

What Endowment Effect? A Public Good Experiment¹

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Abstract: Previous tests of the endowment effect have employed either auctions or contingent valuations. 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. Data do not indicate that preferences shift in a way consistent with the endowment effect.

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What Endowment Effect? A Public Good Experiment

1. Introduction:

Traditional approaches to testing the endowment effect have employed either auction experiments or contingent valuation methods. We introduce a new approach using instead a public good experiment. In treatments designed to alter subjects' perceived control over an initial endowment, we find no evidence that preferences shift as suggested by the endowment effect.

Disparities between individuals' willingness to accept (WTA) and pay (WTP) are so commonly observed that it now seems naïve to argue that such apparent anomalies do not exist under certain conditions. Instead, the controversy has recently centered on what explains observed value disparities: the endowment effect vis-à-vis neoclassical consumer theory. The endowment effect suggests that loss aversion or status quo bias can create some manipulation (shift, kink, rotation, etc.) of standard indifference curves occurs at the point of initial endowment (Knetsch 1989; Kahnemann, Knetsch and Thaler 1991; Morrison 1997). Recent studies have focused on whether acknowledged value disparities are borne of such anomalous preference formation or are instead due to substitution effects consistent with conventional preferences. It has been widely shown that competitive market forces cause observed value disparities to diminish (Brookshire and Coursey 1987; Shogren et al. 1994; List 2003). Thus market discipline and the information acquired from market experience tend to mitigate value disparities, whether they are due to endowment or substitution effects. Recent theoretical clarifications predict smaller value disparities for goods that are more substitutable (Hanemann 1991, 2003). This prediction is supported by experimental data within a Vickrey auction environment (Shogren et al. 1994, 1997). However, the question of whether value disparities are preference-based (i.e., fundamental) remains. The literature leaves open the conflated possibility that preferences are anomalous yet constrained in the usual manner by market institutions: the consumer wants, the consumer cannot necessarily have. This suggests that there is value in testing for the presence of

an endowment effect in a non-market situation that controls for the substitution effect as a viable alternative explanation.

We present a one shot public good (voluntary contribution mechanism) design that allows the experiment simultaneously to: 1) eliminate market discipline; 2) eliminate market experience; 3) control for substitution effects; and 4) observe treatments that one would expect to elicit the endowment effect. 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 use a public good that is perfectly substitutable: cash. Therefore, our experimental environment creates favorable circumstances for observing an endowment effect, while holding constant the leading alternative explanation for value disparities.

The results from this experiment do not support the thesis that preferences depend on initial endowments. In one set of treatments, the duration for which participants hold a cash endowment before making their public good decisions fails to influence participants' allocations. In another set of treatments, participants contribute more (less) to the public account when the endowment is said to start in participants' private (public) accounts. The direction of this difference is the opposite of what the endowment effect should impart.

In Section 2 we discuss previous tests of the endowment effect and explain how the VCM can be applied. In Section 3 the experimental design and hypotheses are explained. Section 4 contains the results, and Section 5 concludes.

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 (1980) endowment effect, which suggests that agents may value a good more highly when the property right is already established. Knetsch and Sinden

(1984), for example, show that $WTA > WTP$ for lottery tickets and attribute the disparity to the endowment effect and loss aversion. Subsequent experiments—e.g., Coursey, Hovis and Schulze (1987), Knetsch and Sinden (1987), Kahneman, Knetsch and Thaler (1990)—allowed for subject experience, yet also explained observed value disparities as endowment effects.² 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.³

These researchers invoked the endowment effect explanation because received theory (Willig 1976; Randall and Stoll 1980) provided 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 value disparity for quantity changes of public goods depends on both the income and the substitution effects. His theory demonstrates 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).⁴ This implies that the substitution and endowment 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 (decreased health risk). For the high-substitutable good they find that the value disparity diminishes to statistical zero, and

² The subsequent studies cited also achieved greater control by eliminating the need for subjects to calculate expected winnings, and differing attitudes toward risk, associated with lottery tickets.

³ Going further, Kahnemann, Knetsch, and Thaler (1991) argue that the endowment effect can result in intersecting indifference curves.

