USE OF CHAMBERLAIN FIXED EFFECTS APPROACH TO ESTIMATE WILLINGNESS-TO-PAY FOR LITTLE TENNESSEE RIVER BASIN MANAGEMENT ALTERNATIVES

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Abstract

The paper discusses an application of Chamberlain's fixed effects model to contingent valuation method survey data obtained for eight management alternatives for the Little Tennessee River basin. The advantages of using this approach versus cross-sectional logit, pooled logit, and cross-sectional logit with lags are discussed and a technique to obtain willingness-to-pay estimates from estimated coefficients is offered. Drawbacks of using Chamberlain's fixed effects model, difficulties encountered, and directions for further research are presented.

Introduction and Objectives

The Little Tennessee River watershed is located in Georgia, North Carolina, and Tennessee. The watershed encompasses 10,783 acres, including 18 rivers and streams and 26 lakes. The Little Tennessee River (LTR) originates in Rabun County, Georgia; it flows north into North Carolina before terminating at Fontana Dam, just south of the Smokey Mountains.

The LTR watershed is used by logging, agriculture and mining industries; however the aesthetically pleasing environment in the basin has recently led to a tremendous increase in the population of people who visit and live within the watershed. In the last twenty years the population has doubled, leading to concerns about the future health of the watershed and the ecosystem services the watershed provides. The majority of land within the watershed is privately owned and private land use decisions have a major impact on ecosystem structure and function. For example, agricultural activities, such as watering cattle in streams, and development—housing and commercial development along the streams and creeks—influence water quality, a key parameter of ecosystem health.

The objectives of the initial study by Bergstrom *et al.* were "to develop and test a general methodology for valuing ecosystem services and to identify and value particular ecosystem services present in the Little Tennessee River watershed." To place a value on ecosystem services, a stated preference or contingent valuation (CVM) survey instrument was designed and implemented as an economic experiment.

Two valuation panels were held in Macon County, NC, at Franklin High School and Southwestern College. A total of 191 respondents 18 years of age or older completed the survey. The latter was presented to the two panels in different versions. The valuation question was posed the following way: "If a local county sales tax were to reduce your annual household income by $X each year for the next 10 years to support program X, would you vote in favor of it?"

Version 1 included 4 alternatives. Alternative 1.1 included the restoration of an additional 2 miles of stream bank along the 20 mile stretch of the LTR. Alternative 1.2 extended the proposed restoration to an additional 4 miles, and Alternative 1.3 included the restoration of 6 miles of stream bank. Alternative

1.4 did not contain any restoration propositions but was offering an overall watershed protection plan, which required buffer strips along all small streams and creeks running into the LTR.

Version 2 also included 4 alternatives. Alternative 2.1 was identical to Alternative 1.4, while Alternatives 2.2–2.4 offered the same restored stretches as Alternatives 1.1–1.3 along with the overall watershed protection plan, as described above.

The computer-assisted bidding followed a simple adaptation structure: say, if the respondent j has voted in favor of alternative k, $k=1..3$, then the bid for alternative $k+1$ would increase, otherwise $k+1$ would be offered at the same bid amount as k.

The objective of this *ex post facto* applied econometric analysis study was to use a panel data approach to the data generated through the above survey, with a view to obtain better quality estimates of willingness-to-pay (WTP) values and, more importantly, to address econometrically the issue of respondent heterogeneity with as little as possible assumptions about its nature.

Econometric Model

For each of the different programs, respondents were asked if they would vote to support the LTR protection program at the stated price or cost. Using the well known binary logistic model, we can statistically analyze a respondent's "yes" or "no" response. A binary response is recorded with a "yes" response indicated by $Y=1$ and a "no" response indicated by $Y=0$. If a "yes" is recorded for X$ then we know a particular person would pay at least X$, however he or she may be WTP more than X$. We can say that a "yes" response bounds the true WTP from below the bid offered. Similarly, in theory, then a "no" response will bound the true WTP from above the bid offered.

The logistic, often referred as logit model can then be written as:

$$(1) \qquad Prob(Y_i = 1) = \frac{e^{b' x_i}}{1 + e^{b' x_i}} = L(b' x_i),$$

where x_i is a vector including the bid amount, characteristics of the respondent, etc.

