Chapter 10
The Endowment Effect in a Public Goods Experiment

Edward J. Lopez and William Robert Nelson Jr.

Abstract The endowment effect suggests that consumer preferences are reference-dependent; i.e., that the shapes of indifference curves depend on an agent's initial endowment and the direction of exchange offers. Hence, a person may value a good more highly once ownership is established, causing disparity between willingness to accept and willingness to pay value measures. In this paper we test for the endowment effect in a manner that does not rely on observing value disparities. We employ a one shot voluntary contribution mechanism (VCM) with treatments for account framing, duration effects, and the physical handling of the initial endowment. The treatments are designed to vary subjects' perceived ownership over their experiment endowments. Results generally fail to support reference-dependence in manners suggested by the endowment effect. Contribution rates are higher when initial endowments begin in subjects' private accounts compared to when originating in the shared public account. Contributions are no different when subjects hold their endowments for up to one week. And contributions are higher among subjects who physically handle cash compared to those indicating their decisions in writing.

10.1 Introduction

The endowment effect suggests that preference formation is reference-dependent; i.e., that loss aversion, status quo bias, or inertia can create some manipulation (shift, kink, rotation, etc.) of indifference curves about the point of initial endowment (Knetsch 1989; Tversky and Kahneman 1991; Kahneman et al. 1991; Morrison 1997; List 2004). If preferences depend on the reference state, one consequence may be that individuals exhibit disparities between willingness to accept (WTA) and willingness to pay (WTP) measures of value. Such value disparities have been the key observational medium for testing the endowment effect. Ongoing theoretical work on

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reference-dependence continues to unfold in terms of value disparities (Kőszegi and Rabin 2006). In surveys, lab experiments, and field tests, value disparities have been so widely reported that it now seems naive to argue that such apparent anomalies do not exist under certain conditions.[1]

Rather, the developing literature has centered on whether value disparities are anomalous or instead are borne of substitution effects consistent with conventional preferences. Hanemann (1991, 2003) predicted that value disparities should be smaller for goods that are more substitutable. In experiments, competitive market forces caused observed value disparities to diminish (Brookshire and Coursey 1987; Shogren et al. 1994; List 2003). In addition, experience and information also tended to mitigate value disparities (Coursey et al. 1987; Knetsch and Sinden 1987; Kahneman et al. 1990).[2] Beyond the impact of market discipline and experience, the question remains fairly open whether value disparities are due to endowment effects, low substitutability, or other.

Furthermore, there is a tendency to conflate "endowment effect" with value disparities.[3] This unfortunately obscures the broader nature of the endowment effect; it is a statement that preferences change when the reference state changes, of which only one potential consequence would be WTA-WTP disparities. The endowment effect and WTA-WTP disparity should not be treated synonymously. These reasons and others suggest the usefulness of testing for the endowment effect in a manner that does not rely on observing WTA-WTP disparities.

This study presents a one-shot public good (voluntary contribution mechanism) design that allowed the experiment simultaneously to: (1) eliminate market discipline; (2) eliminate market experience; (3) hold substitution effects constant; and (4) observe treatments that one would expect to elicit the endowment effect.[4] If preferences depend on the initial endowment in manners suggested by the value disparity literature, then the endowment effect is likely to emerge under the conditions of the one-shot voluntary contribution mechanism (VCM), because it eliminates both market discipline and experience. To control for substitution effects, we used a public good that is perfectly substitutable: cash. Therefore, our experimental environment created favorable circumstances for observing an endowment effect, while holding constant the leading alternative explanation for value disparities.

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[1] See Horowitz and McConnell (2002) for a survey of WTA-WTP studies, but note that results vary. Most of this type of evidence have come from laboratory experiments, but increasing amounts of field evidence continues to emerge. Macmillan et al. (1999), for example, compared donations to an actual charity under alternative contingent valuation procedures, and List (2003, 2004) observed bid and ask prices for sports memorabilia in actual markets.

[2] Plott and Zeiler (2005) found no evidence of a WTP-WTA gap after extensive subject education and practice with a modified Becker–DeGroot–Marschak mechanism.

[3] One anonymous reader characterized an early version of this paper as considering "whether the endowment effect–people seem to dislike giving something up more than they like getting it–exists in a VCM..."

[4] Some of the WTA-WTP auction experiments have controlled for substitution effects (magnitude of the *MRS*) as well as income effects (movement among alternative indifference curves). See, in particular, List (2004) and Shogren et al. (1994). The present design holds substitution constant and implicitly assumes negligible income effects.