⁴ Hanemann reformulates the bounds on the neoclassical compensating and equivalent variations determined earlier by Willig (1976) and Randall and Stoll (1980). He reduces the difference between WTA and WTP to the ratio of income elasticity of the public good to the elasticity of substitution between public and private goods. As we will argue below, our experiment assumes negligible income effects and holds the substitution effect constant in treatments designed to elicit an endowment effect.

converges to the approximate market price, after approximately four trials. However, for the low-substitutable good, the value disparity persists even after many trials.

Shogren et al. (1997) isolated the implications of different auction mechanisms (i.e., institutions) on measured value disparities. They recreated the Kahneman et al. (1990) coffee mug experiments, which used a random bid (Becker-DeGroot-Marshack) auction, using instead a Vickrey auction. Shogren et al. found, contrary to Kahneman et al., that the value disparity diminishes after the first of ten trials. In short, their results support Haneman's substitution effect over the endowment effect with the explanation that the Vickrey mechanism is more market-like than the BDM elicitation procedure. List (2003) confirmed the market experience effect 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 their design required the endowment and substitution effects to work mutually exclusively. Following the irreversible indifference curve logic of 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. Shogren and Hayes (1997), in response, note that the degree of Morrison's pivots are seemingly arbitrary. By using different pivots they show that the value disparity can be of equal size for linear as for 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) Some degree of 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.⁵ (4) The endowment effect, if it exists, can be described as some manipulation (e.g., rotation, pivot, kink) of agents' indifference curves. Accordingly, identifying said manipulations could test its existence, whether in an auction or some other experimental environment.

The VCM approach to testing the endowment effect simultaneously addresses several aspects of this developing literature. First, one of the parameters 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 rotate/pivot/kink 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 the ideal 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 experiments have been used to test for value disparities in experiments; only auctions have been tried. One study (Brookshire and Coursey 1987) uses a public good (trees in a neighborhood park, which have “a large degree of substitutability” p.555),

⁵ See Brookshire and Coursey (1987), Coursey, Hovis and Schulze (1987), Shogren et al. (1994; 1997) and List (2003). 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, d'Arge, and Brookshire 1980). Second, buyers are often 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, Smith and Williams 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).

but its purpose is to compare the results of contingent valuation versus auction mechanisms. The VCM approach supplements previous work that has experimented on both high- and low-substitutable private goods but so far only on imperfectly substitutable public goods. Our study is the first to offer evidence on this literature using a public goods experiment.

3. 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 maximize⁶

$$[1] \quad u^i = u^i(\omega - c^i, g(\Sigma c^i))$$

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

$$[2] \quad du^i = -1 + \frac{u_y^i}{u_x^i} g'$$

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, Walker and Thomas (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 $MRS_{x,y}$ (under a given payoff structure) and the marginal rate of transformation (as specified by experiment parameters). Proceeds from the public account depend

⁶ The following exposition is based on Isaac, Walker and Thomas (1984) and Ledyard (1995).

on the technology of the experiment, g , which characterizes the *MRT*. The general form of the VCM public good production function is $g = \frac{a\sum c^i}{N}$, 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 reveal a contribution rate approximately 40 per cent of the optimal (Ledyard 1995; Dawes and Thaler 1988). 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, McCabe and Smith 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 (Shelling 1960) for prospective reciprocators to converge toward a fairness equilibrium (Rabin 1993). If some people want to treat others as others treat them, they would attempt to contribute the same amount as their counterpart. Some prospective reciprocators are likely to think that avoiding a reallocation is more focal 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.

We are unaware of any previous experimental evidence on duration effects, but there are a few 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, Knetsch and Thaler 1991). 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 with someone 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.⁷

⁷ 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.

C. Treatments and Hypotheses

We conducted six types of treatment groups, two for AF and four for DF. Each treatment group consists 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 1 summarizes the six types of treatments. Details regarding logistics and the protocol we followed are reserved for the Appendix.

