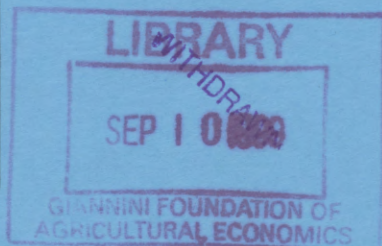


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## HOUSE MONEY EFFECTS IN PUBLIC GOOD EXPERIMENTS

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## House Money Effects in Public Good Experiments

Jeremy Clark\*

August 1<sup>st</sup>, 1999

Are contributions in voluntary public good experiments inflated because subjects are given money for their initial endowments? There is evidence that people receiving small, one time "windfall gains" have a high marginal propensity to consume them, and when doing so, exhibit greater risk-seeking behaviour. Similar effects may be present in voluntary contribution experiments, causing subjects to contribute more to public goods than they would if using their own money. The effect of windfall money is tested by comparing VCM contribution rates when subjects supply their own endowments with those when endowments are provided, while holding constant the distribution of total promised earnings.

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## 1. Introduction

A standard practice in economics experiments is to provide subjects with an initial endowment of money. This endowment serves as "starting capital" from which subjects draw when making decisions of interest to the experimenter. While there are good reasons for the use of such endowments, their effects have not been widely investigated. Yet there is some empirical evidence from economics and psychology that start-up money may create a "house money effect." People may treat this money differently than their regular earnings. This paper investigates the house money effect in the case of voluntary contributions to a public good.

A robust finding within the experimental voluntary public good (VCM) literature is that a significant proportion of individuals contribute towards a public good when the individually payoff-maximizing choice is to free-ride on others' contributions (Ledyard, 1995). This result would seem to correspond to the empirical observation that private organizations can successfully raise one-time contributions for charitable causes.

It is possible, however, that the level of provision observed in VCM experiments may provide misleading indications of the degree of free-riding that private organizations can expect. For subjects making generous contributions to the public good do so from an initial endowment funded by the experimenter. In contrast, charities must ask people to contribute out of their own pre-existing income.

This paper reports a test for house money effects in a standard (VCM) experiment. A control treatment is run using the standard Isaac, Walker and Thomas (1984) and Andreoni (1995) design, with linear payoffs, 10 decision- rounds, and continuous mixing. This is compared to a second treatment in which subjects use their own money to fund their initial endowment. The range of money subjects can expect to earn is held constant across the two treatments.

The paper is organized as follows. Section 2 provides a survey of the literature on house

money effects. Section 3 describes the experimental design used here, and Section 4 provides the results and analysis. A brief discussion and conclusion follow in Section 5.

## **2. Windfall Money, Marginal Propensity to Consume (MPC), and Risk Preference**

Various objections to experimental economic methodology have been raised by sceptics within the profession. It has been argued that university student subjects are not representative of adult decision-makers, or that the financial incentives provided in experiments are too small to be salient. Appropriately enough, such objections have been addressed empirically. Experiments have been replicated with businessmen (Smith, Suchanek and Williams (1988) p. 40), church congregations (Cummings, Harrison and Rutstrom (1995)), or actual families (Peters, Unur, Clark and Schulze (1997)). Financial stakes in bargaining experiments have been scaled upward, or experiments have been conducted in poorer countries where the payments represent greater purchasing power (Hoffman, McCabe and Smith (1996), and Kachelmeier and Shehata (1992)). More recently, experimental economists themselves have recognized that they do not have complete control over subjects' preferences according to the payoff structures they design. Some subjects bring altruism, envy, reciprocal norms or fairness concerns to an experiment, along with a desire to make money. There is ongoing research into the types of other-regarding preferences that need to be reckoned with, and the environments in which they are invoked (Fehr and Schmidt (forthcoming), Bolton and Ockenfels, 1998).

