Survey protocol and income effects in the contingent valuation of public goods: a meta-analysis

Felix Schläpfer

Dr. Felix Schläpfer, Institute of Environmental Sciences, University of Zurich, Winterthurerstrasse 190, 8057 Zürich, Switzerland, *E-Mail:* schlaepf@uwinst.unizh.ch

Abstract

Income effects reported in contingent valuation (CV) studies tend to be much smaller than those found in the literature on collective choice (CC). The present study uses meta-analysis to explore determinants of the income effect in a sample of CV surveys. The probability of significant income effects was higher when 'progressive' payment vehicles were used and tended to be lower when cost distribution and institutions were well defined, when the choice was formulated as a policy referendum, or when 'passive-use' goods were involved. An interpretation of this pattern in terms of respondent behavior is proposed which also accommodates the CV/CC disparity.

JEL classification: C91, Q26
Keywords: Contingent valuation, Income effect, Meta-analysis, Stated preferences, Willingness to pay

1. Introduction

The 'income effect' in contingent valuation studies, as defined e.g. in Horowitz and McConnell (2003), measures the change in stated willingness to pay for a proposed good associated with a change in income. Estimates of the income effect are of interest for several reasons. First, the income effect is widely perceived as a useful indicator of internal validity of survey responses: Lack of a positive income effect is commonly interpreted as an indication that respondents did not seriously consider their budget constraint when making hypothetical choices. Second, reported income effects (or income elasticities) play an important role in recent attempts to explain the gap between willingness to pay (WTP) and willingness to accept (WTA)[1]. Finally, the distribution of benefits among different incomes may be important for the design of acceptable financing mechanisms. Nevertheless, there has been little attention on the effects of survey protocol on income effects reported in the contingent valuation literature. This is surprising especially since the few existing studies that compile income effects or income elasticities from stated preference surveys report values that appear to contradict those reported in studies on collective decision making. Kriström and Riera (1996) and Horowitz and McConnell (2003) report income elasticities of around 0.2–0.4. In contrast, Borcherding and Deacon (1972), in a classic paper on the demand for the services of non-federal governments, found income elasticities for various public goods, including parks and recreation, to be greater than one. Similar results were obtained by Bergstrom and Goodman (1973). McFadden and Leonard (1993) and McFadden (1994) challenged the evidence from stated preference surveys, in part on the basis that income elasticities less than one do not accord with economic intuition (see Flores and Carson 1997.) The present study attempts to shed light on this unresolved disparity by exploring the determinants of the income effect in a sample of recently published contingent valuation surveys using meta-analysis regression. Previous studies have applied meta-analysis to consolidate results of contingent valuation surveys for individual classes of goods such as groundwater quality or recreation benefits[2]. Further, meta-analysis was used by List and Gallet (2001) to study determinants of the disparity of actual *vs.* hypothetical WTP for private goods and by Horowitz and McConnell (2002) to examine factors explaining the magnitude of WTA/WTP ratios.

[1] See e.g. Hanemann (1991), Sugden (1999), Horowitz and McConnell (2003).
[2] See Bateman and Jones (2003) for an overview of these approaches.

2. Analytical framework

The present study conceptualizes the income effect observed in stated preference surveys on public goods as a function of both properties of the public good and characteristics of the survey protocol. The characteristics of the survey protocol include the question format and the specifications of the implementation rule, the payment vehicle, and the institutions entrusted with public good provision. The objective of this study is thus to organize properties and characteristics of the public goods and survey protocols into a set of reaonably well-defined variables and then to regress a measure of the income effect on these variables.

Income effect coefficients in contingent valuation studies are sometimes reported as coefficients on a continuous income variable and sometimes as coefficients on dummy variables for two or several income classes. Moreover, effects are sometimes those on open-ended WTP responses and sometimes those on the probability of accepting a proposed policy scenario or other offer, as in dichotomous choice question formats. Due to the difficulties involved in deriving a parameter of the income effect that is comparable across studies, and also to allow a maximum number of observations or survey studies to be included in the dataset, the present study does not focus on the magnitude of the income effect. Instead, it pursues the more modest objective of examining the determinants of the presence/absence of a significant income effect in a binary (logistic) regression framework:

$$\text{Prob}[IES = 1] = \frac{e^{X'\beta}}{1 + e^{X'\beta}} \qquad (1)$$

X is an n by m matrix with rank m, where the m regressors are the experimental protocol and other independent variables. The dependent variable, *IES,* was coded as follows: 1 = significant positive income effect reported ($P < 0.1$), 0 = income effect not significant ($P \geq 0.1$) or not reported. It appears reasonable to assume that, for an internal validity check or to improve the fit of their models, most researchers had tried to fit income as a covariable at some point in the process of data analysis and would likely have reported any significant effect.[3] When several regressions with different dependent variables were reported the income effect was coded as significant when income was significant in at least half of the models.