Results from this experimental design do not support the thesis that preferences depend on initial endowments. In one set of treatments, the duration for which participants held a cash endowment before making their public good decisions failed to influence participants' allocations. In another set of treatments, participants contributed more to the public account when they were told their initial endowment originated in their private accounts. As we will argue, the direction of this difference was the opposite of what the endowment effect should impart. Furthermore, participants who physically held cash contributed more than those who did not. This effect is also in the opposite direction than the endowment effect would suggest, but the differences were not statistically significant.

In the next section, we discuss previous tests of the endowment effect and explain how the VCM can be applied. In Sect. 10.3 the experimental design and hypotheses are explained. Section 10.4 contains results and their discussion, and Sect. 10.5 concludes with extensions for future research.

10.2 The VCM and the Endowment Effect

The endowment effect has usually been studied in market auctions by comparing the extent to which agents' WTA exceeds their WTP. The early literature on this topic explained observed value disparities in terms of Thaler's endowment effect (Thaler 1980), which suggests that agents may value a good more highly when their property right is already established. Knetsch and Sinden (1984), for example, showed that WTA > WTP for lottery tickets and attributed the disparity to the endowment effect and loss aversion. Subsequent experiments-e.g., Coursey et al. (1987), Knetsch and Sinden (1987), Kahneman et al. (1990)-allowed for subject experience, yet also explained observed value disparities as endowment effects.[5] Knetsch (1989) presented similar experimental evidence and concluded that the endowment effect implies anomalous preference formation-the shapes of indifference curves depend on the agent's initial endowment and the direction of exchange offers.[6]

These researchers invoked the endowment effect explanation because received theory (Willig 1976; Randall and Stoll 1980) indicated that value disparities for private goods would depend on the magnitude of the income elasticity, which is negligible for magnitudes of typical experiment earnings. In contrast, Hanemann (1991) showed that the value disparity for quantity changes of public goods depends on both the income and the substitution effects. His solutions demonstrated that as the substitution effect becomes smaller (greater) the value disparity becomes greater (smaller), holding the income elasticity constant. With negligible income effects or

[5]The subsequent cited studies also achieved greater control by eliminating the need for subjects to calculate expected winnings, and differing attitudes toward risk, associated with lottery tickets.

[6]Going further, Kahneman et al. (1991) argued that the endowment effect can result in intersecting indifference curves.

perfect substitutability, there should be no value disparity (Hanemann's Proposition 3).[7] This implies that the substitution and endowment effects are alternative, though not necessarily mutually exclusive, explanations for value disparities (cf. Morrison (1997)).

Shogren et al. (1994) tested endowment versus substitution explanations for value disparities using multiple-trial, second-price, sealed-bid Vickrey (1961) auctions for two goods: one with close substitutes (candy bars) and one with few substitutes (sandwiches with decreased health risk). For the high-substitutable good they found that the value disparity diminished to negligible amounts, and converged to the approximate market price after approximately four trials. However, for the low-substitutable good, the value disparity persisted, even after many trials. Shogren et al. (1994) isolated the effects of different auction mechanisms (i.e., institutions) on measured value disparities by recreating the coffee mug experiments using a Vickrey auction instead of a random bid auction (Becker et al. 1964), which Kahneman et al. (1990) used. Shogren et al. (1994) found, contrary to Kahneman et al. (1990), that the value disparity diminished after the first of ten trials. The results were more consistent with the substitution effect than the endowment effect, which Shogren et al. explained by the Vickrey mechanism being more market-like than the Becker–DeGroot–Marschak.[8] List (2003) further demonstrated the market discipline and experience effects using an innovative field experiment.

Morrison (1997) suggested that the experimental design of Shogren et al. (1994) was insufficient for rejecting the endowment effect because the design required the endowment and substitution effects to work mutually exclusively. Following the logic of irreversible indifference curves (Knetsch 1989), Morrison graphically demonstrated how the value disparity can be larger for goods with fewer substitutes if the endowment effect is allowed to reinforce the substitution effect, such that the indifference curves pivot in a particular manner. In response, Shogren and Hayes (1997) noted that Morrison's pivots were seemingly arbitrary. By using different pivots, they showed that the value disparity can be of equal size for linear and convex indifference curves. Thus, *if observed value disparities change in magnitude while holding the*

[7] Hanemann (1991) reformulated the bounds on the neoclassical compensating and equivalent variations determined earlier by Willig (1976) and Randall and Stoll (1980). He reduced the difference between WTA and WTP to the ratio of the income elasticity of the public good to the elasticity of substitution between public and private goods. As we will argue, our experiment assumes negligible income effects and holds the substitution effect constant in treatments designed to elicit an endowment effect.

