THE ENDOWMENT EFFECT IN A PUBLIC GOOD EXPERIMENT*

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Abstract: Previous tests of the endowment effect have usually observed WTA-WTP disparities. Here, a public good experiment is employed. Both account framing and duration framing treatments are introduced to alter subjects' perceived control over an initial endowment. Results do not indicate that preferences shift in a way consistent with the endowment effect.

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THE ENDOWMENT EFFECT IN A PUBLIC GOOD EXPERIMENT

The endowment effect suggests that preference formation is reference-dependent; i.e., that loss aversion or status quo bias can create some manipulation (shift, kink, rotation, etc.) of indifference curves about the point of initial endowment [Knetsch 1989; Tversky and Kahnemann 1991; Kahnemann et al. 1991; Morrison 1997; List 2004b]. If preferences depend on the initial endowment, 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 a key observational medium for testing the endowment effect. In surveys, lab experiments, and field tests, value disparities have been so widely reported that it now seems naïve to argue that such apparent anomalies do not exist under certain conditions.[1] Rather, much of the developing literature has centered on whether these disparities are anomalous or instead if they are, to some extent, borne of substitution effects consistent with conventional preferences. The theoretical motivation was Hanemann's [1991, 2003] prediction that value disparities should be smaller for goods that are more substitutable.[2] 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].[3] Beyond the diminishing impact of market discipline and experience, the question remains fairly open

[1] See Horowitz and McConnell for a survey of WTA-WTP studies, but note that results vary. Most of this type of evidence has 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, 2004b] observed bid and ask prices for sports memorabilia in actual markets.

[2] Shogren et al. [1994, 1997] present experimental results, within a Vickrey auction environment, which are consistent with this prediction.

[3] Plott and Zeiler [2003] found no evidence of a WTP-WTA gap after extensive subject education and practice with a modified Becker-DeGroot-Marscak mechanism.

whether value disparities are due to endowment or substitution effects. This suggests the usefulness of testing for the endowment effect in a manner that does not rely on observing WTA-WTP disparities.

We present a one-shot public good (voluntary contribution mechanism [VCM]) 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, the endowment effect is most likely to emerge under the conditions of the one-shot 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.

Results from this experiment 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 (less) to the public account when the endowment was said to start in participants' private (public) accounts. The direction of this difference was the opposite of what the endowment effect should impart.

In the next section, we discuss previous tests of the endowment effect and explain how the VCM can be applied. In Section 2 the experimental design and hypotheses are explained. Section 3 contains the results, and Section 4 concludes.

[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 [2004a] and Shogren et al. [1994]. The present design holds substitution constant and implicitly assumes negligible income effects.

I. The VCM and the Endowment Effect

The endowment effect usually has 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 [1980] endowment effect, 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 perfect substitutability, there should be no value disparity [Hanemann's Proposition 3].[7] This implies that the substitution and endowment

[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, Kahnemann et al. [1991] argued that the endowment effect can result in intersecting indifference curves.

[7] Hanemann 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.

effects are alternative, though not necessarily mutually inclusive, 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) 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 (Becker-DeGroot-Marshack) auction, which Kahneman et al. [1990] used. Shogren et al. found, contrary to Kahneman et al., 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 BDM.[8] List [2003] further demonstrated the market experience effect using an innovative field experiment.

Morrison [1997] suggested that Shogren et al.'s [1994] experimental design 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

[8] See also Brookshire and Coursey [1987], Coursey et al. [1987], and List [2003, 2004a]. 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 often are able to negotiate a lower price if they understate their WTP and sellers a higher price if they overstate their WTA; if the associated rules of thumb are adopted, then equilibrium WTA exceeds WTP [Knez et al. 1985]. In surveys and one-shot auctions, 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 value elicitation mechanism is used [Shogren et al. 2001].