[3] An example and rationale for this procedure is provided by Poe and Bishop (1999, p. 361).

Characteristics of the studies and survey protocols were selected and included as explanatory variables in regression models based on their (perceived) meaningfulness as determinants of the income effect. Variables for sample size and choice format (closed-ended vs. open-ended) were included to control for trivial variation in effects due to differences in statistical power. Properties of the public good may determine the income effect due to differences in the availability of private good substitutes. Payment vehicle and cost distribution may affect the income effect due to income-specific fairness perceptions and related rejection or protest responses (Arrow et al. 1993, Morrison et al. 2000). Further, as suggested by Cummings and Taylor (1998), the institutional context and hence the 'realism' or credibility of hypothetical offers specified in CV surveys may affect the way respondents consider their budget constraint, which could be income-specific. However, due to a lack of studies addressing income effects in contingent valuation surveys, these expectations are currently not well founded. The analyses of the present study are thus of a largely exploratory nature.

3. Data

Selection of survey studies

This analysis covers original studies reporting on applications of stated preference survey techniques to estimate WTP for environment-related public goods. Since the literature on applications of stated preference techniques to public goods is extremly large this survey somewhat arbitrarily considers only studies published in the five academic journals which are currently important outlets for SP survey research: *Land Economics, Ecological Economics, Environmental and Resource Economics, American Journal of Agricultural Economics,* and *Journal of Environmental Economics and Management*,. The sample of studies was further limited to papers published during 1998 through 2002. This definition of the sample guarantees an overall high quality of the included survey studies, most of them conducted well after publication of the widely-cited NOAA panel report (Arrow et al. 1993). Studies reporting on stated WTP for recreation trips and for park access were not included because the good directly valued in these studies is a private one. To avoid pseudo-replication, decisions had to be made regarding the systematic selection of only one observation where several formats or samples were used for valuing identical scenarios. When both CV and attribute-

based choice modelling (CM) approaches were used, the CV surveys was selected. When several different CV elicitation approaches were used within one study, the prefered (selected) approach was the dichotomous choice (single-bounded) format. Where studies reported results for different respondent samples, those with protest bids excluded were prefered. One study was excluded because it reported on a survey already described in another study of the sample.[4] These definitions yielded 64 relevant studies reporting on 83 different valuation scenarios. Thus there are 83 observations in the data set (see Appendix). Eleven of the observations are choice modelling (CM) surveys, the remainder are or include standard contingent valuation (CV) scenarios or "paired comparison" approaches (PC).

Classification of survey characteristics

Survey characteristics later used as independent variables in regression were defined as follows (see Tables 1 and 2).

Sample size.—This variables provides the the sample size of the selected survey (see previous section) or, if available, the sample size of the reported regression model.

Type of good.— This variable distinguishes 'passive-use' goods including habitat and biodiversity protection from other public goods such as pollution control or public recreation areas for which use values tend to be the dominant values.

Question format.— 'Open-ended' (OE) and 'closed-ended' (CE) question formats are distinguished.

Multiple-bounded format.— This binary variable distinguishes single-bounded from double- or multiple-bounded dichotomous choice formats.

Implementation rule.— This is a binary classification distinguishing studies that try to evoke a popular referendum or other voting scenario (suggesting a majority decision rule) from studies that do not suggest a specific implementation rule.

Providing institution.— Surveys suggesting provision of the good through actually existing institutions ('actual') are distinguished from those relying on ad hoc institutions such as "special trust fund" agencies or "special tax" vehicles (termed 'hypothetical') and those not specifying any institutions. If authors explicitly reported that details of the institutional framework of provision were presented to respondents, this was coded as hypothetical.

[4] This study is Riddel and Loomis (1998) (cf. Loomis and Gonzalez 1998). Of the Loomis (2000) surveys only those not otherwise represented in the sample were included.