[8] See also Brookshire and Coursey (1987), Coursey et al. (1987), and List (2003, 2004). The effect of market discipline/experience appears to be sensitive to institutional design. There are many institution-specific explanations for observed value disparities. First the perceived illegitimacy of a transaction might cause the required (narrowly interpreted) surplus from the transaction to exceed epsilon, thereby driving a wedge between WTP and WTA (e.g., Rowe et al. (1980)). Second, buyers are often able to negotiate a lower price if they understate their WTA; if the associated rules of thumb are adopted, then equilibrium WTA exceeds WTP (Knez et al. 1985). In surveys and one-shot acutions, reported preferences might be misrepresentations/mistakes, but in repeated market interactions such mistakes tend to diminish in magnitude and frequency. Third, WTP and WTA might vary according to which elicitation mechanism is used (Shogren et al. 2001).

substitution effect constant, it would be due to the endowment effect even for goods that are perfect substitutes.

In short, the literature on value disparity currently offers the following stylized facts. (1) Value disparity is observed under certain elicitation conditions. (2) The endowment and substitution effects are alternative but not mutually exclusive explanations. (3) The disparity diminishes as agents gain market experience and as the experimental environment imposes more market discipline. (4) The endowment effect can be described as some manipulation (e.g., pivot, kink, rotation) of agents' indifference curves.

The VCM simultaneously addresses several aspects of testing for the endowment effect. First, a parameter in the VCM is the agent's marginal rate of substitution between proceeds from the private and public accounts. We introduce treatments that are designed to elicit an endowment effect-i.e., to change the *MRS* based on the subjects' perceived control over the initial endowment. With experimental control and a constant substitution effect, any difference in contribution levels between treatment groups would be due to the endowment effect. Thus, it can be inferred whether subjects' indifference curves pivot/kink/rotate sufficiently so as to alter their observed decisions. Second, proceeds from either the private or public account are cash-denominated. This feature allows substitution and endowment to work mutually inclusively, since the goods are perfect substitutes. Third, the design creates favorable conditions for the endowment effect to emerge because we eliminate both market discipline (by using a public good) and market experience (by allowing only one trial). Fourth, a public good experiment may afford a closer test of value disparity theory. Hanemann's advance was the result of considering the exchange offer as a change in the quantity of a public good. Yet, to our knowledge, no public good (VCM) experiments have been used to test for value disparities. One study (Brookshire and Coursey 1987) used a public good (trees in a neighborhood park, which have "a large degree of substitutability" [p. 555]), but its purpose was to compare the results of contingent valuation versus auction mechanisms. Furthermore, Cherry et al. (2005) provide recent evidence on VCM experiments that indicates contribution rates are sensitive to certain treatment effects on subjects' endowments. Thus, the VCM public good experiment presents the opportunity to test for endowment effect-style preference formation without the need to observe WTA-WTP disparities.

10.3 Experimental Design

10.3.1 The VCM

In the two-player VCM, each player $i = 1, 2, i \neq j$, is given an initial endowment of ω dollars to be invested in two accounts-one shared, one private. Define x^i and y^i as $i's$ dollar proceeds from the public and private accounts, respectively. Total dollar

payoffs to each player i equal the sum of x^i and y^i. The rational agent's objective in this environment is to maximize

$$u^i = u^i(\omega - c^i, g(\Sigma c^i)) \tag{10.1}$$

where $i's$ choice variable is c^i dollars contributed to the public account. In this experiment, private account payoffs are unweighted such that $y^i = \omega c^i$. To define payoffs from the public account and to characterize contribution incentives, differentiate equation 10.1 with respect to x^i and normalize by u^i_x to obtain

$$du^i = -1 + \frac{u^i_y}{u^i_x} g'. \tag{10.2}$$

Note that Eq. 10.1 contains the agent's marginal rate of substitution between the private and public goods. In a seminal study on VCM experiments, Isaac et al. (1984) defined the second term in Eq. 10.1 as the marginal per capita return (*MPCR*) from the public account. It is the product of the agent's $MRS_{x,y}$ (under a given payoff structure) and the marginal rate of transformation (as specified by experiment parameters). Proceeds from the public account depend on the technology of the experiment, g, which characterizes the *MRT*. The general form of the VCM public good production function is

