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Measuring Hypothetical Bias in Choice Experiments: The Importance of Cognitive Consistency*

Olof Johansson-Stenman and Henrik Svedsäter

Abstract

A choice experiment eliciting environmental values with both real and hypothetical trade-offs is set up in order to test for hypothetical bias. A larger hypothetical bias was found in a between-subject than in a within-subject design, using otherwise identical scenarios, which can explain previous diverging results in the literature. We argue that people strive for consistency between their attitudes and behaviors, leading them to act in ways that correspond with their prior hypothetical statements. People hence seem to prefer to do what they say they would do, although this may not always reflect their true preferences regarding the good being valued.

KEYWORDS: stated-preference methods, choice experiment, hypothetical bias, internal consistency, non-market valuation

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1. Introduction

The validity of stated-preference (SP) methods for valuing non-market goods is of vital importance for public policy purposes, not least in the US where all major new regulations have to be preceded by a cost-benefit analysis. An obvious validity test of SP methods is to compare hypothetical statements with people's real willingness-to-pay (WTP). Empirical evidence of the contingent valuation method suggests a non-negligible hypothetical bias in that people's *stated* WTPs often exceed their *real-money* WTPs; see meta-analyses by List and Gallet (2001) and Murphy et al. (2005).

It has been argued that so-called choice experiments (CEs), where people make repeated choices between hypothetical bundles of goods, may be a more promising SP method (e.g. List et al., 2006). Previous results are mixed, however. Carlsson and Martinsson (2001) did not find any hypothetical bias in a within-subject CE design. On the other hand, using a between-subject design, Cameron et al. (2002) found that the mean hypothetical WTPs were substantially larger than the real-money WTP, although common underlying preferences could not be rejected due to large error terms. Lusk and Schroeder (2003) and List et al. (2006) also found a hypothetical bias in a between-subject test, although the bias disappeared once a so-called cheap-talk script was introduced in the latter study.

The present note is, as far as the authors know, the first to employ both within- and between-subject designs in the same CE. Drawing on an extensive body of research in psychology we hypothesize that people strive for consistency in their statements and behavior; see Nisbett and Ross (1991) for an overview of empirical studies. Much of the recent literature on cognitive dissonance (Festinger, 1957) has, following Aronson (1969), drawn attention to potential conflicts between people's actions and their self-perceptions. For example, people want to see themselves as rational and thoughtful (Cialdini and De Nicholas, 1989), as well as honest and trustworthy (Tedeschi et al., 1971), which preclude them from holding inconsistent beliefs.[1] Yariv (2005) recently proposed a utility function comprising both instrumental utility and belief-utility, the latter reflecting the direct utility from a sequence of beliefs or behaviors adopted by the agent. Following this description, an individual continuously faces a trade-off between making optimal choices (maximizing instrumental utility) and being or appearing consistent over time and across situations (maximizing belief-utility).

Consequently, a straightforward hypothesis is that real-money WTP is influenced by previously expressed hypothetical WTP. This is also consistent with available meta-analyses of contingent valuation studies: Murphy et al. (2005)

[1]See Elliot and Devine (1994) and Harmon-Jones and Mills (1999) for other contributions on cognitive dissonance, and Santos-Pinto and Sobel (2005), and Benabou and Tirole (2006) for recent economics contributions.

found much lower hypothetical bias in studies relying on within-subject tests than in studies making comparisons between subjects. List and Gallet (2001) found similar but smaller effects in the same direction. The experiment below aims to directly test this hypothesis using both within- and between-sample designs.

2. Experimental Design

The experiment, conducted among students enrolled in a variety of courses at the London School of Economics, was first advertised during class lectures. Interested students were subsequently contacted via e-mail regarding date and time for the experiment. Seventy (mainly graduate) students volunteered and were randomly divided into two groups of equal size. The subjects in the first group made hypothetical choices followed by choices that involved real payments; subjects in the other group made real-money choices directly. As can be seen in Table 1, the groups were similar with respect to registered characteristics; none of the differences are significant at conventional levels.

Table 1. Descriptive statistics of the two sub-samples.