Payment vehicle — 'Taxes' are defined to be payments to a government that are not related to the physical consumption of a service. In contrast 'charges' include utility and other service charges but also payments into designated trust funds, 'required contributions' as in Jorgensen and Syme (2000) and 'required donations' as in Rolfe et al. (2000). 'Donations' includes voluntary individual contributions and group contribution mechanisms with provision point rule. The latter are distinguished in a separate binary classification. 'Various' was used when costs were distributed over two or more payment vehicles

Cost distribution schedule.— The term 'progressive' is used for income or property taxes which are usually "progressive" (payments increase overproportionately with income). The term 'proportional or equal' is used when the payment vehicle is a utility or service charge and payments in relation to income may thus vary according to individual consumption decisions. 'Equal' codes for explicitly equal costs to households. 'None' is used with donation vehicles, while 'not specified' designates cases where no (explicit or implicit) information on the costs schedule in relation to income is available. 'Not precise' is used when a cost distribution is unclearly specified.

Well-defined distribution.— This classification is based on the previous two variables and indicates whether the respondent's share of the total costs, in the event of provision of the proposed good, is both coercive and well defined in a survey.

A further binary variable 'attitude variable included' was used as an explanatory variable to account for the fact that potential correlation of income with attitudes may have caused non-significance of the income effect in some surveys. However, this variable was not significant and was thus dropped for further analysis. All classifications are based strictly on the information available in the published journal articles. Lack of clear expectations regarding effect directions in the meta-analysis facilitated an unbiased classification of the characteristics. A table of the studies with their characteristics is provided in the Appendix.

The independent variables derived from these classifications and included in models are listed in Table 3. Since CM studies do usually not report effects of socioeconomic covariates these 11 studies were excluded for the regression analysis. The sample size of the two 'paired comparison' (PC) studies by Petersen and Brown (1998) and by Lockwood (1999) was not comparable with that in CV studies. For the study by Halvorsen and Saelensminde (1998) sample size was not available. These studies were excluded from the regressions, leaving a sample size of N=68 observations (sample A). Further, three "summary studies", Markowska and Zylicz (1999), Jorgensen and Syme (2000) and Jacobson and Dragun (2001), report on several different survey scenarios but lack detail of description.

These studies were removed in a second series of models (sample B, *N*=55). The variables PPMECH and MBOUND were naturally fitted only in interaction with other variables in the process of model selection.

4. Results

Descriptives

The frequencies of basic study characteristics in the sample are summarised in Table 1. Table 2 provides an overview of the survey protocols used. Since the study sample can be regarded as representative of current academic contingent valuation reseach, a few patterns in the distribution of survey characteristics are of potential interest. Merely 15% of surveys used a hypothetical policy referendum in their scenario description in spite of the explicit recommendation of the referendum context by Arrow et al. (1993) and Hanemann (1994). Only a minority of studies specified a coercive and well-defined cost distribution. Finally, about 50% of studies involved public goods with prevailing passive-use values. Parameter estimates for "income" as a covariable were reported for 47 (57%) of the 83 surveys. Of these parameter estimates 30 were significantly larger than zero (P<0.1). Of the 68 contingent valuation surveys included in regression analysis only 29 reported a significant positive income effect. This is 42% of all CV studies or 63% of the studies in which an income coefficient was reported.[5]

Regressions

Estimation results for equation (1) are reported (a) for the full sample of contingent valuation surveys (Table 1) and (b) for the reduced sample where four less detailed reports of multiple surveys were excluded (Table 2). All presented models are clearly significant. Models A3–A5 and B1–B5 each explain a substantial portion of the variation in the dependent variable (McFadden R^2 between 0.18 (Model A3) and 0.39 (Model B5) (Tables 4 and 5). A first (trivial) model shows that the variables 'sample size' (SSIZE) and 'closed ended format' (CE) are significant and affected the dependent variable in expected ways (Model 1A, 1B).

[5] One study reported a significant negative income effect (Huhtala (2000).

Including a dummy variable for multiple-bounded CE format (MBOUND) in interaction with CE did not further improve the explanatory power of survey format (Model A2, B2) and was therefore dropped in the further models.

The Models A3 and B3 include additional variables characterizing how clearly and how realistically the hypothetical public good provision was specified in the scenarios. Moreover the models include a variable for the 'use' *vs.* 'passive-use' nature of the good. Formulation of the scenario as a hypothetical policy referendum (variable REF) significantly reduced the probability of a significant income effect. Actually existing institutions in the scenario (ACTINST) and a well-defined cost distribution (WDDIST) were not significant individually but their interaction tended to reduce the probability of a positive income effect. These results remained robust in the subsequent models. The type of good (PASSIVE) did not affect or only weakly reduce (Model B5) the chance that an income effect was observed.

