Decomposing Desert and Tangibility Effects in a Charitable Giving Experiment

David Reinstein and Gerhard Riener

Abstract

Several papers have documented that when subjects play with standard laboratory "endowments" they make less self-interested choices than when they use money they have either earned through a laboratory task or brought from outside the lab. In the context of a charitable giving experiment we decompose this into two common artifacts of the laboratory: the intangibility of money (or experimental currency units) promised on a computer screen relative to cash in hand, and the distinct treatment of random "windfall" gains relative to earned money. While both effects are found to be significant in non-parametric tests, the former effect, which has been neglected in previous studies, has a stronger impact on total donations, while the latter effect has a greater impact on the probability of donating. These results have clear implications for experimental design, and also suggest that the availability of more abstract payment methods may increase other-regarding behavior in the field.

1. Introduction

Several economists have found that when subjects play with standard laboratory "endowments" they make less self-interested choices than when they use money they have either "earned" through a laboratory task or brought from outside the lab (Cherry et al. 2002; Hoffman and Spitzer 1985; Loomes and Burrows 1994). This effect is typically interpreted as a result of Lockean desert effects (Rutstrom and Williams 2000; see Locke 1988, pp. 287-8.), fairness concerns (*a la* Rabin 1993), or a different mental accounting over windfall gains (Sheffrin and Thaler 1988, Thaler 1985, and Thaler and Johnson 1990). Our experiment does not differentiate between these models (this is left for future research); we will refer to the

net effect of these as the *windfall effect*.

There is ample evidence for windfall effects in the context of dictator games. Cherry et al. (2002) ran a series of dictator game experiments where, in the baseline treatment the dictator's endowment was randomly determined, while in their *earnings* treatment the endowment was based on performance in a cognitive task (solving GMAT questions), and this was common knowledge. Their *double blind with earnings* treatment modified the *earnings* treatment to increase subject-experimenter anonymity. Both their earnings and double-blind-earnings treatments lead to significantly less generous dictator behavior; in the latter treatment the dictators became almost entirely *hardnosed*, keeping nearly all of the money.

Oxoby and Spraggon (2008) compare dictator behavior in treatments adapted from Hoffman et al. (1996), comparing a case where the funds to be divided are "earned" by the dictator's quiz performance to a case where the potential recipient takes the quiz (to determine the amount available for the dictator to divide). They find that dictators are significantly more generous in the latter case. This suggests that fairness concerns are important, and that *relative* desert may be driving dictator decisions. Ruffle (1998), Mittone and Ploner (2006), and Cherry and Shogren (2008) find similar results on the importance of the receiver's effort.[1]

In contrast to the dictator environment, there is little evidence for windfall effects in voluntary contribution mechanism (henceforth VCM) experiments. Clark (2002) examines contribution rates in a VCM game. He finds no significant difference between contributions in the "own money" treatment, in which subjects are asked to bring $8 from outside the lab to purchase tokens, and the "house money" treatment, in which subjects are simply given the tokens.[2] However, as Clark's "own money" subjects are *also* given *house money* at the end

[1] In all of the experiments mentioned above the dictator subjects' "earnings" come from answering GMAT questions, some of which involve retailers' and consumers' decisions, dishonest job applicants, wealth, investments, money, and marketing. These may be triggering more self-interested behavior through a framing effect as in Vohs et al. (2006) and Cookson (2000), rather than simply increased legitimacy of the dictators' own endowments. However, the estimated relationships between the dictators' gifts and the *recipient's* performance, hence the observations of a fairness (or *relative* desert) motive is robust to this critique; the variation in the recipient's performance does not yield any additional framing effect of this sort.

[2] Still, Harrison (2007), who reanalyzed Clark's data to deal with the potentially non-independent error structure, suggests that a house money effect is present. However, the tangibility and earnings effects are not

of the experiment, they presumably have the same expected "windfall" earnings as the other subjects. Furthermore, Clark's subjects use tokens, and the earnings effect may be more salient when the rewards are more tangible. In related VCM experiments, neither Cherry et al. (2005) nor Kroll et al. (2007) find that subjects who earned their endowments (through answering GMAT questions) contribute less than those who did not.

Finally, there is some very recent evidence in the context of charitable giving itself. Carlsson et al. (2009) find windfall effects in a charitable giving experiment in both a laboratory and a field setting; subjects in both environments donate less when they have "earned" their pay by completing a survey.

This literature has ignored a second component of the bias that may limit the external validity of many laboratory results: people may treat money they are promised (or are given in the form of tokens) differently then cash they physically hold – we call this the *tangibility* effect We hypothesize three potential reasons why this may occur. First, psychology experiments demonstrate that subjects given "reminders of money" are both less helpful and less likely to ask for help in a variety of non-remunerated tasks (Vohs et al., 2006).In addition, Cookson (2000) finds that subjects subtly motivated with an "I" frame contribute less in a VCM setting than those motivated with a "we" frame, and Oberholzer-Gee and Eichenberger (2004) find that offering an unattractive lottery option to the choice set leads dictators to give less to other subjects.Second, using cash may cause subjects to more carefully consider the consumption they are sacrificing. Along similar lines, Oberholzer-Gee and Eichenberger (1999) argue that subjects do not fully consider the opportunity costs of the funds they give away in experiments, and Mazar et al. (2008) find that people cheat more when using exchangeable tokens then when they use cash. Finally, parting with cash may itself bring some disutility, perhaps through an attachment to this money similar to the "endowment effect" of Kahneman et al. (1991). For all of these reasons, we might therefore expect that subjects holding cash will be less likely to contribute this to a public good or a charitable cause.

separable in this context.