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 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 explanations. (3) The disparity diminishes as agents gain market experience and as the experimental environment imposes more market discipline. (4) The endowment effect, if it exists, 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, we fill a gap left by traditional approaches to testing the endowment effect. 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.[9] 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. The VCM public good experiment presents the opportunity to test for endowment effect-style preference formation without the need to observe WTA-WTP disparities.

II. Experimental Design

A. 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

$$(1) \qquad u^i = u^i\left(\omega - c^i, g(\Sigma c^i)\right)$$

where i's choice variable is c^i dollars contributed to the public account. In our 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 (1) to with respect to x^i and normalize by u_x^i to obtain

$$(2) \qquad du^i = -1 + \frac{u_y^i}{u_x^i} g'.$$

Note that (2) 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 (2) as the marginal per capita return (*MPCR*) from the public account. It is the product of the agent's $MRS_{x,y}$

[9] Many WTA-WTP studies have been conducted using public goods. See Horowitz and McConnel [2002] for a review.

(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 $g = \frac{a\Sigma c^i}{N}$, which, for our two-player experiment is $g = \frac{1.5(c^1 + c^2)}{2}$. Proceeds from either account are denominated in dollars such that $\frac{u_y^i}{u_x^i} = MRS_{x,y} = 1$. Note that with the parameter $a = 1.5$ the $MPCR = (1)(1.5/2) = 0.75$. The socially optimal contribution is $c^i = \omega$, but the Nash equilibrium is the strong free rider prediction $c^i = 0$. Previous experiment results under these types of conditions revealed a contribution rate approximately 40 per cent of the optimal [Dawes and Thaler 1988; Ledyard 1995]. The endowment effect suggests that $MRS_{x,y}$ will vary as subjects' perceived control over ω is varied under experimental control. We test whether this, in turn, affects subjects' observed contributions.

B. Framing and Duration Effects

We attempted to elicit the endowment effect using two kinds of treatment effects: account framing and duration framing. In the account framing (AF) treatments, we varied the account in which participants were told the initial endowment was placed. In one treatment, subjects were told ω began in the shared public account; in the other treatment, ω began in each participant's own private account. For the treatments with duration framing (DF), we varied the length of time (by up to one week) that the participants held the endowment prior to making their allocation decision. It is well known that framing can affect decisions [Kahneman and Tversky 1984; Samuelson and Zeckhauser 1988; Burnham et al. 2000]. The particular effects of account and duration framing are not known.

The AF treatments may elicit an endowment effect if subjects perceive the originating account as an initial property right. In auction experiments, WTA > WTP because the agent was given rights to the good in the former but offered the opportunity to acquire the good in the latter. By varying the

originating account, the VCM experiment may mimic this difference regarding the direction of the exchange offer: When ω originates in the private (public) account, the VCM contribution decision is like the WTA (WTP) auction decision. 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. There are at least two additional reasons to expect an AF effect. First, ω in either account provides a focal point [Schelling 1960] for prospective reciprocators to converge toward a fairness equilibrium [Rabin 1993]. Those people who want to treat others as others treat them would attempt to contribute the same amount as their counterpart. Some prospective reciprocators are likely to think that avoiding a reallocation is more important than moving the money. Second, the impacts of the warm glow and the cold prickle [Andreoni 1995] might differ in magnitude. Putting money in the public account would produce the warm glow associated with doing something good. Taking money out of the public account might create the cold prickle associated with doing something bad. If the magnitudes of the warm glow and cold prickle differ, then the AF treatment could affect public account contributions.

Regarding the DF treatments, there are several reasons to expect average contributions to be lower as subjects hold the endowment longer. 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].[10] Second, individuals may be more readily willing to part with windfall gains than earned wealth [Thaler and Johnson 1991]. 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 someone

[10] Cf. Strahilevitz and Loewenstein [1998], who derive duration-effect hypotheses by combining a value function from prospect theory 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" [p. 277]. Duration treatments up to one hour have 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.

else's money. Subjects who are able to savor the increase in wealth for enough time may play as though the money is their own.[11]

C. Treatments and Hypotheses

We conducted six types of 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 I summarizes the six types of treatments. Details regarding logistics and the protocol we followed are contained in an Appendix.