There is another objection to experimental methodology, however, that has received surprisingly little empirical attention, despite being well-recognized. There is disparate evidence that people treat small amounts of "windfall" (unexpected) money differently than they do their regular income. Evidence of a "windfall effect" has emerged in the macroeconomic investigation of Friedman's (1957) permanent income (PI) hypothesis. According to the PI theory, people with

preferences for smooth life-time consumption will have a smaller marginal propensity to consume (mpc) from a windfall increase in income if it is to be temporary (one-time) rather than permanent. Empirical tests of the permanent income hypothesis have been conducted using American war veteran's payments or windfall cash and insurance payouts reported in the United States Consumer Expenditure Survey. These tests have yielded only mixed support for PI (see, for example Lee (1975) and Keeler, James and Abdel-Ghany (1985)). Relevant here, Keeler et al. find that a person's mpc from a one-time windfall gain depends upon the size of the gain relative to the person's permanent income. Consistent with the PI hypothesis, the mpc out of a one-time windfall gain is lower on average than that from a permanent one if the gain represents 50% or more of permanent income. However, the mpc for one-time and permanent windfall gains become roughly equal if the gain is only 20% - 50% of the size of permanent income. And for gains less than 20% of permanent income, the mpc out of one-time windfall gains is extremely high absolutely, and relative to that from permanent gains. Thus, recipients of small *one-time* windfalls will tend to spend more of them than they would comparable *permanent* ones.

There is also evidence from psychology that people treat small windfall gains differently from small *expected* gains. Arkes, Joyner and Pezzo (1994) find in experiments that subjects spend more at a sporting event or place larger gambling bets when given unanticipated, rather than anticipated money. Arkes et al's work suggests a link between differential mpc's and risk preference, and this has found support in Battalio, Kagel and Jiranyakul (1990), Thaler and Johnson (1990) and Keasey and Moon (1996). Each of these latter studies find that subjects are more risk seeking over uncertain choices after being given starting endowments. For example, Thaler and Johnson find that subjects are more risk-seeking if a one-stage lottery is redefined as a two-stage one that keeps final earnings constant but introduces an initial certain gain.

Some behavioural explanations for windfall effects have been suggested. High windfall

mpc's have been attributed to "mental accounting" as described in Sheffrin and Thaler (1988), Thaler (1985) and Thaler and Johnson (1990). Individuals are thought to place money accumulated under different circumstances into separate, imperfectly-fungible mental accounts, with differing mpc's. Thus people may place small, one-time windfall gains in a "mad money" account with an mpc near unity, in a way they would not do with anticipated or large unanticipated income gains. Thaler and Johnson (1990) next combine mental accounting with prospect theory to explain risk preference effects. The authors borrow the gambling expression "playing with the house money" to denote this effect, where losses faced after an initial gain are coded merely as reductions in the initial gain. Reduced gains are thought to be less painful than out-of-pocket losses, and promote more risk-seeking behaviour up to the point the initial gain is exhausted.

What relevance would house money effects have in economics experiments? Two possibilities seem obvious. First, in experiments where subjects can use their decisions *within* an experiment to "purchase" non-pecuniary goods, such as fairness, altruism or revenge, they might be willing to buy more if their starting incomes are windfall gains. There is indirect evidence of this in dictator experiments run by Oberholzer-Gee and Eichenberger (1999). They find that dictators become less generous when given undesirable third options aside from simply giving or keeping money. The authors interpret this to mean that subjects do not fully consider the opportunity costs of the money they give away in experiments. Extrapolating to public goods, giving subjects their initial "investment accounts" may cause them to purchase more "public-spiritedness" by contributing than they would have if they had to venture their own permanent income.

Secondly, house money effects may cause experimental subjects to be unusually risk-seeking in experiments. In the case of public goods, Andreoni (1988) has posited that subjects

might contribute "strategically" in early rounds of the VCM to invoke reciprocal norms and coax others into contributing. Weimann (1994) and Andreoni (1988) have found only mixed evidence of this, but to the extent it occurs, subjects might be more likely to venture their starting money in this way if it were bestowed rather than brought.<sup>1</sup> A natural way to test for both effects jointly would be to have subjects use their own money to fund their investment accounts.

