the connecting

A sites (Rs19.3) and 63 percent of the average for the nonconnecting A sites (Rs8.7). The coefficients for the dummy variable for the villages in the B sites in the full model (table 3) are negative and highly significant. In other words, the households in the B sites placed a relatively low value on the new water system in 1988, and they were telling the truth about it: their responses in 1988 predicted their own behavior very well. The respondents in the A sites turned out to be very poor substitutes for the respondents in the B sites, apparently because of unobservable, site-specific factors.

Behavioral Modeling

As described above, the benefit-revelation method gave reliable predictions of actual behavior at the B site. Although this is an important finding, in practice it provides an answer to only part of the question of interest to researchers and policymakers, who are also concerned with likely responses to changes in a cluster of related policy variables (such as tariffs, connection charges, and reliability) while controlling for other confounding variables (such as education and income). The simple tabular analysis of the contingent valuation data from the B site cannot, of course, answer such questions.

The numbers in table 4 were generated by predicting behavior in the scarce-water B site using only the data from that sample and the model appearing in the third column of appendix table A-1.[7] In table 4, the sample size is increased to 169.[8]

We had predicted that 13.6 percent (23/169) of families would connect, whereas in fact 16.6 percent (28/169) of the larger sample of families did connect. Thus, the model meets our criterion of being conservative, that is, it underestimates connections. The difference is not statistically significant at the 5 percent level. The gross accuracy was 82 percent, compared with 91 percent for the simple analysis. The specificity of connector predictions fell from 69 percent in the simple case to 43 percent. The sensitivity of nonconnector predictions was 88 percent, down from 95 percent in the case of the simple statistics.

The behavioral model estimated using the contingent valuation data from the B site does not predict behavior quite as well as the simple descriptive statistics do. Because the behavioral model should give us more information rather than less, this result suggests that functional form and distributional assumptions may be more restrictive than we expected. But we doubt that conclusion and attribute the lower accuracy to the fact that the model allowed us to include those observations at the maximum bid that had to be dropped for table 4 (under "benefit revelation"). These households introduced virtually all the additional error. If we use behavioral modeling to predict for the same sample that appears in table 4 (under "benefit revelation"), we get similar results to those in that table (results not reported here).

Thus, inferences from the econometric model probably provide good, conservative guidance for water policy. Inferences from the model about elasticities (with respect to price, income, and reliability, for instance) and changes in welfare are likely to be reliable. This result suggests that the strong policy conclusions of the initial study (Singh and others 1993) should be taken quite seriously. That article suggested that substantial improvements in welfare would be possible with drastic reform of water sector policies in Kerala. Our results do suggest, however, that considerable care should be taken to make sure that the range of responses to the contingent valuation questions do not artificially censor those who might bid very high or very low. In the simple benefit-revelation case, the censoring requires us to throw out observations; in the behavioral model case, it reduces the predictive accuracy of the model, even though we can predict values for those observations.

IV. RESULTS FOR THE SALINE-WATER SITE

We will only briefly present the results for the saline-water site because the results buttress the findings from the scarce-water site but are less interesting and less reliable.[9] As already noted, sample attrition was much greater in the saline-water site and was concentrated among the poorer households (we lost 30 percent of the sample, compared with only 13 percent for the scarce-water site). In the saline-water site, we lost twenty-one households for the benefit-revelation exercise because they were at the maximum bid for connection cost. Nineteen of these did not connect, and two did. We lost an additional three households with a missing value for connection cost in 1991.

The results for the saline-water site are shown in table 6. We predicted that 0 percent (0/117) of families would connect, whereas in fact 15.4 percent (18/117) of families did connect. So, although we were infinitely wrong about the proportion connecting, we were at least wrong in the desired direction, underpredicting connectors.

Our failure to predict connectors appears to have been caused by exogenous changes that took place at the saline-water site between surveys. During the

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[7] We also performed the analysis summarized in table 4 (under "behavioral modeling") using the whole-sample estimates presented in table 3. The results are almost identical. The counterpart to table 4 is available from the authors.

[8] The change in sample size from 148 to 169 can be explained as follows: we started with 148 in table 4 (under benefit revelation), and then we gained 13 that had been dropped because their bids were at the maximum. For these households, we could predict the probability that they would connect because of the properties of the estimating equation. We gained another 12 households, for which the willingness-to-pay data were missing in 1988, but for which we knew whether they connected in 1991. For these households we could predict whether they would connect using the model. We lost 4 households with a missing value for one or more independent variables in the equations, even though we knew the outcome variables for them.

[9] We do not include here the additional analyses performed for the scarce-water site because they do not add new information. The results are basically the same as those for the scarce-water site, with the benefit-transfer model performing slightly better than for the scarce-water site (but still badly overpredicting connections) and the behavioral model basically replicating the benefit-revelation results with less accuracy. These results are available from the authors on request.