Inclusion of the payment vehicle variables (CHARGE, VOLUNT, PPMECH, TAX) slightly improved model fit (A4, B4). Tax vehicles (TAX) tended to be more frequently associated with positive income effects than the other vehicles. The negative sign of DONATE is surprising and may suggest that these surveys did not always produce economically plausible patterns. The variables representing different cost distribution among income levels (EQUAL, EQPROP, PROGR, NONCOER, UNPRDIST) included in Models 5A and B5 were mostly nonsignificant. An exception is the 'progressive' cost distribution dummy (PROGR) which increased the probability of a significant income effect. Payment vehicle and cost distribution variables were not simultaneously entered in the model because of natural correlations between the two sets of variables. Variables for 'journal' and for 'year of publication' were also included in preliminary models but, as expected, were not found to be significant.

It is common practice in meta-analysis to include multiple observations from single studies in datasets. However, the Models B1–B5 in Table 5 should be regarded as superior to A1–A5 because, as explained earlier, sample B includes fewer multiple observations which are therefore not truly independent. Nevertheless, the B-regressions explained an overall larger portion of the variation in the dependent variable than the A-regressions, which can perhaps be explained by the more homogeneous quality of the observations.

5. Discussion

Two main results of meta-analysis can be identified. First, there was a surprisingly large proportion of CV studies that do not report a significant income effect. This was the case also when only those studies were considered in which effects of a covariable for respondent income is explicitly reported. Second, the present study found characteristics of the survey protocol to be significant predictors of survey response patterns. When the effects of question format (closed-ended *vs.* open-ended) and sample size were controlled for, the specifications in the survey protocol concerning the institutional context of public good provision and the precision of information on cost distribution appeared to affect the way how respondents answer contingent valuation questions. These effects of survey protocol were stronger than another economically meaningful effect, that of the type of public good concerned. However, the payment vehicle and, related to this, the distribution of costs among different incomes did not appear to affect the income effect in consistent ways. This result is surprising considering the importance of distributional issues in actual political decision processes. Although tax vehicles were more frequently associated with positive income effects it is difficult to say if this is a valid effect of the vehicle. Possibly, tax vehicles were simply a prefered vehicle for the valuation of goods that were known in advance to be highly elastic. The same reservation concerns the interpretation of the effect of progessive payment vehicles, since the dummy variables for progessive vehicles and for tax vehicles were highly correlated (sample A correlation coefficient = 0.60).

The use of a policy referendum context or of actually existing institutions in the public good provision scenario, the latter combined with a clear definition of the cost distribution, tended to reduce the probability that a significant income effect was observed. In other words, studies with characteristics of the survey protocol commonly perceived as 'desirable', which would be expected to produce a "truer" picture of preferences (Cummings and Taylor 1998), led to economically less plausible results. How can this pattern be accounted for? A tentative explanation might be based on the idea that, as suggested by Schwarz (1997) and reported by Champ et al. (2002), a substantial portion of respondents to dichotomous choice questions may not believe that the presented bid amounts are the amounts they would actually pay if the proposed policy was implemented. This could be particularly true when the scenario and cost distribution is otherwise well defined, which would allow respondent to come up with a rough expectation of their actual costs. As Flores and Strong (2003) suggest, such expectations might induce individuals to effectively respond to different questions (questions with different bid levels) than those posed by the researcher. High incomes, given their generally higher financial contribution to public goods, may then respond to upward-adjusted bids and low

incomes to downward-adjusted bids. This would quite naturally explain the observed pattern of reduced income effect when survey protocols use specific and realistic provision scenarios. 'Income-dependent bid adjustment' in CV surveys would moreover provide an explanation for the gap between income elasticities of public goods obtained in contingent valuation and collective choice approaches. To further test this hypothesis of income-dependent bid adjustment in future studies one could attempt to link bid credibility as measured by Champ et al. to respondent characteristics.