As is apparent from Table I, the six treatment groups were organized as three matched pairs. Comparing average contribution levels within 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. If the AF and/or DF 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 a manner that we discussed above, which would *decrease* average contribution levels.

All AF sessions were conducted in a laboratory setting. Participants did not handle 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 I shows, we named these groups *ALR* and *ALU,* respectively. Subjects then wrote down their allocation decisions and were paid accordingly at the end of the session. We can then test the following, where $\bar{c}$ is the mean (or median) public account contribution within a group.

[11] Note that this has implications for any experiment whose methodology is to give subjects an initial endowment with which to play. We return to this point later in the paper.

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, we 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 minutes, the length of time required to complete the instructions. 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 sample 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 classroom incentive to show up for Week 2, reducing the likely extent of selection effects. As shown in Table I, 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, $\bar{c}_{DLL} < \bar{c}_{DLS}$ and $\bar{c}_{DCL} < \bar{c}_{DCS}$.

III. Results

Through 12 experiment sessions, a total of 284 undergraduate participants, from a wide range of majors at the University at Buffalo (UB) and the University of North Texas (NT), each made one allocation decision. In the first set of sessions, 75 students from UB were divided among four laboratory treatments. The second set consisted of 80 students from NT, divided among the four classroom sessions. In the third set were 129 students, also from NT, divided among the remaining four laboratory sessions. The experiments were run at two schools for robustness. There is little evidence that where the experiments took place had an effect on participants' decisions that would invalidate our results (see Table I). UB students contributed less than NT students in the combined AF treatments (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 of the differences in means were similar. The contributions ranged from \$0 to \$10 as expected, and the overall mean contribution equaled 4.79. The mean contribution within treatment groups varied between 3.94 in the *ALU* treatment run at UB and 5.40 in the *DCS* treatment. The contributions overall appear normally distributed around four, but with bimodal spikes at 0 and 10 (see Figure Ia). All of the laboratory sessions had similarly shaped trimodal distributions as described above, but the

distribution of the classroom treatments was different. The distribution of contributions in the classroom treatments appears more uniform with a single mode at 10, full contribution to the public good. These full contributions do not significantly increase the means. The mean contribution in the classroom treatments equals 4.72, and the mean in the laboratory treatments equals 4.61.

H1 is rejected. Participants contributed more to the public account when the money began in their private account than when the money began in their public account. This treatment variation is the opposite of what the endowment effect would impart and is significant according to a two-sample t-test assuming unequal variances (two tailed p=0.07). As evident from Figure Ib and Table II, about \$1 *more* was contributed when ω began in the private account than when the money began in the public account.

These results are consistent with Andreoni's [1995] result that the warm glow of giving provides a greater influence than the cold prickle of taking. Participants gave more from their own accounts than they left in the public account. This is interpreted as meaning that they received more utility from giving than from not taking.[12] The focal point explanation for H1 is also rejected.

H2 receives no support. There is no indication that the duration for which one holds cash has an impact on an individual's contributions to the public good. The DF treatment variable was not close to significant according to similar t-tests performed on the laboratory (two tailed p=0.95) and the classroom (two tailed p=0.68) data (see Figures Ic and Id and Table II). 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 using just over 1,000 classroom observations. Thus, our experiment cannot support earlier speculations that the "immediacy of the transaction"

[12] 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 of 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."

[Knetsch et al. 1984] or "gambling with the house's money" [Thaler and Johnson 1991] might alter preference formation.