$$g = \frac{a\Sigma c^i}{N}, \tag{10.3}$$

which, for this two-player experiment is

$$g = \frac{1.5(c^1 + c^2)}{2}, \tag{10.4}$$

such that the $MPCR = 0.75$. Proceeds from either account are denominated in dollars such that

$$\frac{u^i_y}{u^i_x} = MRS_{x,y} = 1. \tag{10.5}$$

The socially optimal contribution is $c^i = \omega$, but the Nash equilibrium is the strong free rider prediction $c^i = 0$. Previous experimental results under these types of conditions revealed contribution rates approximately 40 % of the optimal (Dawes and Thaler 1988; Ledyard 1995). Reference dependence in general, and the endowment effect in particular, suggests that $MRS_{x,y}$ will vary as subjects' perceived control over ω is varied under experimental control. If treatments successfully elicit the endowment effect, this will increase the disutility of c^i, the marginal dollar contributed to the public account. Hence, this will increase the $MRS_{x,y}$ such that the indifference curve

is rotated in the manner discussed above, which would decrease average contribution levels.

10.3.2 Treatment Designs

Within the above VCM environment, this study features two primary treatment effects-account framing and duration framing-which, according to the surveyed literature, are expected to elicit an endowment effect. A third effect is also presented, by which the endowment effect may arise if participants' physical handling of cash imparts an endowment effect. We discuss these three effects in turn.

First, in the account framing (AF) treatments, the originating account is varied. In one treatment, participants were told ω (the initial endowment) began in the shared public account; in the other treatment, participants were told ω began in their private account. The AF treatments may elicit an endowment effect if subjects perceive the originating account as an initial property right. When ω originates in the private account, participants may initially feel a greater sense of ownership than when the endowment begins in the public account. Accordingly, if the AF elicits an endowment effect, there is reason to expect average contributions to be lower in treatments where ω starts in the private account.[9]

Second, in the duration framing (DF) treatments, the length of time that participants held the endowment prior to making their allocation decision is varied (by up to one week). Research on the endowment effect provides several reasons to expect a duration effect. First, some scholars have speculated that the endowment effect may have a temporal component that it may take time to bind in some sense (e.g., Knetsch and Sinden (1984); Kahneman et al. (1991); Strahilevitz and Loewenstein (1998).[10] Second, individuals may be more readily willing to part with windfall gains than earned wealth (Thaler and Johnson 1990; Cherry et al. 2005). Third, current spending may increase by less following a temporary increase in income compared to a longer duration increase (Friedman 1957). Participants who make their experiment decision immediately after receiving the endowment may perceive the endowment as a windfall gain and play as if they are using the "house's money." Subjects who are

[9]In value disparity experiments, WTA assigns the agent rights to the good while WTP offers the opportunity to acquire the good. By varying the originating account, the VCM experiment may mimic this difference regarding the direction of the exchange offer.

[10]To our knowledge, Strahilevitz and Loewenstein (1998) is the closest to this study in testing for duration effects. The derive duration-effect hypotheses by combining a prospect theory value function with *adaptation*, a concept in psychology, which "in the context of object ownership, is the tendency for people to become psychologically accustomed to changes in their material situation" (Strahilevitz and Loewenstein 1998, p. 277). Duration treatments up to one hour have been introduced in a variety of experiments employing WTP and WTA elicitation questionnaires. Results indicated that subjects generally express greater WTP and WTA as the endowment is held for a longer duration. We are unaware of other experimental results on duration effects.

able to savor the increase in wealth for enough time may play as though the money is their own.

Third, in all the DF treatments, participants physically held cash as their endowment. In all AF treatments, participants submitted their decisions in writing without handling cash. Cash in hand may induce feelings of ownership more so than money in an account. Accordingly, if cash-in-hand elicits an endowment effect, there is reason to expect average contributions to be lower in treatments where participants handled cash.

10.3.3 Treatment Groups and Hypotheses

We conducted six treatment groups, two for AF and four for DF. Each treatment group consisted of two sessions, meeting simultaneously in separate rooms, for a total of 12 sessions. All AF sessions were run in a laboratory setting. For reasons that will become apparent, the DF sessions were run in both laboratory and classroom settings. Table 10.1 summarizes the six treatment groups. Details regarding logistics and the protocol are available from the authors.