Table 6. *Comparison of Predicted Behavior of B-Site, Saline Water Area Households in 1988 and Actual Behavior in 1991*
(number of households)

	Actual behavior[b]		
Predicted behavior[a]	*Did connect*	*Did not connect*	*Total*
Will connect	0	0	0
Will not connect	18	99	117
Total	18	99	117

a. Based on the maximum bid that households were willing to pay for a connection in 1988.
b. Based on the actual number of connections after true cost was known.
Source: Authors' calculations.

follow-up survey, 78 percent of the households in this area that did connect, despite our predicting that they would not, cited "unanticipated improved economic circumstances" as the reason for connecting. Another 22 percent said it was because they could borrow money for the connection (in contrast, no one in the scarce-water site borrowed to finance the connection).

The gross accuracy of predictions based on the intentions households declared in the 1988 contingent valuation survey was 85 percent. Thus, even though we predicted no connectors, because the proportion connecting was small, we did very well in predicting behavior for the whole sample. The specificity of connector predictions was 100 percent. We were by definition perfectly correct in getting our predicted connectors right, because we predicted none. The sensitivity of nonconnector predictions was 85 percent.

The results for the saline water site show the fragility of our endeavor to compare bids with behavior three years later, with almost no information about what happened in between. Even so, the results are fairly robust, despite our misgivings even about the 1988 data in the saline-water site. For example, analysis of the 1988 data suggested that a family in a saline-water area was willing to pay less for an improved water supply than a similar family in a water-rich area (see table 3), which was counterintuitive. But more important, between the first and second rounds of the surveys, there were major exogenous changes in the saline-water villages.

V. CONCLUSIONS

Contingent valuation studies suffer from three potential sources of bias (Cummings, Brookshire, and Schultze 1986). First, strategic bias might arise because respondents perceive it to be in their interest to respond inaccurately to the questions. Second, hypothetical bias might arise because respondents are not fully acquainted with the good or service in question. And third, compliance bias might arise because respondents give replies they believe the questioners would find most satisfactory. A priori, in this particular setting, it might be predicted that strategic biases would be relatively high, with respondents underestimating their willingness to pay in the hope that this might lower the tariffs that would be charged. Hypothetical biases would be relatively low because the good in question—a piped water supply—is familiar to all. Compliance biases might be relatively high, because this has happened with surveys in the Indian subcontinent in the past (for example, Mamdani 1972).

The comparison of actual behavior with that emanating from the contingent valuation survey amounts to a resounding confirmation that, *in this particular study,* the net effect of these biases was small. The caveat regarding "this particular study" cannot be overstressed for several reasons. First is the issue of strategic and compliance bias. Recognizing the potential for these sources of error, the study made strenuous efforts to reduce to a minimum the effects of these biases: by explaining procedures in the survey; by being—and being seen as—independent of the supply agency; and by training interviewers rigorously. There is considerable evidence that "quick and dirty" willingness-to-pay surveys of a similar nature in the past have yielded nonsensical results (Saunders and Warford 1972). Accordingly, the survey instruments were developed with great care in the course of a multicountry study (World Bank Water Demand Research Team 1993) and were already "tried and tested" by the time of the 1988 survey in Kerala. As shown in table 2, the instrument met the NOAA standards five years before the standards were developed. Furthermore, the instrument was carefully pretested in Kerala, and modifications were made so that it was appropriate for the local setting.

Second is the issue of hypothetical bias. Proponents of the contingent valuation methodology have understood for some time that the greatest problem for contingent valuation studies arises not from strategic but from hypothetical bias (see Cummings, Brookshire, and Schultze 1986). Stimulated by the controversy on the damage caused by the Exxon Valdez oil spill in Alaska, a rigorous and heated debate on the contingent valuation methodology has taken place, with much of the attention focused on the issue of hypothetical bias (see Portney 1994; Hanneman 1994; and Diamond and Hausman 1994). Diamond and Hausman (1994: 62) have made a forceful denunciation of the method, arguing that "contingent valuation is a deeply flawed methodology for measuring nonuse values." With respect to this controversy, the Kerala water study simultaneously (a) provides clear evidence that carefully conducted contingent valuation studies can provide reliable information on how people value well-defined goods and services and (apparently paradoxically) (b) does *not contradict the concerns that underlie the Diamond-Hausman argument.*

The key issue here is that, even though the good at stake in the Kerala study was tangible and simple, there was still a problem with hypothetical bias in parts of the study. This problem emerged in the finding that an important service characteristic—reliability—was of major importance to those who were already connected, but was not perceived as being important by those who had not directly experienced the service. Well-conducted contingent valuation studies can provide reliable and valuable information on behavioral responses to well-defined and well-understood goods such as a household water supply. But this finding in no way vitiates the very serious problems arising when this method is used to assess such abstract concepts as "existence values." In the words of Diamond and Hausman (1994: 62), "it is precisely the lack of experience both in markets for environmental commodities and in the consequences of such decisions that makes contingent valuation questions so hard to answer and the responses so suspect."