One might have expected that participants would have contributed less to the public account when they physically held the dollars, compared to when they were merely told that the dollars were under their control. Surprisingly, there is no indication that participants given cash (and then using the cash to make public account deposits) contributed less than those indicating their contributions in writing. Contributions were 0.46 higher (two tailed p=0.22) in the DF treatments (4.96) than in the AF (4.50) treatments (see Table II). Though not significant, the direction of the difference is opposite the expected direction. This (non) result is not included as a formal hypothesis test because we did not control for holding cash as a *ceteris paribus* treatment effect: 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. Granting imperfect control, data again indicate that the endowment effect is elusive in the cash-based VCM.

Overall, these results fail to support the endowment effect—that preferences are sufficiently reference-dependent so as to alter observed contributions to the public good. Although the VCM design used here created favorable circumstances for the endowment effect to emerge (e.g., no market incentives or experience), these conditions could be overwhelmed by the use of a cash endowment. Cash is more divisible than the goods used in earlier tests of the endowment effect—coffee mugs, candy bars, sports cards, etc. As the numeraire good, cash also differs in that subjects hold cash for future purchases, not for consumption per se [cf. Kahneman et al. 1990, p. 1328]. Our results invite an even stronger test using a less divisible, more consumable initial endowment good, while holding substitution effects constant in a public good environment.

IV. 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 WTA and WTP

measures of value [Hanemann's 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 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.

Debate over the endowment effect will continue to unfold. Traditional approaches tested for the endowment effect by comparing subjects' willingness to accept (WTA) and pay (WTP) as elicited in auction and contingent valuation studies. This literature has grappled, more recently, with the relative merits of alternative elicitation procedures and their institutional attributes [e.g. Shogren and Hayes 1997; List 2004a], rather than whether preferences are reference-dependent—the central question of the endowment effect. The VCM approach disentangles the endowment effect from value disparities and institutional differences, i.e. alternative auction mechanisms. In principle, the endowment effect can be tested in a variety of environments that offer subjects an initial endowment with which to play. Our results invite similar account framing and duration framing treatments with games other than the VCM and using initial endowments other than cash.

Finally, our approach can be viewed as legitimating 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.[13] Similarly, our results on duration framing can be received to mean that there is no evident problem 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.

[13] See the references provided by Davis and Holt [1993, p. 17]. For more recent evidence comparing student and professional data in a sophisticated signaling game, see Potters and van Winden [2000].

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Table I—Summary of Treatment Groups and Hypotheses

	Account Framing		Duration Framing			
	Laboratory		Laboratory		Classroom	
	ω begins in		ω held for		ω held for	
	Private Account	Public Account	25 minutes	1 minute	1 week	1 minute
Treatment Group Name	*ALR*	*ALU*	*DLL*	*DLS*	*DCL*	*DCS*
Endowment Effect Hypothesis	$\bar{c}_{ALR} < \bar{c}_{ALU}$		$\bar{c}_{DLL} < \bar{c}_{DLS}$		$\bar{c}_{DCL} < \bar{c}_{DCS}$	

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

Table II— Contribution Descriptive Statistics by Treatment Group*

	ALR_B	ALU_B	DLS_B	DLL_B	ALR_T	ALU_T	DLS_T	DLL_T	ALR_A	ALU_A	DLS_A	DLL_A	DCS_T	DCL_T	AL	DL	DC
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	4.73	5.25
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	0.28	0.4
Median	5	4	4	3.5	4	4	5	5	4.5	4	5	4	5	5	4	4	5
Mode	4	5	4	3	4	4	5	4	4	0	4	3	10	10	4	4	10
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	2.82	3.61
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	7.98	13.03
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
Count	15	18	20	22	35	35	29	30	50	53	49	52	45	35	103	101	80
Confidence Level(95.0%)	1.49	1.46	1.43	1.23	1.09	0.98	1.03	1.04	0.86	0.78	0.83	0.78	1.08	1.26	0.58	0.56	0.8

*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
Subscript letter, if present, denotes the location: *A*=all, *B*=University at Buffalo and *T*=University of North Texas

Table III— T-Test for Account Framing Effect

Two-Sample Assuming Unequal Variances

	ALR_A	ALU_A	DLS_A	DLL_A	DCS_A	DCL_A	$D(CL)(SL)_A$	$AL(RU)_A$
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=0.10 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	

*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
Subscript letter, if present, denotes the location: *A*=all, *B*=University at Buffalo and *T*=University of North Texas

Note. When two letters appear in parentheses, sessions of both descriptions are included in that column's statistics.