	Sample with a hypothetical followed by a real-money CE (n=34)				Sample with a real-money-directly CE (n=35)			
	Mean	Std	Min	Max	Mean	Std	Min	Max
Female	0.618	0.486	0	1	0.571	0.495	0	1
Self-reported disposable income in £ per month	744.0	568.5	100	3000	607.7	316.3	150	2000
Age in years	24.71	2.59	20	32	23.91	3.40	18	33
Member of an environm. organization	0.118	0.323	0	1	0.143	0.350	0	1
Yearly contributions to environmental organizations in £/year	10.59	26.39	0	120	7.43	20.91	0	1
Have own children	0	0	0	0	0.029	0.167	0	1

The subjects faced 15 choice-sets that were identical in all three treatments (hypothetical, real-after-hypothetical, and real-directly). An extra choice-set was added in order to test for individual inconsistency within the same valuation treatment but was not included in the main analysis. The design largely follows Carlsson and Martinsson (2001), with the important difference that we used both within- and between-sample designs. The experiment was conducted in six sessions with 10-15 subjects in each session. The sessions involving subjects who made both hypothetical and real choices were held first.

Each session started with questions about age, income, and gender to ensure that the sub-samples were fairly similar in terms of characteristics that could

affect behavior. The subjects then received verbal and written instructions about the choice experiment and the nature and purpose of the environmental projects in question: two campaigns run by the World Wildlife Fund (WWF). In the hypothetical setting, the instructions additionally read (see the supplemental file at the journal web-page for exact instructions):

The choices are hypothetical but it is still very important that you answer them truthfully and as if they involved real money. There are altogether 16 choices for you to make. Try to consider each of them in isolation as if that was the only choice you have to make.

The subjects did not know that they were to face real-money choices immediately after completing the hypothetical ones. As an introduction to the "real" experiment, the subjects were given the following information:

In the following you will be presented similar choices as before, although now your choices will in fact determine how much money you earn in this experiment, as well as how much money is contributed to the campaigns. It thus involves real money. The procedure is the following:

- *you will again make 16 pairwise choices*
- *afterwards one of these will be drawn randomly as the 'actual' choice set*
- *you will be paid the amount of money according to the alternative chosen in this particular choice set, whereas the corresponding contribution is paid anonymously by us to the WWF*

Thus, your choices will determine how much money you earn in this experiment, and how much money that is contributed to the campaigns.

Essentially the same information was presented to the subjects faced with "real" choices directly. An example of a choice-set is presented below.

Table 2. Example of a choice-set.

Choice number 3	Alternative A	Alternative B
Money given to you	5	10
Contribution to campaign	14	7
Campaign	Elephant	Sea Turtle

Each of the two alternatives was thus characterized by three attributes: four payment levels (£0, £5, £10, £15), four donation levels (£0, £7, £14, £21), and two campaigns: the African Elephant and the Green Sea Turtle.

We determined the attribute levels based on pre-tests with 15 students at two different occasions, where we initially made use of the so-called *d*-optimal design (Zwerina et al., 1996). We varied payments and donations in order to pick levels that appropriately discriminated between individuals and scenarios, for instance avoiding choice sets that on the whole lead to lexicographic preferences for many students. The "real" choices that followed the hypothetical ones were presented in a different order to prevent respondents from simply replicating their

previous answers. The order of the choice-sets was reversed for half of the subjects in each scenario, but no significant order effects were found. Once the experiment was completed, all subjects left the classroom. They were called back individually, and a draw was made to decide which one of the 16 choice sets would determine how much the subject would earn, how much would be donated, and to which campaign.

3. The Random Utility Model and Results

Consider a random utility framework consisting of a systematic part V and a random unobservable part ε. The utility for individual i from choosing Alternative 1 is given by

$$u_{i1} = V_{i1} + \varepsilon_{i1}. \tag{1}$$

The probability that individual i chooses Alternative 1 is

$$\Pr(A_{\mathrm{i}} = 1) = \Pr(V_{\mathrm{i1}} + \varepsilon_{\mathrm{i1}} > V_{\mathrm{i2}} + \varepsilon_{\mathrm{i2}}) = \Pr(\varepsilon_{\mathrm{i1}} - \varepsilon_{\mathrm{i2}} > V_{\mathrm{i2}} - V_{\mathrm{i1}}), \tag{2}$$

where the differences between the error terms are assumed to be logistically distributed, and V is assumed to be linear in the interval considered. Of the variables in Table 1, we chose not to include whether the respondents had children, respondent age, and self-reported disposable income, primarily since there is little variation in these variables in the our student sample.[2] We then have:

$$\begin{aligned} V_{\mathrm{i}} = {} & \alpha + \beta^{x} x + \beta^{E} D^{E} + \beta^{T} D^{T} + \beta^{C} C_{i}(D^{E} + D^{T}) + \left(\beta^{F} z_{i}^{F} + \beta^{M} z_{i}^{M}\right)(D^{E} + D^{T}) \\ & + \left(\beta^{RaH} z_{i}^{RaH} + \beta^{H} z_{i}^{H}\right)(D^{E} + D^{T}) \end{aligned}, \tag{3}$$

where x is money paid to the individual, D^{E} and D^{T} are the donations given to the Elephant- and Turtle-projects, respectively, and z_i^F and z_i^M are dummy-variables reflecting whether a respondent is female and/or is a member of an environmental organization. z_i^{RaH} and z_i^H are dummy variables reflecting the *real-after-hypothetical* and the *hypothetical* treatment, respectively; C_i reflects the self-reported yearly contributions otherwise made to environmental organizations in 100£/year. It is straightforward to rewrite (3) as:

$$\begin{aligned} V_{\mathrm{i}} = {} & \alpha + \beta^{x} x + \beta^{E}\left(D^{E} + D^{T}\right) + \left(\beta^{T} - \beta^{E}\right) D^{T} + \beta^{C} Cont_{i}(D^{E} + D^{T}) \\ & + \left(\beta^{F} z_{i}^{F} + \beta^{M} z_{i}^{M}\right)(D^{E} + D^{T}) + \left(\beta^{RaH} z_{i}^{RaH} + \beta^{H} z_{i}^{H}\right)(D^{E} + D^{T}) \end{aligned}. \tag{4}$$

[2] Moreover, present income is probably a poor measure of their wealth. It is also theoretically incorrect to include income as an interaction-effect in a linear random-utility model. A non-linear model would make the interpretations less straightforward.

The parameters associated with this model (except for the intercept that cancels out) can be estimated with a conditional logit model (e.g. Louviere et al., 2000). Table 3 below presents the parameter estimates of a pooled sample and of each of the three subsamples.

Table 3. Parameters from conditional logit models (t-values in parentheses).

	Hypothetical	Real-after-hyp.	Real directly	Pooled sample	Pooled Heterosc.
Money to themselves, β^x	0.156***	0.161***	0.157***	0.157***	0.173***
	(7.40)	(7.79)	(7.57)	(13.12)	(9.80)
Money to donation, β^E	0.098***	0.094***	0.026*	0.039***	0.042***
	(5.16)	(5.06)	(1.65)	(3.43)	(3.37)
Turtle-donation, β^T	-0.010	-0.003	-0.014*	-0.009*	-0.010*
	(-1.10)	(-0.33)	(-1.66)	(-1.80)	(-1.93)
Female, β^F	0.102***	0.048***	0.107***	0.086***	0.097***
	(4.85)	(2.59)	(6.23)	(8.06)	(7.30)
Member of env. org., β^M	0.117***	0.127***	0.061	0.118***	0.132***
	(2.81)	(3.27)	(1.23)	(5.13)	(4.89)
Contr. to env. org., β^C	-0.068*	-0.099**	0.046	-0.066***	-0.078**
	(-1.76)	(-2.55)	(0.57)	(-2.58)	(-2.30)
Hypothetical sample, β^H				0.067***	0.080***
				(5.18)	(4.10)
Real-after- hypothetical				0.026**	0.035**
Sample, β^{RaH}				(2.13)	(2.17)
Heterosced. parameters:					0.010
Hypothetical sample					(0.96)
Real-after-Hypothetical					0.016
sample					(1.41)
Statistical observations	510	510	525	1545	1545
N	34	34	35	69	69

Note: ***, ** and * denote significance at the 0.01 level, 0.05 level and 0.1 level, respectively.

The results are consistent with our hypotheses. In order to control for unequal variance among the three samples, we also estimate a heteroscedastic full information maximum-likelihood model, where the relative scale-factors and the utility-parameters are estimated simultaneously (see Louviere et al., 2000). We can clearly not reject equal variances among the subsamples, and the parameter-estimates are similar to those of the homoscedastic model.