The seven different alternatives first were evaluated separately. Table 1 reports the parameter estimates for each model. First part of Table 1 presents results for the first version of the survey dealing

mainly with stream bank restoration. Second part of Table 1 presents results for the second version of the survey, which deals with stream bank restoration along with the idea of establishing additional buffer zones. The initial set of explanatory variables included: “Aware of restoration measures on LTR (binary yes/no),” “Participate in activities along LTR (binary yes/no),” “Household size (number of persons),” “Education (levels coded 1 to 5, from elementary school to graduate, respectively),” “Gender (1 = female, 0 = male),” “Age (years),” “Bid amount (dollars), variable BID in tables.”

Observations featuring at least one indecisive vote (“do not know”) were not used in estimation to avoid any misinterpretations. This effectively reduced the size of samples from 95 to 64 for survey Version 1, and from 96 to 58 for survey Version 2. Regressors which were found to be largely insignificant across programs and having no substantial influence on the likelihood function value were removed from the model for parsimony sake. Educational attainment was raised to the fourth power to account for nonlinearity, variable EDUC4 in tables.

As one can see, the simple cross-sectional logit model does not appear to be adequate for the case in hand. Education seems to have some influence on the voting outcome, while estimates for bid amounts turned out to be reasonably significant for Alternatives 2.1 and 2.2 only.

One can reasonably hypothesize that the reason of the apparently poor data fit lies in omitted important characteristics of respondents, which made the data heterogeneous. Indirect evidence to this comes from logit regression of votes on their lagged values, variable VOTE in tables, and bid amounts. The results are reported in Table 2. Based on the estimates, it is obvious that votes in series strongly correlate; in other words, if a given person had voted, say, “yes” for Alternative 1.1 (or Alternative 2.1 for Version 2), he/she would have been highly likely to vote “yes” for the remaining alternatives in spite of differing bid amounts and the weights he/she attached to those alternatives. Here, one can notice another problem with the cross-sectional approach: given the fact that at least three programs in each version are very similar, it is hard to believe that respondent would employ different decision rules, which would, in turn, necessitate separate estimation.

If the above situation takes place indeed, then cross-sectional logit estimates will be inconsistent and the cross-sectional approach cannot be used. A reasonable alternative is to view the data in a panel data sense. If we do not want to make any distributional assumptions about the omitted regressors, we can consider them to be fixed effects in now the panel model.

A fixed effects panel logit model is

(2) $$Prob(y_{it} = 1) = \frac{e^{a_i + b' x_{it}}}{1 + e^{a_i + b' x_{it}}} = L(a_i + b' x_{it}),$$

where a_i is an individual effect, which is constant to the given individual, i.e. constant across series.

To re-analyze the data, Chamberlain's conditional maximum likelihood estimation method was chosen; this technique is commonly known as Chamberlains fixed effects model (FE). Under the FE model a conditional likelihood function,

(3) $$L^C = \prod_{i=1}^{n} Prob\left(Y_{i1} = y_{i1}, Y_{i2} = y_{i2}, \ldots, Y_{it} = y_{it} \mid \sum_{t=1}^{T} y_{it} \right),$$

is used to obtain a conditional maximum likelihood estimator (CMLE) instead of maximum likelihood estimation (MLE) procedure employed for cross-sectional or pooled logit. As its key quality, Chamberlain's FE model enables consistent estimation of all parameters in (2) except person-specific intercepts while requiring no distributional assumptions about individual effects whatsoever. One can see Chamberlain for further particulars of the technique.

In parallel to fitting Chamberlain's FE model, formal testing for fixed effects was performed using an adaptation of Hausman test, available from Hsaio. As with any Hausman specification test, both MLE and CMLE are consistent and CMLE is inefficient under the null, while the alternative is the inconsistency and inefficiency of MLE, with CMLE being consistent and efficient. The results are presented in Table 3.