Figure I. Contribution Frequencies by Treatment Groups

Figure Ia. All Treatment Groups

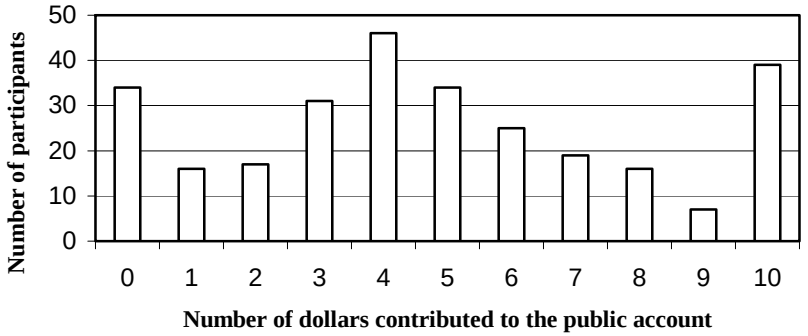


Figure Ib. Private and Public Treatment Groups

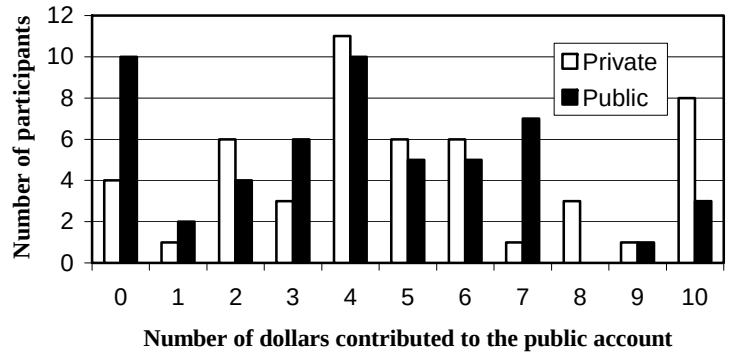


Figure Ic. Short and Long Treatment Groups
Laboratory Sessions

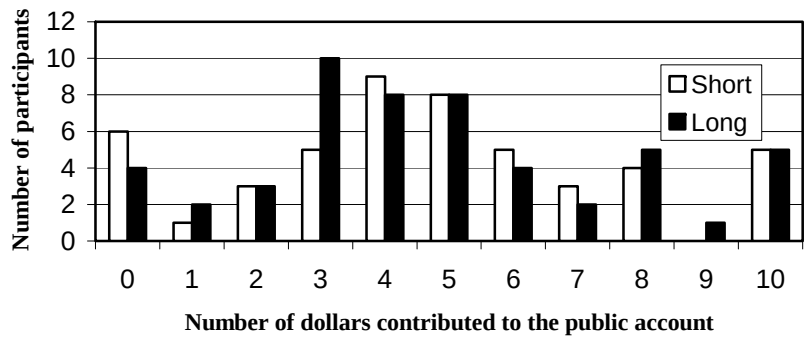
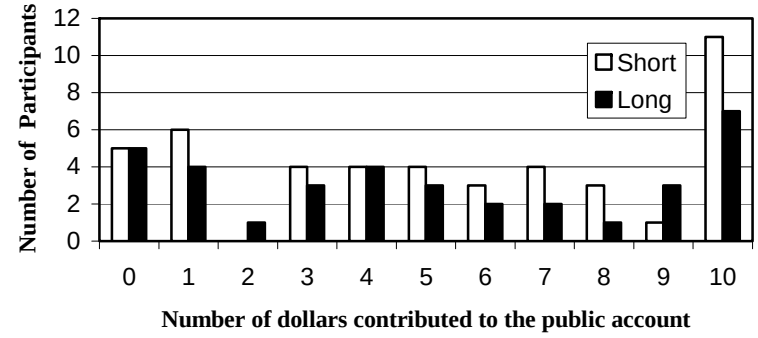