### 3. Experimental Design

Two treatments of a VCM experiments were planned as follows, using the standard Isaac, Walker and Thomas (1984) and Andreoni (1995) VCM design. For each of 10 rounds, each participant is "placed" in a new group of 5 people, endowed with 80 tokens, and asked to allocate them between an "Individual Exchange" and a "Group Exchange." Each token yields one (New Zealand) cent in the Individual Exchange, and one-half a cent for each of the group members in the Group Exchange ( $MPCR = .5$ ). With this linear payoff design, a subject who wishes only to maximize own-earnings, and who assumes all others do also, has a dominant strategy to invest zero tokens into the Group Exchange on every round. Total earnings (and efficiency) are maximized, however, if all subjects were to invest all 80 tokens in the Group Exchange on every round. Individuals would each earn \$20 over all rounds under the efficient solution, and \$8 under the dominant strategy.

All subjects were recruited with the requirement that "you must bring \$8 to the experiment." They were also informed that they could avoid losing the \$8 with certainty, and that they could also expect to earn, on average, between \$10 and \$18 (N.Z.) for a one hour session.

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<sup>1</sup> Such giving is not strategic in the standard game-theoretic sense for a finite round VCM experiment; giving zero is always the dominant strategy. As Kreps et al. (1982) show, however, early giving could be rational for income-maximizing subjects if they are uncertain about the objectives of others.



Sixty students participated in total, with thirty per treatment in two sessions of fifteen each. Within each session, subjects formed three groups of five for each decision round. The experiment was conducted manually with paper decision slips in a classroom, with a single computer outside. No communication was allowed, and students were placed so as not to be able to view others' decisions. Two assistants collected the slips within the classroom, and a third entered the results outside on a spreadsheet. A blank "Earnings Report" containing the earnings from the Individual Exchange, Group Exchange, and Total, was collected from subjects after each round along with their decision slip. The Report was then filled in by assistants outside the classroom, and passed back to subjects before each successive round. Group compositions were pre-ordered so that individuals would never be in exactly the same group twice. After making decisions over ten rounds, subjects were called out of the room individually, and paid.

Treatment I, the control treatment, was conducted along these lines. In Treatment II, after everyone was seated, (different) subjects were asked to pay the \$8 that all subjects had been told to bring. Two lines were inserted near the start of their instructions which read:

"In a few moments, you will be asked to give the \$8 you brought with you today to the experimenter. The \$8 will be used to fund your personal investment account for use in today's experiment."

As with all treatments, subjects were then told that "[e]ach participant will always be able to avoid money losses with certainty through his or her own decisions." [underlining in original]. The session was then held as in Treatment I. Since on net, subjects in Treatment II would accumulate \$8 less than those in the control treatment, an unannounced \$8 participation fee was added *ex post* to the earnings they accumulated over the 10 rounds before their session earnings were calculated and paid out. Experimental control thus relies upon subjects being promised and correctly *expecting* to earn the same amount in each treatment, while having to use their own money to fund their endowment in Treatment II.

With linear payoffs, and continuous mixing, both treatments have the standard (selfish preference) game-theoretic predictions that contributing zero each round is a dominant strategy.

## **4. Results**

### **4.1 Subject Pool**

Subjects were recruited among undergraduate students at the University of Canterbury in Christchurch, New Zealand, over a two month period in early 1999. Attracting participants from large lectures was more difficult than anticipated, and recruitment was carried out in lectures for first and second year economics, second year math, and first year political science.<sup>2</sup> Thus there was greater subject heterogeneity than is ideal. Treatment sessions were run in the sequence I, II, II, I, and as far as possible, a mix of students from the various classes was sought for each session. A high degree of subject heterogeneity may increase the variance of the decisions made within any given session. Similarly, cohort effects that remain may have increased the differences across supposedly identical sessions, making it more difficult to discern differences between treatments.