Statistical Analysis

The estimates for survey Version 1 are significant and have proper signs, which may imply a better and less noisy fit after the removal of heterogeneity. Hausman's statistic is significant at 95% confidence,

which is consistent with the presence of fixed effects. However, the estimates for Version 2 do not reflect much improvement. Hausman's statistic is still marginally significant, which means fixed effects are possibly present, but the coefficient estimate for bids turned out to be absolutely insignificant.

At a glance, this may seem paradoxical, given the fact that it was survey Version 2 that yielded the only two significant estimates for bid amount in the cross-sectional approach. Nevertheless, there is a possible explanation to this outcome. In Version 1, the "fine-tuning" of the individual's WTP bound started from the first alternative offered, i.e. the computer-generated bid for Alternative 1.2 depended on the vote for Alternative 1.1. In Version 2, however, first two Alternatives (2.1 and 2.2) were offered at the same bid level, while the adjustment began for the two remaining two. Therefore, the valuation process for the entire Version 1 and the second part of Version 2 had similar dynamics, while its dynamics for the first part of Version 2 was different, hence the structural changes.

Unfortunately, it is hardly possible to formalize the conjectured structural changes, and/or to test for them formally in the context of Chamberlain's fixed effects model. Since the bid amount remained constant in the first part of Version 2, it cannot be used in CMLE for this part, thus making tests based on difference of coefficient estimates or likelihood ratios inappropriate.

WTP Estimation

Lacking any practical meaning, the estimated coefficients in (2) are of little use; the matter of our primary concern—expected WTP—were needed to be estimated. According to the utility difference model by Hanemann, expected WTP is calculated as

(4) $$E(WTP) = \int_0^\infty [1 - F(dV)]\, dV,$$

where dV is the difference in indirect utility and $F(dV)$ is the probability of a "no" response.

Obviously, the estimates of the coefficients for bid and alternative weight would not suffice to estimated the probability of either response. However, we know of the sufficiency of $\sum_T y_{it}$ for a_i. We also have consistently estimated coefficients in the model. If we make an assumption that a consistent

estimator of a_i can be approximated by a linear function of $\sum_T y_{it}$, it is quite possible to perform a pooled second step restricting weight and bid coefficients to their estimated values, to obtain the estimates for the fixed effect and the constant term.

The empirical distribution of resulting WTP values was found to highlight sample selection problems. In Version 1, approximately third of all WTP values fell within \$0–\$10, another third was within \$335–\$600, with the rest of estimates scattered more or less uniformly in between. Version 2 showed a roughly uniform distribution of WTP estimates between \$1.5 and \$45 for Alternatives 2.1 and 2.2, and between \$1.5 and \$75 for Alternatives 2.3 and 2.4. The results for the entire Version 2, however, are hardly of any use, since the very small and insignificant estimate of the bid coefficient was used in integration to obtain estimated WTP.

The distribution of estimated WTP for alternatives in Version 1 is apparently bimodal, which reveals the existence of two large groups of respondents with polar views of either river value or suggested payment vehicle, a tax. One can also easily notice this from aforementioned strong longitudinal correlation of responses. Apart from sample selection problems, this type of correlation means that the "extrapolation" of coefficients estimated from part of observations (series of all zeros and all ones contribute nothing to conditional likelihood) might be questioned.

Discussion and Conclusions

We have seen that the use of Chamberlain's FE approach allows us to eliminate the influence of fixed effects in the panel binary response model and, with the assumption that a reasonable quality estimator for an effect term can be approximated as a linear function of the sum of ones in the series, to estimate WTP. However, such an approach has several weak points.

First, although Chamberlain's FE model does indirectly address the issue of protest votes (understood herein as voting "yes" or "no" for reasons other than the pure neoclassical utility maximization), it basically excludes probable protest voters, i.e. all homogeneous series, from further consideration up until the point where WTP is estimated. If respondents that produced such homogeneous

series are indeed protest voters, then (4) is simply not valid for them. Further, if a stochastic term in the respondent's decision whether or not to protest is correlated with the disturbance in the binary response equation, neither MLE nor CMLE are consistent.

Addressing this kind of problem, Volinskiy attempted to apply a bivariate probit model with censoring to the data from the LTR survey, also introducing the notion of discontinuous utility function. The study returned mixed results though, most probably due to inadequate representation of the selection mechanism by ordered probit.