Figure Id. Short and Long Treatment Groups
Classroom Sessions



Appendix to
THE ENDOWMENT EFFECT IN A PUBLIC GOOD EXPERIMENT

Note from Authors:

This Appendix is provided to editors and reviewers in order to convey important details of the experiment. It is not necessarily intended for publication.

Italics atop the following pages contain further notes for reviewers and did not appear on the original forms used during experiment sessions.

Reviewer Note: This form is the instructions for Groups ALR and ALU (both of the account framing groups).

Private/Public Account Game: Instructions

Thank you for participating in this economics experiment. You will receive a guaranteed $5 payment at the conclusion of the experiment for being on time. In addition, you will also receive performance-based pay. The amount of performance-based pay you receive will depend on your decision and your counterpart's decision during the experiment. Each participant in this experiment makes one decision.

Participants in this experiment have been separated into two separate rooms. All participants in both rooms have received these same instructions and face the same situation. You will be randomly matched with a **counterpart** from the other room. No participants will know the identity of any counterpart, including their own.

The experiment begins with each participant receiving $10. You will have to decide how to allocate the $10 between two accounts: 1) your own **private account**; and 2) a **public account** that you share with your counterpart (both accounts are explained below). All of your $10 allocation must be deposited into one or both of these accounts. Your deposits into the two accounts must sum to $10. You and your counterpart are facing the same decision. Neither your counterpart nor the other participants will know your allocation decision. You will only know your own allocation decision, and will never learn any of the other participants' allocation decisions.

The **private account** is like a piggy bank. Any dollars you deposit in your private account are yours to keep, and will be added to what you receive from the public account.

The **public account** is shared between you and your counterpart in the following way. Any dollar deposited into the public account turns into $1.50, and is then split evenly between you and your counterpart—regardless of who initially made the deposit. So for every **$1** that *either* you or your counterpart deposit into the public account, *both* you and your counterpart each receive **$0.75**.

Your performance-based pay equals the sum of your private account deposit and your public account proceeds.

This experiment depends on all participants accurately understanding the situation. So on the following two pages we present some numerical examples, and then ask you to complete a short quiz.

Reviewer Note: This form is Week 1 instructions for Group DCL (the duration framing classroom group given cash for 1 week).

Experiment Sign-Up Form*
(Group L)

You are invited to participate in an economics experiment that will take place over two weeks. Today is **Week 1**. The experiment will continue in this class one week from today. Next week is **Week 2**.

You will receive a guaranteed $5 in cash for arriving on time to Week 2. In addition, you will also earn payments that are based on your decisions during Week 2 of the experiment.

Thus, your total pay will equal the guaranteed $5 plus your decision-based earnings. Your total pay will exceed $10.

Week 2 of the experiment will start at the beginning of class and will last approximately 30 minutes. If you are late for Week 2, we will ask you to wait outside until the experiment is completed.

In order to participate, all you have to do is complete this form and show us a picture ID.

You will also need to **bring your ID next week** in order to participate in Week 2.

We will send you a reminder email.

As we pick up the forms, we will give you ten $1 bills that are yours to keep. However, you **must bring** ten dollars with you to Week 2 in order to participate in the experiment. If you do not bring ten dollars to Week 2 you will not be allowed to participate—you will not receive the guaranteed $5 or the additional decision-pay. **Remember:** by participating in Week 2 you will earn more than $10.