As is apparent from Table 10.1, the six groups were organized as three pairs of treatments. Comparing average contribution levels between the treatment pairs provides the basis for the hypothesis tests. The null hypothesis is no identifiable treatment effect. The alternative hypothesis is provided by the direction of the anticipated endowment effect.

All AF sessions were conducted in a laboratory setting. No AF participants handled cash until after all decisions were made. Rather, in one AF treatment subjects were told the initial endowment ω originated in their own private account. In the other AF treatment, subjects were told ω began in the public account. As Table 10.1 shows, these groups are named *ALR* and *ALU*, respectively. Subjects then wrote

Table 10.1 Summary of treatment groups and hypotheses

	Account framing (Did not handle cash)			Duration framing (Handled cash)			
	Laboratory			Laboratory		Classroom	
	ω begins in			ω held for		ω held for	
	Private account	Public account		25 min	1 min	1 week	1 min
Treatment group name	*ALR*	*ALU*		*DLL*	*DLS*	*DCL*	*DCS*
Endowment effect hypotheses							
Account and duration framing	$\bar{c}_{ALR} < \bar{c}_{ALU}$			$\bar{c}_{DLL} < \bar{c}_{DLS}$		$\bar{c}_{DCL} < \bar{c}_{DCS}$	
Handling versus not handling cash			$\bar{c}_{AL} > \bar{c}_{DLS}$ or $\bar{c}_{AL} > \bar{c}_{DCS}$				

Notes The initial endowment is ω, and $\bar{c}$ is the mean or median contribution within a treatment group

down their allocation decisions and were paid accordingly at the end of the session. This enables the test of the following, where is the mean (or median) public account contribution within a group.

H1: Account framing imparts an endowment effect; *ALR* participants will contribute less to the public account than *ALU* participants. That is $\bar{c}_{ALR} < \bar{c}_{ALU}$.

In the DF treatments, this experiment had "Short" and "Long" groups, which were defined by the length of time that subjects held the endowment prior to making the allocation decision. Duration framing is easy to accomplish in the laboratory, but the length of the duration treatment is limited by how long participants can be asked to stay. We were cautious not to make our sessions too long so as to hinder subject recruitment. More importantly, because Short and Long participants were recruited simultaneously, having one session last longer could introduce a loss of experimental control. Therefore, in the laboratory treatments we varied the duration by only 25 min, the length of time required to complete the instructions. As shown in Table 10.1, we assigned the Short and Long laboratory groups the treatment names *DLS* and *DLL*, respectively.

The design problem was more challenging for observing the longer duration treatments. We considered scheduling participants for two laboratory sessions. In Session 1 we would explain the potential winnings and take care of paperwork, such as consent forms. We would give the cash to the Long group in Session 1 but not to the Short group. In Session 2, perhaps a week later, all participants would reconvene and play the VCM. According to the endowment effect, we would expect the Long participants to contribute less on average to the public account than the Short group. The obvious difficulty with this approach is that participants who attended Session 1 might fail to show up for Session 2. If those who do show up for Session 2 are more trustworthy than those who do not, this design would likely select cooperators. To minimize this risk and obtain results as free from selection bias as practicable, we decided to run the longer duration treatments under the structure of regularly meeting university classes. With instructor permission, we were allowed to visit four different classes during two consecutive weeks. In Week 1, we explained to students that they would have the opportunity to participate in an experiment that would take place in the same class one week later, and we took care of paperwork. For the Long treatment, we also distributed cash in Week 1 and asked students to bring an equal amount of cash with them to Week 2. For the Short treatment, we simply told students they could participate in an experiment, for monetary earnings, during the following week's class. Using this approach, students had the added incentive to show up for Week 2, reducing the likely extent of selection effects. As shown in Table 10.1, the endowment effect suggests the following.

H2: The length of time one possesses an item increases the strength of the endowment effect; Long group participants will contribute less than Short group participants. That is $\bar{c}_{DLL} < \bar{c}_{DLS}$ or $\bar{c}_{DCL} < \bar{c}_{DCS}$.

Finally, all DF participants handled cash when making their experiment decisions, while all AF participants wrote their decisions without handling cash. Comparing

only Short group DF participants with AF participants, it is possible to test the following.

H3: Cash-in-hand increases the strength of the endowment effect; Short group participants in the laboratory or classroom setting, who handled cash, will contribute less than AF participants, who did not handle cash. That is, $\bar{c}_{DLS} < \bar{c}_{AL}$ or $\bar{c}_{DCS} < \bar{c}_{AL}$.