However, we are not primarily interested in the logit-parameters but in the corresponding differences in marginal willingness-to-pay (*MWTP*) for an additional pound donated to the campaigns in the different treatments. The *MWTP* to the Elephant-Campaign is obtained from (5) as follows:

$$MWTP_i^E = \frac{\partial u_i / \partial D^E}{\partial u_i / \partial x_i} = \frac{\partial V_i / \partial D^E}{\partial V_i / \partial x_i} = \frac{\beta^E + \beta^C C_i + \beta^F z_i^F + \beta^M z_i^M + \beta^H z_i^H + \beta^{RaH} z_i^{RaH}}{\beta^x}. \quad (5)$$

The *MWTP*-difference between the two campaigns is given by

$$MWTP^T - MWTP^E = (\beta^T - \beta^E) / \beta^x. \quad (6)$$

Other relevant *MWTP*-differences can similarly be obtained from (4); the results are presented in Table 4, where the *t*-values are calculated using the delta method.

Table 4. MWTPs based on the models in Table 2 (t-values in parentheses).

	Hypothetical	Real-after-hypothetical	Real directly	Pooled sample	Pooled Heterosc.
Baseline $MWTP^E$	0.631***	0.582***	0.165*	0.251***	0.240***
	(6.31)	(6.18)	(1.85)	(3.77)	(3.85)
$MWTP^T$ -$MWTP^E$	-0.064	-0.018	-0.092*	-0.057*	-0.060*
	(-1.10)	(-0.33)	(-1.66)	(-1.79)	(-1.91)
$MWTP^F - MWTP^{Male}$	0.654***	0.296***	0.682***	0.547***	0.559***
	(4.58)	(2.54)	(5.35)	(7.49)	(7.46)
$MWTP^M - MWTP^{Non\text{-}member}$	0.753***	0.789***	0.391	0.752***	0.762***
	(2.74)	(3.17)	(1.22)	(4.99)	(5.03)
$\partial MWTP / \partial C$	-0.436*	-0.613**	0.297	-0.419**	-0.452**
	(-1.74)	(-2.48)	(0.57)	(-2.56)	(-2.35)
$MWTP^H - MWTP^R$				0.428***	0.462***
				(5.03)	(4.59)
$MWTP^{RaH} - MWTP^R$				0.165**	0.205**
				(2.12)	(2.29)
Evaluations at sample means:					
$MWTP^E$	1.077***	0.792***	0.633***	0.831***	0.849***
	(12.28)	(12.16)	(10.51)	(20.68)	(18.69)
$MWTP^T$	1.013***	0.774***	0.541***	0.773***	0.790***
	(8.87)	(7.96)	(5.70)	(13.30)	(12.91)

Note: ***, ** and * denote significance at the 0.01 level, 0.05 level and 0.1 level, respectively.

We can observe that the *MWTP*, at sample means, for both the Elephant-Campaign and the Turtle-campaign, is always statistically different from zero at the 0.01 level. Moreover and as expected due to the importance of maintaining cognitive consistency, it is largest in the *hypothetical* treatment (where it is indeed larger than one), followed by the *real-after-hypothetical* treatment, and hence lowest in the *real-directly* treatment. The *MWTP* for the Elephant-Campaign is 25% and 70% larger in the *real-after-hypothetical* and *hypothetical* treatments, respectively, than in the *real-directly* treatment. For the Turtle-Campaign, the corresponding *MWTPs* are 43% and 87% larger.

From the pooled regressions we have that the between-subject hypothetical bias, as measured by $MWTP^H - MWTP^R$, is statistically significant at the 0.01 level, whether corrected for potential heteroscedasticity or not. Second, it shows that the respondents who were first confronted with hypothetical questions were largely influenced by their answers to these. This influence is significant at the 0.05 level and arguably economically important according to the figures presented above. We finally ran a model in order to test whether the *MWTPs* differ between the *hypothetical* and the *real-after-hypothetical* treatments (not presented in Table 4), demonstrating a statistically significant difference at the 0.05 level. The results from likelihood-ratio tests regarding the sample-differences are similar. The finding that women have a higher *MWTP* across all treatments is consistent with some earlier experimental evidence (Eckel and Grossman, 1998).

4. Conclusion

As far as we know, this is the first study that has tested hypothetical bias in CEs using both within- and between-subject designs in otherwise identical setups. The results suggest a presence of a hypothetical bias, and that the size of this bias is underestimated when measured in within-subject tests, consistent with our hypotheses based on psychological insights. It appears that hypothetical statements in part work as internal commitment devices, which also corresponds with recent experimental evidence suggesting that people prefer not to lie (Gneezy, 2005). A practical implication is that hypothetical bias in CEs, and other SP methods, should preferably be tested using between-subject designs. At the same time it is important to further analyze how hypothetical bias is affected by the characteristics of the valued good, and how it varies across situations.

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