If you wish to participate, please fill out the information below and produce your ID.

Name (print):_____

Signature:_____ (indicating receipt of $10)

Email address (print):_____

Reviewer Note: This form is Week 1 instructions for Group DCS (the duration framing classroom group given cash for 1 minute).

Experiment Sign-Up Form *

(Group S)

You are invited to participate in an economics experiment that will take place over two weeks. Today is **Week 1**. The experiment will continue in this class one week from today. Next week is **Week 2**.

> You will receive a guaranteed $5 in cash for arriving on time to Week 2. In addition, you will also earn payments that are based on your decisions during Week 2 of the experiment.
>
> Thus, your total pay will equal the guaranteed $5 plus your decision-based earnings. Your total pay will exceed $10.
>
> Week 2 of the experiment will start at the beginning of class and will last approximately 30 minutes. If you are late for Week 2, we will ask you to wait outside until the experiment is completed.

In order to participate, all you have to do is complete this form and show us a picture ID.

You will also need to **bring your ID next week** in order to participate in Week 2.

We will send you a reminder email.

If you wish to participate, please fill out the information below and produce your ID.

Name (print):_____

Signature:_____

Email address (print):_____

Reviewer Note: This form is Week 2 instructions for Group DCL and instructions for Group DLL (the duration framing groups that held the initial endowment for1 week and 25 minutes, respectively).

INSTRUCTIONS

Thank you for participating in this experiment.

Participants in this experiment are currently in two rooms. Every participant has received the same instructions and faces the same situation. Every participant will make one decision. You will be randomly matched with a **counterpart** from the other room. No participant will know the identity of any participant's counterpart, including his or her own counterpart.

You will receive a guaranteed $5 payment at the conclusion of the experiment for being on time. In addition, you will also receive decision-based pay. The amount of your decision-pay will depend on your decision and the decision of your counterpart.

You have been given ten $1 bills. Your decision is to determine how to allocate the ten $1 bills between two accounts: 1) your own **private account**; and 2) a **public account** that you share with your counterpart.

The **private account** is like a piggy bank. Any $1 bills you deposit in your private account are yours to keep, and will be added to what you receive from the public account.

The **public account** is shared between you and your counterpart in the following way. Any dollar deposited into the public account turns into $1.50, and is then split evenly between you and your counterpart—regardless of who initially made the deposit. So for every $1 that either of you deposit into the public account, both of you receive $0.75 each.

Your decision-pay equals your private account deposit plus your public account winnings. Your total pay equals your decision-pay plus the guaranteed $5.

This experiment depends on all participants accurately understanding the situation. So on the following two pages we present some numerical examples, and then ask you to complete a short quiz.

Reviewer Note: This form is Week 2 instructions for Group DCL and instructions for Group DLL (both of the duration framing groups that held the initial endowment for 1 minute).

INSTRUCTIONS

Thank you for participating in this experiment.

Participants in this experiment are currently in two rooms. Every participant has received the same instructions and faces the same situation. Every participant will make one decision. You will be randomly matched with a **counterpart** from the other room. No participant will know the identity of any participant's counterpart, including his or her own counterpart.

You will receive a guaranteed $5 payment at the conclusion of the experiment for being on time. In addition, you will also receive decision-based pay. The amount of your decision-pay will depend on your decision and the decision of your counterpart.

You will be given ten $1 bills. Your decision is to determine how to allocate the ten $1 bills between two accounts: 1) your own **private account**; and 2) a **public account** that you share with your counterpart.

The **private account** is like a piggy bank. Any $1 bills you deposit in your private account are yours to keep, and will be added to what you receive from the public account.

The **public account** is shared between you and your counterpart in the following way. Any dollar deposited into the public account turns into $1.50, and is then split evenly between you and your counterpart—regardless of who initially made the deposit. So for every $1 that either of you deposit into the public account, both of you receive $0.75 each.