10.4 Results

10.4.1 Overview

We present all results in Fig. 10.1, Tables 10.2, and 10.3. Through 12 experiment sessions a total of 284 undergraduate participants, from a wide range of majors at two large public universities, one in New York (NY) and the other in Texas (TX), each made one allocation decision. In the first set of sessions, 75 students from NY were divided among four laboratory treatments. The second set consisted of 80 students from TX, divided among the four classroom sessions. In the third set were 129 students, also from TX, divided among the remaining four laboratory sessions. All duration framing (DF) treatments took place in TX. In the combined account framing (AF) treatments, NY students contributed less than TX students (mean 4.12 versus 5.17, p = 0.07 according to a two tailed t test assuming unequal variances). In each of the pairs of AF treatments at each school, the directions were the same and the sizes

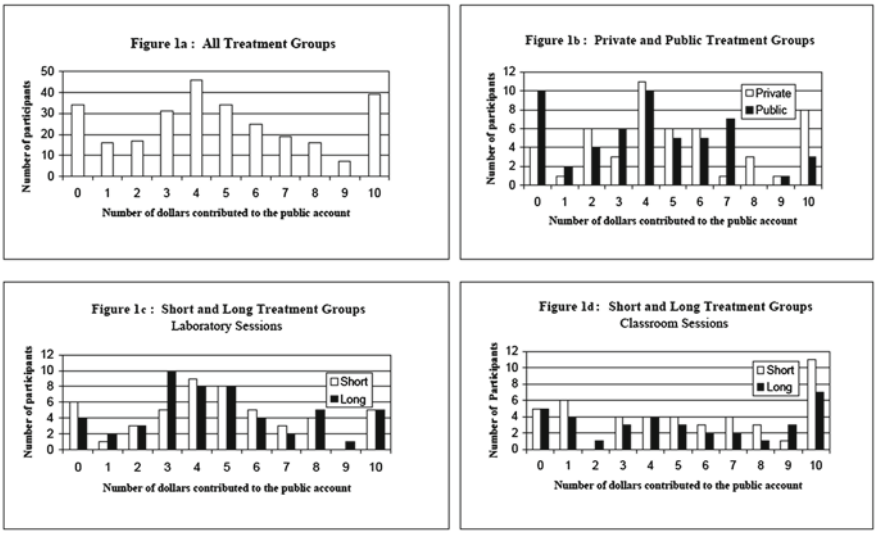

Fig. 10.1 Contribution frequencies by treatment groups

Table 10.2 Contribution descriptive statistics by treatment group

	Treatment groups divided by location								Six treatment groups						Combined groups		
	A_{LRY}	A_{LUY}	D_{LSY}	D_{LLY}	A_{LRT}	A_{LUT}	D_{LST}	D_{LLT}	A_{LR}	A_{LU}	D_{LS}	D_{LL}	D_{CS}	D_{CL}	AL	D_{short}	D_{long}
Mean	5.4	3.94	4.1	4.14	4.89	4	5.14	5.2	5.04	3.98	4.71	4.75	5.4	5.06	4.5	5.04	4.87
Standard error	0.7	0.69	0.68	0.59	0.54	0.48	0.5	0.51	0.43	0.39	0.41	0.39	0.54	0.62	0.29	0.33	0.34
Median	5	4	4	3.5	4	4	5	5	4.5	4	5	4	5	5	4	5	4
Mode	4	5	4	3	4	4	5	4	4	0	4	3	10	10	4	10	3
Standard deviation	2.69	2.94	3.06	2.78	3.17	2.84	2.71	2.78	3.02	2.85	2.87	2.81	3.6	3.66	2.96	3.24	3.16
Sample variance	7.26	8.64	9.36	7.74	10.05	8.06	7.34	7.75	9.1	8.1	8.25	7.88	12.97	13.41	8.78	10.51	9.99
Range	8	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Minimum	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Maximum	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
No. observations	15	18	20	22	35	35	29	30	50	53	49	52	45	35	103	94	87

Total number of subjects is 284. Group name key: First letter denotes kind of treatment: A = account framing and D = duration framing. Second letter denotes settings: C = classroom and L = lab. Third letter denotes specific treatment: L = long, S = short, R = private account and U = public account. Subscript denotes location: Y = New York and T = Texas