6. Conclusion

The patterns identified in the present study suggest that absence of statistically significant income effects in CV estimates of WTP for public goods might be an artifact of a combination of the dichotomous choice (DC) referendum format with well-specified, realistic survey scenarios. Although, the specificity and realism of the survey scenario as stipulated by Arrow et al. in the NOAA report is clearly important for informed decision-making, it could ultimately turn out to be problematic due to an inherent conflict with the random bid levels used in the DC format. This tentative conclusion from the present study, if it can be confirmed by further work, would have important implications for contingent valuation research and practice. The finding would advocate the use of the DC referendum format mainly as an instrument to assess WTP for goods with actual costs largely unknown to respondents. Once specific policy options and means of provision are identified, however, researchers and policy-makers might gain more by directly confronting citizens with propositions that include actual and credible, instead of hypothetical, cost information. Specific tests of the 'bid adjustment' hypothesis of Flores and Strong are now needed for more definitive conclusions than those we can draw based on the present analysis.

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Table 1. Frequencies of basic study characteristics and reported income effects

	Frequency	Percent[a]
Year of publication		
1998	14	16.9
1999	15	18.1
2000	20	24.1
2001	19	22.9
2002	15	18.1
Journal		
AJAE	3	3.6
EE	23	27.7
ERE	31	37.3
JEEM	7	8.4
LE	19	22.9
Survey type		
CV or paired comparison	72	86.7
CM	11	13.3
Type of good		
habitats, biodiversity	41	49.4
use values	42	50.6
Reported income effect		
income variable included/reported	47	56.6
income effect significant	30	36.1
no income variable included/reported	36	43.4

[a] Percentage of the 83 observations

Table 2. Classification and frequencies of characteristics of the survey protocol

	Frequency	Percent[a]
Question format		
closed-ended (CE)	70	85.5
CE multiple-bounded	7	8.4
CM	11	13.3
open-ended	12	14.5
'Implementation rule'		
hypothetical policy referendum	12	14.5
other or no rule	71	85.5
Payment vehicle		
charge	16	19.3
voluntary contribution	18	21.7
provision point mechanism	6	7.2
not specified	16	19.3
tax	27	32.5
various vehicles	6	7.2
Cost distribution schedule		
equal	16	8.4
equal to proportional	18	14.5
progressive	16	16.9
none (voluntary contribution)	16	24.1
unprecisely specified	27	14.5
not specified	6	21.7
Cost distribution definition		
coercive and well-defined	27	32.5
non-coercive or not well-defined	56	67.5
Providing institutions		
actually existing	33	39.8
hypothetical	26	31.3
not specified	24	28.9

[a] Percentage of the 83 observations

Table 3. Variables used in Logit regressions[a]

SSIZE	Sample size (see definition in the text)
CE	= 1 if closed-ended format; 0 otherwise
MBOUND	= 1 if double of multiple-bounded CE format; 0 otherwise
REF	= 1 if question formulated as a hypothetical policy referendum; 0 otherwise
WDDIST	= 1 if cost distribution is well defined; 0 otherwise (see definition in the text)
ACTINST	= 1 if actually existing institutions are envisaged to provide the good; 0 otherwise (see definition in the text)
PASSIVE	= 1 if the good is species or habitat protection (mainly passive use); 0 if the good is physical environmental quality for humans or recreation
CHARGE	= 1 if the payment vehicle is a user charge; 0 otherwise
VOLUNT	= 1 if the payment vehicle is a voluntary contribution (mechanism); 0 otherwise
PPMECH	=1 if the payment vehicle is a voluntary contribution with provision point mechanism; 0 otherwise
TAX	=1 if the payment vehicle is a tax; 0 otherwise
VARVEH	=1 if a multiple payment vehicle is specified (e.g. taxes *and* higher prices); 0 otherwise
EQUAL	=1 if the costs are distributed equally across incomes; 0 otherwise
EQPROP	=1 if the cost distribution depends on consumption (mainly charges, see text); 0 otherwise
PROGR	= 1 if the cost distribution is 'progressive', as in progressive income taxes; 0 otherwise
NONCOER	= 1 if no cost distribution is specified, as in voluntary contributions; 0 otherwise
UNPRDIST	= 1 if the cost distribution is specified but unprecise; 0 otherwise

a See the Text for detailed definitions.