Benefit-Transfer Literature

The results are equally striking for the prospects of using the estimates based on a behavioral model for one population to predict the behavior of another population. Virtually all the characteristics of the study population and the alternative water sources are apparently quite similar at the A and B sites. When behavior at the scarce-water B site was estimated from a well-specified and carefully estimated model of actual behavior at the scarce-water A site, however, predictions were wrong for about half of the sample. The number of connections was overestimated by a factor of four.

This finding is not surprising, for two reasons. First, even when the determinants of behavior are easy to specify (as in this case), detailed models of this behavior are formulated, the full required set of data is collected, and sophisticated statistical tools are applied, less than one-third of variance can be explained (see table 3). Second, the results of a multicountry study of willingness to pay for water (World Bank Water Demand Research Team 1993) shows that both appropriate specifications and parameter estimates vary considerably in different locations.

Attempts to estimate behavior (and thus benefits) in a particular community on the basis of results of studies in other communities in other settings can reach conclusions that are seriously erroneous. This can occur even when the communities, the natural conditions, and the service to be offered are apparently quite similar. Substantial additional information is collected when the expected beneficiaries are interviewed directly.

Assessing the Demand for Services through Behavioral Models

Benefit revelation through direct methods has great potential for assessing the demand for services, especially for capital-intensive and costly services such as water supply in developing countries. Carefully designed and conducted contingent valuation studies can produce reliable estimates of the demand for water and sanitation and are appropriately becoming widely used for this vital function (Whittington and others 1992; Altaf, Jamal, and Whittington 1992).

The sample size requirements are modest (a couple hundred families sufficed in this case). As experience with these studies accumulates, it has been possible to substantially improve quality, increase speed, and reduce cost by the judicious use of off-the-shelf survey components. If the policy interest is limited (in the present case, to the number of families who would connect to a new supply), then simple tabular analyses may suffice. If the policy interest is more complex (for example, elasticities with respect to price and service reliability; simulations of policy changes; or welfare analysis), then behavioral models using econometric techniques need to be estimated.

Caveats

The contingent valuation method is validated under a very specific set of circumstances. Of particular importance (as stressed earlier) is the fact that hypothetical biases for the service evaluated in this study are much more limited than for many of the environmental resources that the technique is typically used to value. Of equal importance is the careful design and conduct of these studies. In all cases this meant several weeks of pretesting and adaptation to local circumstances, meticulous training and supervision of interviewers, and careful cleaning of data. More specifically, it is noted with concern that the relative success of the set of studies of which the Kerala one is part (World Bank Water Demand Research Team 1993) has inspired some investigators to conduct two-day studies of hundreds of households to determine willingness to pay. In the past such studies gave willingness-to-pay surveys a (well-deserved) bad name. The old adage of "garbage in, garbage out" is certainly applicable to such poorly conducted studies.

In some circumstances, researchers and policy analysts will have access to rich data sets on populations that (a) already have access to the service of interest and (b) are very similar to the population for which the service is to be introduced. In such circumstances, it would appear that carefully specified and estimated models of behavior could be used to predict behavior by the unserved population. The results of this study, however, show that site-specific factors are of major importance and that predictions based on extrapolations may be far off the mark, even when many conditions in the population and environment are apparently similar.

What We Learned That Can Improve Survey Design

Much more care should be exercised in defining the range and the increments for bidding games or referenda. They should be connected as closely as possible to actual costs, or the ranges should be pretested for validity. Often researchers pick very high willingness-to-pay bids for the top of the range, which they think are higher than anyone would pay, yet when the data come back they show that large proportions of the sample have chosen the highest bid. That does not give us good information about willingness to pay. In our case, despite efforts to design the best possible survey in 1988, the top of the ranges for tariffs and connection charges was too low. For example, in the scarce-water site, one connector who bid at the maximum of Rs700 in 1988 actually paid Rs2,547 for the connection. In this case, we could have avoided the problem by reviewing the range of actual connection costs in 1988 before finalizing the survey.

Quick analysis of the survey data immediately after the fieldwork, with a plan to return to the field for additional work, would resolve many questions that we had in the analysis. In the saline-water site, for instance, we should have caught right away (in 1988) that the bidding behavior was not as we expected, and households should have been reinterviewed. After the 1991 survey, we should have immediately gone back to understand better why six households that had bid Rs100 in 1988 for connections actually hooked up at an average cost of Rs1,380. Doing the analysis quickly in the field, preparing a qualitative questionnaire to complement the quantitative work, and aggressively addressing issues of data quality could raise the validity of benefit-revelation techniques considerably and increase their value for assessing demand.