### **4.2 Pooling Sessions within Treatments**

Whether due to subject pool heterogeneity or other causes, different sessions of identical treatments yielded surprisingly different mean contribution levels. A Mann-Whitney rank sum test was used to compare the sample distribution of contributions of each subject (averaged over all rounds) between sessions. This test checks whether sample means are significantly different in the two sessions of a given treatment. It found a significant difference between the sessions of the own-money treatment at the 5%, though not 1% level. The same test did not find a significant

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<sup>2</sup> The experiment reported here was part of a larger research project involving the recruitment of 120 subjects.

difference within the control treatment. Figure 1 illustrates the intra-treatment variance; mean contributions appear systematically *higher* in the first session of Treatment I, and *lower* in the first session of Treatment II.

If we break up the comparison between sessions round by round, we see that neither pair of sessions in Treatments I or II are significantly different in the first round. In Treatment I (the control), however, the two sessions produced significantly different distributions at the 5% level for Rounds 2, 3 and 4. In Treatment II (own money), the two sessions differed significantly at the 5% level for Rounds 5, 6, 7, 8 and 10, with Round 8 alone different at the 1% level. The lack of significant difference in Round 1 between each pair of sessions is of some comfort. It suggests that subjects were not so different in background between sessions as to behave differently from the outset. In the non-parametric analysis that follows, I shall pool the two sessions for Treatment I despite the high intra-treatment variation. Session effects shall be controlled for, however, in the regression analysis that follows.

#### 4.3 Findings

Figure 2 presents the mean contribution towards the public good, round by round, for the control (I) and own money (II) treatments. Overall the mean contributions show a familiar VCM pattern, starting at just below 50% of the maximum and declining gradually over the 10 rounds. At first glance, the average contribution per round appears slightly but consistently lower when subjects use their own money. We turn next to test whether this appearance is statistically significant, first using the Mann-Whitney rank sum test, secondly by regression analysis.

If we take the average contribution of each subject across all rounds as our unit of observation, we can compare the distribution of contributions between treatments. Using this approach, the distribution of giving was not significantly lower when subjects had to use their own money to fund their endowments ( $z = .732$ ,  $\alpha = .465$ ,  $N_I = N_{II} = 30$ , two tail test).

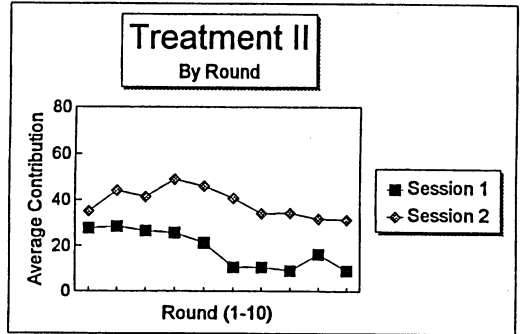
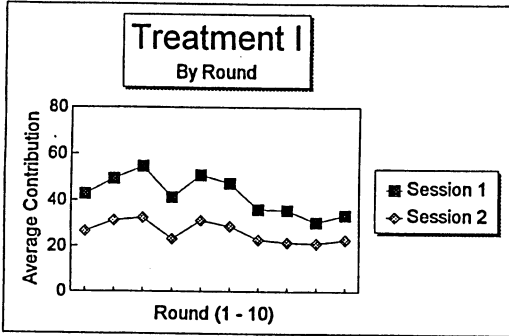