Table 10.3 t-Tests for account framing effect

	ALR		ALU	DLS		DLL	DCS		DCL	D		AL
Mean amount deposited into public account	5.04		3.98	4.71		4.75	5.4		5.06	4.96		4.5
Variance	9.1		8.1	8.25		7.88	12.97		13.41	10.22		8.78
Observations	50		53	49		52	45		35	181		103
Hypothesized mean difference		0			0			0			0	
df		100			98			73			226	
t Stat		1.83			−0.06			0.42			1.24	
P(T <= t) one-tail		0.04			0.47			0.34			0.11	
t-critical one-tail		1.66			1.66			1.67			1.65	
P(T <= t) two-tail		0.07			0.95			0.68			0.22	
t-critical two-tail		1.98			1.98			1.99			1.97	
Mann–Whitney two-tail P,(test of medians)		0.10			0.90			0.64			0.26	
Number of observations required for one-tailed t-test (p = 0.1 and a power of 0.80)	N-1: 71 N-2: 67			N-1: 46,040 N-2: 45,077			N-1: 1,022 N-2: 1,039			N-1: 419 N-2: 388		

Notes Two-Sample assuming unequal variances. Group Name Key. First letter denotes kind of treatment: *A* = account framing and *D* = duration framing. Second letter denotes setting: *C* = classroom and *L* = lab. Third letter denotes specific treatment: *L* = long, *S* = short, *R* = private account and *U* = public account

of the differences in means were similar. The mean contribution within treatment groups ranged from a low of 3.94 in the *ALU* treatment at NY to a high of 5.40 in the *DCS* treatment. The contributions overall appear normally distributed around four, but with spikes at 0 and 10 (Fig. 10.1a). All the laboratory sessions had similarly shaped trimodal distributions, but the distribution of the classroom appeared more uniform with a single mode at 10 (Fig. 10.1d) while not significantly increasing the mean in these groups. The mean contribution in the classroom treatments equaled 5.25, and the mean in the laboratory treatments equaled 4.61.

10.4.2 Tests of Hypotheses

H1 is weakly rejected. Participants contributed more when the money began in their private account than when the money began in their public account. This variation is the opposite of what the endowment effect would impart. As evident from Table 10.2 and Fig. 10.1d, over $1 more was contributed when ω began in the private account ($ALR = 5.04$) than when the money began in the public account ($ALU = 3.98$). However, the difference is significant at slightly less than conventional levels (two-tailed $\rho = 0.07$) according to a two-sample t-test assuming unequal variances.

H2 receives no support. The results give no indication that the duration for which one holds cash has an impact on an individual's contributions to the public good. In the laboratory, the long group contributed more, although the difference was negligible ($DLL = 4.75$ and $DLS = 4.71$) and not close to significant (two-tailed $\rho = 0.95$). In the classroom the difference was larger in magnitude, but also in the opposite direction than expected ($DCL = 5.06$ and $DCS = 5.40$, with two-tailed $\rho = 0.68$). According to a power test, over 45,000 laboratory observations would be required for the t-test to identify 0.10 significance with 0.80 probability. Similar power would be accomplished with just over 1,000 classroom observations.

H3 is rejected. Cash-in-hand participants contributed more ($DCS = 5.4$ and $DLS = 4.71$) than participants who indicated their contributions in writing ($AL = 4.5$).[11] This is the opposite of what the endowment effect would impart, though not statistically significant. The DLS mean was 0.21 higher (two-tailed $\rho = 0.38$), and DCS was 0.90 higher (two-tailed $\rho = 0.14$). Combining the *DLS* and *DCS* data, mean contributions were 0.46 higher (4.96, two-tailed $\rho = 0.22$).[12]

[11] Note that we compare only the Short groups from the duration treatments.

[12] In the AF treatments, the participants were told that the initial endowment originated in a particular account, but the DF instructions included no reference to the originating account.

10.4.3 Discussion

This experimental design created circumstances that value disparity experiments have shown to be favorable for eliciting an endowment effect. In particular, as discussed in Sect. 10.2 above, recent work has shown that value disparities diminish with market experience and discipline. By using a one-shot public good game, one would expect evidence of endowment effects to emerge in participants' contributions to the public account. On the contrary, the data from this experiment generally point to a negative result. The endowment effect is elusive in a cash-based, one-shot VCM, which fails to support the thesis that preferences are sufficiently reference-dependent so as to alter observed public good contributions. More specifically, the *DF* and cash-in-hand treatments fail to support earlier speculations regarding the "immediacy of the transaction" (Knetsch and Sinden 1984) or "gambling with the house's money" (Thaler and Johnson 1990).