Another problem lies in indecision, "do not know" type of votes. In our case such votes seriously reduced available sample sizes, which is definitely an unwelcome situation. The literature offers a wide range of opinions as to what economic theory indecision votes pertain to but there is little established practice of treating such votes econometrically. In the meantime, the problem exists and should be dealt with one way or another.

Last but not least, the dataset-specific adaptive bid generation algorithm was not taken into account in the model. Again, there is no ubiquitous way to incorporate this sort of dynamics into panel binary response models. A promising direction for research in this respect may be to use evolutionary game theory, at least to reflect respondents learning in the process of survey.

In the meantime, the authors believe that Chamberlain's fixed effects model can be very instrumental when dealing with unobservable individual effects in panel data generated through a CVM survey, and that it does merit further investigation and application in this respect.

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Table 1. Simple Cross-Sectional Logit.

Variables	Management Alternatives		Survey Version 1, sample size N=64	
(est.coeff./std.error)	1	2	3	4
C (intercept)	-0.9393	-0.7938	-0.4034	*-1.0806
	(0.5864)	(0.5476)	(0.5200)	(0.5858)
BID	0.0108	0.0064	-0.0009	-0.0015
	(0.0086)	(0.0045)	(0.0026)	(0.0083)
EDUC4	*0.0024	0.0018	**0.0026	*0.0021
	(0.0013)	(0.0012	(0.0013)	(0.0012)
			Survey Version 2, sample size N=58	
C (intercept)	-0.3983	-0.0279	**-1.2558	0.3555
	(0.5512)	(0.5540)	(0.6167)	(0.5264)
BID	**-0.0243	**-0.0260	0.0098	0.0025
	(0.0106)	(0.0107)	(0.0093)	(0.0044)
	**0.0030	0.0013	*0.0023	0.0008
EDUC4	(0.0014)	(0.0014)	(0.0014)	(0.0014)

*** - significant at 99% confidence

** - significant at 95% confidence

* - significant at 90% confidence

Table 2. Cross-Sectional Logit with Lagged Vote as a Regressor.

Variables	Management Alternatives		Survey Version 1, sample size N=64	
(est.coeff./std.error)	1	2	3	4
C (intercept)	–	***-2.0110	*-0.8371	***-2.8804
	–	(0.7692)	(0.4523)	(0.8933)
VOTE (previous)	–	***7.8711	***4.2491	***3.1644
	–	(2.3736)	(0.9619)	(0.8398
BID	–	*-0.0269	**-0.0078	0.0088
	–	(0.0145)	(0.0032)	(0.0104)
			Survey version 2, sample size N=58	
C (intercept)	–	*-1.6943	***-2.2887	0.0140
	–	(0.7038)	(0.7803)	(0.3786)
VOTE (previous)	–	***3.5781	***2.8984	***3.0193
	–	(0.7958)	(0.7818)	(0.9864)
BID	–	-0.0177	*0.0233	-0.0048
	–	(0.0137)	(0.0135)	(0.0046)

*** - significant at 99% confidence

** - significant at 95% confidence

* - significant at 90% confidence

Table 3. Chamberlain Fixed Effects to Pooled Logit Comparison.

Variables	Survey Version 1, sample size N=64, 4 periods	
(est.coeff./std.error)	Pooled	Chamberlain FE
C (intercept)	-0.1300	–
	(0.2804)	–
BID	0.0021	**-0.0144
	(0.0019)	(0.0069)
WEIGHT	0.0341	***0.6441
	(0.0653)	(0.2337)
Hausman χ^2/DF	**8.7/2	
	Survey Version 2, sample size N=58, 4 periods	
C (intercept)	***-0.9480	–
	(0.3508)	–
BID	-0.0002	-0.0031
	(0.0007)	(0.0108)
WEIGHT	***0.0564	***0.5322
	(0.0181)	(0.1540)
Hausman χ^2/DF	*5.3/2	

*** - significant at 99% confidence

** - significant at 95% confidence

* - significant at 90% confidence