Figure 1: Session Effects

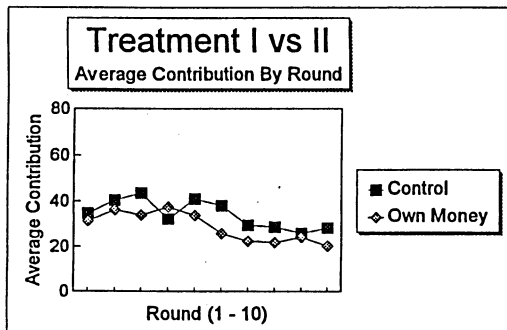


Figure 2: Treatment Effects

As an alternative approach, we can compare the distribution of contributions of subjects round by round. These results are presented in Table 1 below. While the differences are of the predicted sign in 9 of 10 cases, only one (Round 6) is significant at the 10% level (two-tail). Thus the Mann-Whitney test does not rule out the possibility that the suggestive effect in Figure 2 is merely sampling variation.

A suggestive but insignificant result is less than satisfying. A house money effect may be present, but so weak as not to be discernible with this sample size and non-parametric test. As an thumbnail test of sample size effects, the samples in both treatments were hypothetically doubled by replicating actual observations. This has the effect of preserving sample variance, while simulating the increased power of a larger sample size. Mann-Whitney tests on the doubled sample still failed to find a significant difference between treatments overall, or in any but the sixth round, as before.<sup>3</sup> Since the between-sample Mann-Whitney test has a relatively low power to detect differences, and there are uncontrolled session effects in at least the own-money treatment, we shall turn next to parametric regression analysis.

The contribution of each subject in each round was regressed on a constant and dummy variables for treatment, session, session-treatment interaction and round using ordinary least squares.<sup>4</sup> Unfortunately, models that control for subject-specific effects, such as random or fixed-effects regressions, cannot be run while simultaneously testing for treatment effects in a between-subject design. Standard errors are also estimated using the White heteroscedasticity-consistent estimator, with little effect (Greene, 1997). This and several other tests (Glejser, Breusch-Pagan-

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<sup>3</sup> The standard normal statistic between treatments over all rounds becomes  $z=1.047$  ( $\alpha = .29$ ,  $N_t = N_n = 60$ ), and the difference in the sixth round becomes significant at the 5% rather than 10% level.

<sup>4</sup> The baseline constant is average contribution in the first round of the first session of the control treatment.

Table 1  
A Test of Own-Money Effects, Round by Round

	Round									
	1	2	3	4	5	6	7	8	9	10
	(z values, two-tail tests)									
I>II?	.547	.517	1.14	<i>-.658'</i>	.924	1.671*	1.124	1.116	.244	.872

\* refers to a difference in distributions that is significant at the 10% level (two-tail test)

<sup>1</sup> numbers in italics refer to differences of an unexpected sign.

Godfrey and Harvey) suggests that heteroscedasticity is not present. With only 10 decision rounds, tests for autoregressive conditional heteroscedasticity (ARCH) are not pursued. The results are provided in the first model of Table 2.

The coefficients on the multiple dummies need to be interpreted with care. The average contribution to the group exchange appears significantly lower in the own-money treatment when compared with the base-line of the first round of the first session of the first treatment. However, the large but opposite session effects in the two treatments result in the session and treatment/session cross effect also being highly significant. The large positive coefficient on the interaction dummy in particular is problematic. The interpretation that results at there is a significant negative treatment effect, except when it is the second session of the treatment! Finding the "net effect" of Treatment II apart from session effects is hampered by the positive correlation ( $\rho = .65$ ) that exists between the treatment and interaction dummies. To get a clean estimate of the treatment effect alone, the interaction dummy for treatment and session should ideally be orthogonal to both. Fortunately, the interaction dummy "Treatment I x Session 1 or Treatment II x Session 2" meets this requirement. With this interaction term, any effect of the