Inherent features of the VCM or the use of cash in this experiment may be confounding matters. For example, the results from AF treatments-which were designed to frame the contribution decision as giving versus taking are consistent with the result from Andreoni (1995) that the warm glow effect in a positive frame is stronger than the cold prickle in a negative frame. Andreoni's experiment, and several that have followed, were linear public good games like the experiment in this study.[13] Thus, the AF results are consistent with the results obtained in the broader literature, regardless of the attempts to elicit reference-dependence through framing. Second, while the DF and cash-in-hand treatments do not support the "house money" effect, this may derive from there being little expectation of an endowment effect with money. Cash is more divisible than the goods used in earlier value disparity tests (coffee mugs, candy bars, sports cards, etc.). Moreover, as the numeraire good cash also differs in that subjects hold cash for future purchases, not for consumption per se (Kahneman et al. 1990, p. 1328). However, participants in a linear public good experiment are not sensitive to earned income either (Cherry et al. 2005). The VCM has well documented deviations from the Nash prediction, which can create control problems when attempting to elicit a deviation from consumer theory such as reference-dependence. We cannot rule out, for example, that the endowment effect in a VCM is of some statistical magnitude regardless of account/duration framing or handling cash.

Thus, while these results cannot point to the existence (or non-existence) of the endowment effect, the design of this experiment offers potentially fruitful new directions for testing reference-dependence. Traditional value disparity approaches have grappled recently with the relative merits of alternative elicitation procedures and their institutional attributes (e.g. Shogren and Hayes (1997); List (2004)), rather than

[13] Similarly, Andreoni (1995) also found that the warm glow is stronger than the cold prickle. In his experiment, contributions to the public good were greater when the game was explained in terms of a positive rather than a negative externality. In both his treatments, all money began in each individual's "Investment Account," and participants chose between depositing tokens in a "Private Exchange" and a "Public Exchange."

whether preferences are reference-dependent the central question of the endowment effect. Furthermore, too few value disparity experiments have investigated substitution between private and public goods, which would be a closer test of received consumer theory on WTA-WTP (Hanemann 1991, 2003). The VCM approach disentangles the endowment effect from value disparities and institutional differences within alternative auction mechanisms. In principle, reference-dependence generally, or the endowment effect more specifically, can be tested in a variety of experimental environments that offer subjects an initial endowment with which to play. Our results invite even stronger account, duration, and endowment-in-hand treatments with games other than the VCM and using a less divisible, more consumable initial endowment than cash.

10.5 Conclusion

According to neoclassical theory, when the public good is perfectly substitutable with at least one private good and income effects are negligible, there will be no disparity between willingness to accept (WTA) and willingness to pay (WTP) measures of value (Hanemann (1991), Proposition 3). In the presence of an endowment effect, however, individuals may consider a good in possession as less substitutable due to loss aversion or status quo bias (Kahneman et al. 1991). The voluntary contribution mechanism (VCM) provides a tool for inferring whether the (unobserved) marginal rate of substitution between a public and private good is sufficiently sensitive to subjects' perceived control over the initial endowment so as to alter their (observed) contributions to the public account. The one-shot, cash-denominated VCM creates favorable circumstances for the endowment effect to emerge because it eliminates market discipline and experience while holding the substitution effect constant. Within this environment, we designed treatments that framed the initial endowment in several different ways. According to the endowment effect, treatments in which subjects had greater perceived control over the initial endowment should have contributed less to the public account. The results of 284 subjects in 12 different treatment sessions are not consistent with this expected effect.

Finally, our approach touches on critiques of standard experimental methods. Suppose there is some temporal component to how subjects respond when given an initial endowment with which to participate in an experiment. Skeptics could argue, as we hinted earlier in the paper, that subjects' decisions are unreliable if the experiment decisions are made immediately or soon after receiving the endowment. We liken this to the criticism of using student subject pools to represent the behaviors of actual economic agents in relevant markets (Davis and Holt 1993, p. 17). A preponderance of experimental evidence comparing students with professionals indicates that this "subject surrogacy" critique does not seem to detract from standard methodology. Similarly, our results on duration framing do not suggest evidence of problems

associated with allowing subjects to make their experiment decisions soon after receiving the initial endowment. Alternative explanations for these results are possible, thus calling for further investigations.

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