Table 4. Logit coefficients for variables explaining presence/absence of an income effect (sample A)

	Model				
	(A1)	(A2)	(A3)	(A4)	(A5)
Constant	0.4062	0.4346	0.5833	0.6884	1.1396
	(0.7594)	(0.7602)	(0.8624)	(1.0740)	(1.0536)
SSIZE	0.0012 *	0.0012 (*)	0.0018 *	0.0019 *	0.0021 **
	(0.0007)	(0.0008)	(0.0010)	(0.0010)	(0.0011)
CE	-1.6823 **	-1.7312 **	-1.2711 *	-1.4494 *	-1.7546 **
	(0.7416)	(0.7473)	(0.7654)	(0.8347)	(0.8596)
CE*MBOUND	—	0.4910	—	—	—
		(0.9103)			
REF	—	—	-1.7465 **	-2.1727 **	-2.7693 **
			(0.8896)	(0.9688)	(1.1986)
WDDIST*ACTINST	—	—	-1.4069 (*)	-1.7639 *	-3.2642 **
			(0.8837)	(0.9822)	(1.3446)
PASSIVE	—	—	-0.5159	-0.9175	-1.3088 *
			(0.5688)	(0.7081)	(0.7703)
CHARGE	—	—	—	0.4343	—
				(1.0132)	
VOLUNT	—	—	—	-0.4212	—
				(0.9894)	
VOLUNT*PPMECH	—	—	—	0.0864	0.2435
				(1.1521)	(1.1848)
TAX	—	—	—	0.8372	—
				(1.0558)	
VARVEH	—	—	—	1.9938	—
				(1.6566)	
EQUAL	—	—	—	—	0.6943
					(1.6135)
EQPROP	—	—	—	—	0.9117
					(1.3981)
PROGR	—	—	—	—	3.2528 **
					(1.4746)
NONCOER	—	—	—	—	-0.6708
					(0.9472)
UNPRDIST	—	—	—	—	-0.1116
					(1.1115)
N	68	68	68	68	68
LogL unrestr.	-41.89	-41.75	-37.11	-35.36	-31.79
LogL restr.	-46.07	-46.07	-46.07	-46.07	-46.07
χ^2	8.35	8.64	17.92	21.42	28.56
Significance level	0.0153	0.0345	0.0030	0.0184	0.0027

Notes: Standard errors indicated in parentheses.
***= significant at $p<0.01$, **= significant at $p<0.05$, *= significant at $p<0.1$, (*)= significant at $p<0.15$.

Table 5. Logit coefficients for variables explaining presence/absence of an income effect (sample B)

	Model				
	(B1)	(B2)	(B3)	(B4)	(B5)
Constant	0.5497	0.5713	0.2945	0.2799	0.4030
	(0.8785)	(0.8788)	(1.0616)	(1.2834)	(1.3068)
SSIZE	0.0018 **	0.0017 *	0.0031 **	0.0038 **	0.0041 **
	(0.0009)	(0.0009)	(0.0016)	(0.0019)	(0.0021)
CE	-2.0905 **	-2.1307 **	-1.5252 *	-1.9886 *	-2.2012 **
	(0.8854)	(0.8908)	(0.9172)	(1.0568)	(1.0832)
CE*MBOUND	—	0.3884	—	—	—
		(0.9590)			
REF	—	—	-2.3381 **	-3.5477 **	-3.5913 **
			(1.0654)	(1.3986)	(1.5578)
WDDIST*ACTINST	—	—	-1.6683 *	-2.7679 **	-3.3724 **
			(0.9617)	(1.2308)	(1.6691)
PASSIVE	—	—	-0.1851	-0.9345	-1.0927
			(0.6783)	(0.8634)	(0.8690)
CHARGE	—	—	—	1.1228	—
				(1.4009)	
VOLUNT	—	—	—	-0.3721	—
				(1.1622)	
VOLUNT*PPMECH	—	—	—	0.1790	0.2504
				(1.2749)	(1.2965)
TAX	—	—	—	2.2919 (*)	—
				(1.5127)	
VARVEH	—	—	—	3.0339	—
				(2.1868)	
EQUAL	—	—	—	—	1.0745
					(2.2170)
EQPROP	—	—	—	—	1.2339
					(1.5609)
PROGR	—	—	—	—	3.4122 *
					(1.8965)
NONCOER	—	—	—	—	-0.4231
					(1.1535)
UNPRDIST	—	—	—	—	2.4006 (*)
					(1.6222)
N	55	55	55	55	55
LogL unrestricted	-32.78	-32.70	-27.13	-24.02	-23.07
LogL restricted	-37.68	-37.68	-37.68	-37.68	-37.68
χ^2	9.7962	9.9585	21.0951	27.3154	29.2137
Significance level	0.0075	0.0189	0.0008	0.0023	0.0021

Notes: Standard errors indicated in parentheses.
***= significant at $p<0.01$, **= significant at $p<0.05$, *= significant at $p<0.1$, (*)= significant at $p<0.15$.