**Table 2**  
**A Test of Own-Money Effects Using OLS Regression**

	(1) <i>Contribution to Group Exchange<sub>1,t</sub></i> (0 - 80)			(2) <i>Contribution to Group Exchange<sub>1,t</sub></i> (0 - 80)		
	Coeff.	Std. Error	White Std. Error	Coeff.	Std. Error	White Std. Error
<i>Constant</i>	45.135	(3.883)***	(3.870)***	26.718	(3.883)***	(3.956)***
<i>Treatment II</i>	-24.210	(3.046)***	(2.862)***	-5.703	(2.154)***	(2.130)***
<i>Session 2</i>	-16.600	(3.046)***	(2.998)***	1.817	(2.154)	(2.130)
<i>Treat II x Sess 2</i>	36.833	(4.308)***	(4.261)***			
<i>Treat I x Sess 1 OR Treat II x Sess 2</i>				18.417	(2.154)***	(2.130)***
<i>Round 2</i>	4.383	(4.816)	(4.887)	4.383	(4.816)	(4.887)
<i>Round 3</i>	4.683	(4.816)	(4.937)	4.683	(4.816)	(4.937)
<i>Round 4</i>	.567	(4.816)	(4.914)	.567	(4.816)	(4.914)
<i>Round 5</i>	3.350	(4.816)	(5.005)	3.350	(4.816)	(5.005)
<i>Round 6</i>	-2.167	(4.816)	(4.807)	-2.167	(4.816)	(4.807)
<i>Round 7</i>	-7.950	(4.816)*	(4.867)	-7.950	(4.816)*	(4.867)
<i>Round 8</i>	-9.017	(4.816)*	(4.554)**	-9.017	(4.816)*	(4.554)**
<i>Round 9</i>	-9.133	(4.816)*	(4.731)*	-9.133	(4.816)*	(4.731)*
<i>Round 10</i>	-9.867	(4.816)**	(4.873)**	-9.867	(4.816)**	(4.873)**
R <sup>2</sup>	.156			.156		
N	600			600		

\*, \*\*, \*\*\* refer to significance at the 10%, 5% and 1% levels, respectively in two-tailed tests  
Run on Shazam 8.0

treatment dummy that remains should be a net treatment effect. The results of this regression are reported in the second model of Table 2.

The results of the second regression are less ambiguous. Not surprisingly, the interaction dummy is large and significant, picking up as it does the higher first session of Treatment I and second session of Treatment II. With this included, the coefficient remaining on Session is positive but insignificant. Of central interest, the coefficient remaining on Treatment II is small but statistically significant at the 1% level. Thus, controlling for session effects, there is evidence of a small house money effect.

## 5. Conclusions

This paper reports on the attempts to test for house money effects upon the voluntary provision of public goods in the familiar VCM framework. Previous empirical work outside experimental economics has suggested that people may exhibit a high marginal propensity to consume and greater risk-seeking behaviour with small, one-time windfall payments. The "investment account" commonly given to subjects at the start of VCM and other experiments may be thought of in these terms. Thus, it could be predicted that subjects in VCM experiments may use their investment accounts to purchase more non-pecuniary goods, such as reciprocal fairness, and pursue riskier contribution strategies than they would if using their pre-existing income. Yet it is the latter case that is of interest to private fund-raising agencies.

Two treatments of a standard VCM experiment were run. While both treatments asked all subjects to bring their own money to the experiment, the second required them to use it to fund their investment account. An un-announced participation fee was given to such subjects following their sessions to keep expected earnings constant across treatments. Some evidence of a moderate house money effect was found. Descriptively, average contributions were lower in 9 of 10 rounds.



A significant difference was not detected by a Mann-Whitney test in any but one round. However, significant intra-treatment variation between sessions lessened the likelihood that a treatment effect could be detected. An OLS regression that controlled for session effects and interaction between session and treatment found significant evidence of a small to moderate house money effect. Public good contributions were on average slightly lower when subjects had to use their own money, but they certainly did not collapse.

There are some very good reasons for supplying subjects with starting funds in experiments. The practice facilitates recruitment, avoids the appearance of gambling, and allows subjects to make decisions in the realm of losses without leaving the experiment in debt. Notwithstanding these benefits, experimentalists should learn more about the possible behavioural artifacts this practice can induce.

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