As you can see from Table 1, our six treatment groups are organized as three matched pairs. Comparing average contribution levels within pairs provides the basis for our hypothesis tests. The null hypothesis is no identifiable treatment effect. The alternative hypothesis is provided by the direction of the anticipated endowment effect. If our 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 we discussed above (c.f. Morrison 1997; Shogren et al. 1997), and *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. Subjects then wrote down their allocation decisions and were paid accordingly at the end of the session. As Table 1 shows, we assign these groups the treatment names *ALR* and *ALU*, respectively. We can then test the following, where \bar{c} is the mean 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 we have “Short” and “Long” groups defined by the length of time 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 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 Long participants to contribute less on average to the public account than the Short group. The obvious difficulty with this approach arises insofar as participants who attended Session 1 fail to show up for Session 2. If those who do show up for Session 2 were 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 in 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. Under this approach, students have the added classroom incentive to show for Week 2, reducing the likely extent of selection effects. As shown in Table 1, we have 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 $\bar{c}_{DCL} < \bar{c}_{DCS}$.

4. Results

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 any affect on participants' decisions that would invalidate our results. (see Table 1). But, Buffalo students contribute less than the Texas students in the AF treatments combined (mean 4.12 versus 5.17, two tailed $p=0.07$). 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-\$10 as expected and the mean contribution equaled 4.79. The mean contribution to the public good in each treatment varied between 3.94 in the private frame treatment run in Buffalo and 5.40 in the short frame classroom treatment. The distribution of

contributions appears normally distributed around four, but with bimodal spikes at 0 and 10. (See Figure 1) Interestingly, all of the classroom sessions had similarly shaped trimodal distributions as described above, but the distribution of the classroom treatments is 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 increase the means significantly. 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 $p=0.07$). (See Figure 2 and Table 2) About \$1 more was deposited into the public account when the money 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.⁸ 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 one's contribution 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 3 and 4 and Table 2.) Thus, our experiment

⁸ 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."

cannot support earlier speculations that the “immediacy of the transaction” (Knetch 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) contribute less than those indicating their contributions in writing. Contributions were 0.47 higher (two tailed $p=0.22$) in the DF treatments (4.96) than in the AF (4.50) treatments. (See Table 2) 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 do 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 VCM.

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 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, Knetsch, and Thaler 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 frame the initial endowment in several different ways. According to

the endowment effect, treatments in which subjects had greater possession of 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 the endowment effect.

Debate over the endowment effect will continue to unfold. The traditional approach tests for the endowment effect by comparing subjects' willingness to accept (WTA) and pay (WTP) as elicited in auction experiments and contingent valuation mechanisms. This literature has grappled, more recently, with the relative merits of alternative elicitation procedures and their institutional attributes 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 between alternative auction mechanisms. In principle, the endowment effect can be tested in a variety of experiments that offer subjects an initial endowment with which to play. The duration effect shows particular untapped promise. Our results invite similar treatments using games other than the VCM.

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 behavior 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.⁹ Similarly, our results on duration framing can be received to mean that there is no evident problem allowing subjects to make their experiment decisions soon after

⁹ 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).

receiving the initial endowment. This could easily be explained by small income effects, so further research is called for.

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Table 1—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	<i>ALR</i>	<i>ALU</i>	<i>DLL</i>	<i>DLS</i>	<i>DCL</i>	<i>DCS</i>
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 average contribution within a treatment group. ω is the initial endowment.

Table 2—Descriptive statistics of amount contributed*

	Session																
	<i>ALRB</i>	<i>ALUB</i>	<i>DLSB</i>	<i>DLLB</i>	<i>ALRT</i>	<i>ALUT</i>	<i>DLST</i>	<i>DLLT</i>	<i>ALRA</i>	<i>ALUA</i>	<i>DLSA</i>	<i>DLLA</i>	<i>DCST</i>	<i>DCLT</i>	<i>AL</i>	<i>DL</i>	<i>DC</i>
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

*Notation Key — First letter denotes kind of treatment: *A*=account framing and *D*=duration framing

Second denotes setting: *C*=classroom and *L*=lab

Third letter denotes specific treatment: *L*=long, *S*=short, *R*=private account and *U*=public account

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

Table 3— T-Test for Account Framing Effect

Two-Sample Assuming Unequal Variances

	<i>ALRA</i>	<i>ALUA</i>	<i>DLSA</i>	<i>DLLA</i>	<i>DCSA</i>	<i>DCLA</i>	<i>D(CL)(SL)A</i>	<i>AL(RU)A</i>
Mean	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	