If other researchers conduct the same type of test reported in this article, we suggest collecting additional information about the households over the elapsed time between the hypothetical bids and the actual choice. Repeated observations on income, assets, family size, community characteristics, precipitation, and traditional water quality would help control for endogenous and exogenous changes over the period.

Implications for Kerala

To the extent that contingent valuation, or benefit revelation, has been validated by this exercise, it has been driven by the nonconnectors. We predicted that very few would connect to the water systems, given the policies in place. We were right. Standard water systems as designed in India and in much of the developing world do not make people better off. People respond by letting the systems fall apart or by telling us, as they did in the 1988 survey, that they would not connect even at the fairly low prices that were quoted. Their behavior in 1991 just turned the hypothetical rejection of the system by the vast majority of the sample into an actual rejection.

We also predicted, on the basis of the 1988 data, that once people connected, they would become concerned about the poor quality and low reliability of the system. They would not care much about these problems unless they connected. We were right again.

Although we would not have been able to make these predictions without a survey and a technique that worked as expected, we still do not know if we were right about the prescriptions we offered for solving these problems. On the basis of simulations, we recommended that the water authority raise tariffs, fold the connection cost into the tariff, and vastly expand access to private connections. We predicted that both connections and revenue would explode. People would consider themselves much better off. They would begin agitating to pay more to create a more reliable system. Making these policy changes, and tracking the results, would be the true test of the work. We know the technique worked. We do not know yet if the economic analysis was accurate, because the changes in policies that we recommended as a result of the analysis have not been tested.

Table A-1. *1988 Survey: Probit Estimates of Probability of Choosing a Yard Tap for Subsamples*

Variable	*All A sites*	*Scarce-water A sites*	*Scarce-water B sites*
Constant	–0.3302	0.6083	–0.1693
	(–0.877)	(1.00)	(–0.47)
Tariff	–0.0520	–0.0461	–0.0947
	(–15.57)	(–10.54)	(–6.08)
Connection charge	–0.0010	–0.0008	–0.0017
	(–8.32)	(–3.76)	(–8.49)
Improved service	–0.1394	0.0678	
	(–3.15)	(0.97)	
Distance to current source (meters)	0.0103	0.0120	0.0013
	(0.75)	(1.31)	(2.83)
Queue at current source (minutes)	0.0013	0.0067	–0.0076
	(0.64)	(0.82)	(–1.33)
Per capita income (rupees)	0.00004	0.00005	–0.000001
	(1.99)	(1.05)	(–0.04)
Household has electricity	0.0024	0.3689	0.2937
	(0.02)	(1.29)	(1.35)
Number of rooms in dwelling	0.0441	0.0721	0.2677
	(1.53)	(1.33)	(2.75)
Household has females in government service	–0.1894	0.1652	–0.3037
	(–0.67)	(0.48)	(–0.41)
Household has males in government service	–0.1495	–0.0694	0.0741
	(–1.15)	(–0.30)	(0.32)
Hindu household	–0.1138	–0.9599	–0.3984
	(–0.89)	(–3.24)	(–2.17)
Female-headed household	0.0153	0.5330	–0.1017
	(0.12)	(2.26)	(–0.51)
Female respondent	–0.3183	–0.3582	0.1192
	(–2.68)	(–1.66)	(0.71)
Some primary school	0.7960	1.2941	0.3312
	(2.30)	(2.47)	(1.47)
Primary school complete	1.1315	1.3099	0.2859
	(3.74)	(3.44)	(1.22)
Middle school complete	1.0451	0.3247	0.3709
	(3.13)	(0.73)	(1.41)
Secondary school complete	1.4341	1.7492	0.6238
	(4.77)	(4.60)	(2.63)
More than secondary school	1.7274	2.0476	0.4956
	(5.30)	(4.53)	(1.36)
Scarce-water area	0.6983	n.a.	n.a.
	(4.41)		
Saline-water area	–0.1446	n.a.	n.a.
	(–0.91)		
A-site nonconnecting household	–0.4557	–1.1346	n.a.
	(–3.14)	(–5.00)	
B-site village household	n.a.	n.a.	n.a.
Pseudo R^2	0.26	0.30	0.34
Sample size	5,228	1,700	1,416
χ^2 (degrees of freedom)	1,704(21)	697(19)	554(17)

n.a. Variable not appropriate to the sample.

Note: Dependent variable: choice to connect in the bidding game. Estimates are weighted by the population of the sampling unit. Standard errors are corrected using a method explained in Singh and others (1993). The estimating equations are significant at better than the 0.00001 level for a likelihood ratio test (χ^2). The omitted site dummy is A-site connecting households. *t*-statistics are in parentheses.

Source: Authors' calculations.

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