Your decision-pay equals your private account deposit plus your public account winnings. Your total pay equals your decision-pay plus the guaranteed $5.

This experiment depends on all participants accurately understanding the situation. So on the following two pages we present some numerical examples, and then ask you to complete a short quiz.

Reviewer Note: This form is given with instructions for all groups.

NUMERICAL EXAMPLES

Example 1:

You allocate:	***Your counterpart allocates:***
2 into private account	3 into private account
8 into public account	7 into public account

Public Account total is 8+7:	= 15
This gets multiplied by 1.5:	= 22.5
Each participant gets one half:	= 11.25

Your decision-pay:	***Your counterpart's decision-pay:***
2 + 11.25 = $13.25	3 + 11.25 = $14.25

Example 2:

You allocate:	***Your counterpart allocates:***
8 into private account	7 into private account
2 into public account	3 into public account

Public Account total is 2+3:	= 5
This gets multiplied by 1.5:	= 7.5
Each participant gets one half:	= 3.75

Your decision-pay:	***Your counterpart's decision-pay:***
8 + 3.75 = $11.75	7 + 3.75 = $10.75

In addition to these performance-based earnings, you will each receive $5 just for showing up on time.

Reviewer Note: This form is given with instructions for all groups.

QUIZ

Suppose you deposit $3 into the public account and your counterpart deposits $7 into the public account, how much decision-based earnings will each participant receive? Show your work below.

Show your work here:

Your decision-based earnings would be:	***Your counterpart's decision-based earnings would be:***

Reviewer Note: This form is the response sheet for Group ALR (the account framing group for which the initial endowment originates in the <u>private</u> account).

Private/Public Account Game: Response Sheet

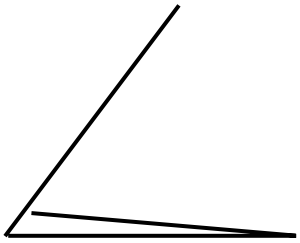
Your deposit decision

You and your counterpart are in identical situations. $10 has been allocated to your private account. You control how much of this $10 will be deposited into the public account and into your private account.

How many dollars do you wish to deposit into the **public account**? (Circle the appropriate number.)

0 1 2 3 4 5 6 7 8 9 10

Fold this response sheet into three approximately equal sections, as you would a letter (see below). Place this folded sheet into the envelope. Do not seal the envelope.



Reviewer Note: This form is the response sheet for Group ALU (the account framing group for which the initial endowment originates in the public account).

Private/Public Account Game: Response Sheet

Your deposit decision

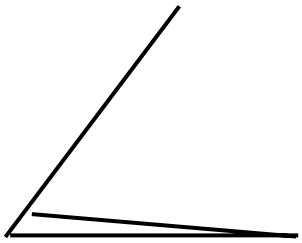
You and your counterpart are in identical situations. $10 has been allocated to the public account.

You control how much of this $10 will be deposited into the public account and into your private account.

How many dollars do you wish to deposit into the **public account**? (Circle the appropriate number.)

0 1 2 3 4 5 6 7 8 9 10

Fold this response sheet into three approximately equal sections, as you would a letter (see below). Place this folded sheet into the envelope. Do not seal the envelope.



Reviewer Note: This form is the response sheet for Groups DLL, DLS, DCL, and DCS (all of the duration framing groups).

RESPONSE SHEET

This form will be used to record your actual deposit decision.

After you have made your decision, write your decision here:

I wish to allocate	**I wish to allocate**
to the private account	**to the public account**

Double check that your private plus public account allocations equals 10.

Now place the number of $1 bills you wish to allocate to the **public account** on top of this sheet.

Double check that the number of $1 bills is the same as the amount entered in the box on the right above.

Now fold the sheet into three approximately equal sections, as you would a letter (see below). Place the folded sheet with $1 bills into the envelope. **Do not seal the envelope**.