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

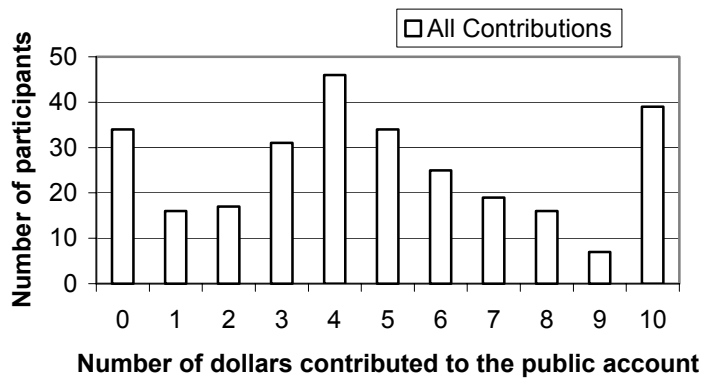


Figure 1. All Contributions

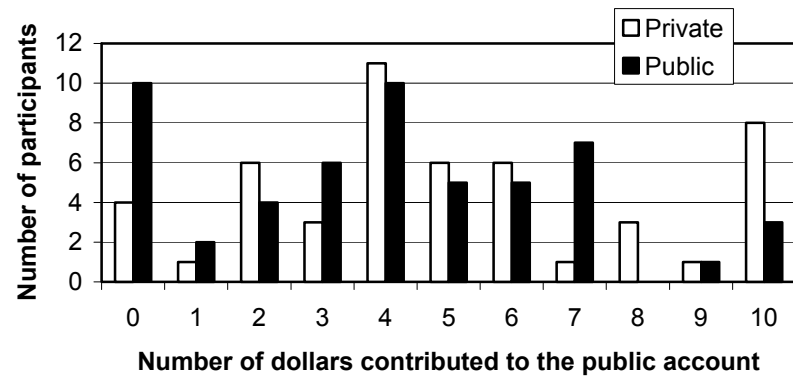


Figure 2. Private Versus Public Account Frames

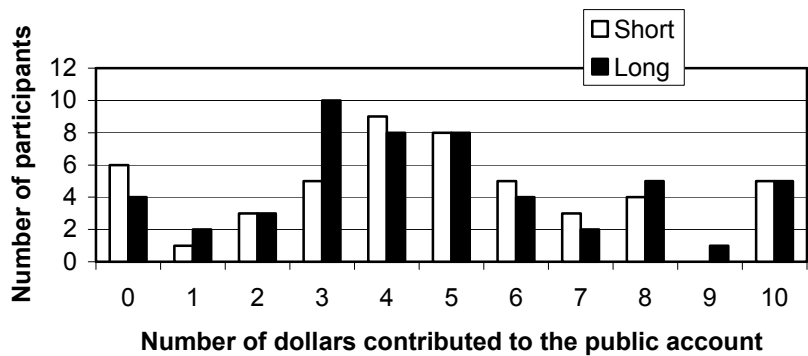


Figure 3. Short Versus Long Laboratory Frames

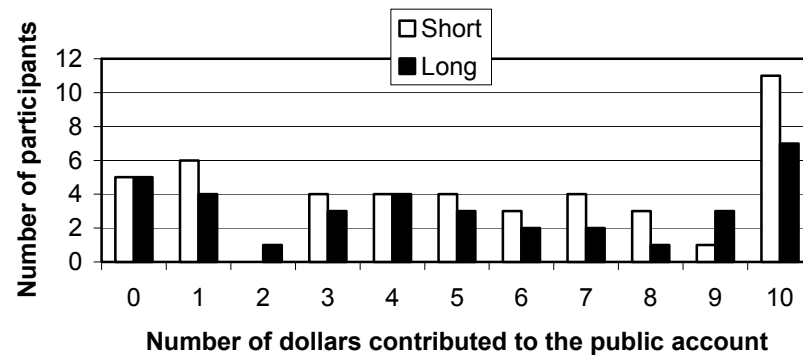


Figure 4. Short Versus Long in Class Frames