To the best of our knowledge, there is no economic evidence on the effect of the medium of exchange on generosity. However, at least one prominent experiment varies this in conjunction with other variations in the treatment, leading to a potential confound.[3] In the present paper we implement a real charitable giving experiment to provide the first salient economic evidence that the tangibility of the choice medium affects other-regarding decisions.

2. Experimental design

We use a charitable giving experiment with a 2×2 design to differentiate two distinct artifacts of laboratory endowments. Firstly, the treatments vary according to the extent to which subjects should see the money as *earned*; we compare giving behavior after compensation based on performance on a five minute task to behavior with a randomly assigned payment. The second dimension of variation involves the tangibility of the payment: we either give cash to the subjects *before* they decide how much to donate (and they physically place any donations they make into envelopes) or they allocate their donation from an endowment on the computer screen and they are paid cash at the end of the experiment. Thus, we separately test whether *earning the money* and having *cash in hand* affect giving behavior in the lab.[4]

Unlike many of the experiments previously mentioned, our subjects make decisions over donations to charitable foundations – institutions outside the laboratory. In line with Eckel and Grossman (1996), we see this as a more obvious and typical expression of other-regarding behavior than donations to a laboratory public good or towards another laboratory subject. Our environment also provides a more demanding test for tangibility and windfall effects. In the real world it is rare to be asked for a gift from a random non-needy stranger (or to receive such a gift); hence, it is not surprising that standard dictator games should be sensitive to framing effects. On the other hand charitable appeals and charitable giving are regular-

[3] E.g., in Hoffman et al. (1996) the "single blind 2" treatment combines both a decreased social distance from the experimenter (relative to "single blind 1") and "a decision form for making the decision, instead of money".

[4] In addition to the treatments mentioned above, we also vary the choice set. As a robustness check, we offer three charities instead of two in the *expanded choice set* treatment. This allows us to demonstrate that our results are not sensitive to a variation in the choice set.

ities, so subjects will have more experience with such decisions and their decisions should be less easily perturbed. While dictator giving to other subjects is highly sensitive to the level of social isolation, falling to very low levels in "double-blind" environments Hoffman et al. (1996)), charitable giving persists at significant levels even under highly anonymous conditions (Eckel and Grossman, 1996). Our setting may also better isolate the effect of asset legitimacy: intuition suggests that in the charitable giving context, subjects will focus less on their desert *relative* to the recipient(s) then they would in deciding how much to give to a fellow subject.

All treatments are assigned orthogonally; we have a (nearly) fully balanced design.[5] By construction, the distribution of initial endowments is the same for each treatment. Finally, all of our treatments involve the same strong level of anonymity.[6] The time spent in each treatment of our experiment is approximately the same, so subjects in each of our treatments should have the same earnings expectations.

The sessions were run at the University of Jena Experimental Economics lab using the standard subject pool. In total 190 subjects participated in the experiments of which 54.2% were female.[7] The sessions were conducted in October 2008 (39 subjects), February 2009 (79 subjects), and September 2009 (72 subjects). While we ran each of the four payment regime treatments in a separate session, the participants were from the same subject pool and the times and dates of the experiment were stratified by treatment.[8] To avoid mixing payment types, we did not give subjects any pre-experiment "show-up fee."

[5]Because the treatments were run in separate sessions and their were some no-shows, the actual observations are very slightly off-balance, and the endowments are not precisely identically distributed by treatment, nor is the "choice set" treatment. However, these slight differences are controlled for in our multivariate regressions and in our balanced bootstrapped rank-sum tests. The lack of balance does not measurably affect any of our results. Our treatments are also not perfectly balanced over time. To test for session-specific effects, in online Appendix 5 Table 5 we also report regressions with standard errors clustered by session, and controls for time-of-day and date of session effects; our results are robust to all of these, and none of these are significant.

[6]See the protocol in online Appendix 1 for a full description of our careful procedure to insure subject-experimenter and subject-subject anonymity.

[7]We did not collect extensive demographics on our subjects in order to preserve subject-experimenter anonymity.

[8]Appendix 5, Table 4 illustrates this balance, and shows our results are insensitive to the time and date of the session.

To guarantee anonymity, the lab was divided into an outer partition - which serves as a meeting room before the experiment and as a room for the administrators during the experiment - and an inner partition with computer terminals on which the subjects make decisions and answer questions. These were separated so that it was impossible to see the inner partition from the the outer partition and vice-versa. For administrative purposes, a volunteer from the participants helped with the procedures whenever communication between the inner and the outer part of the lab was necessary. Furthermore, to ensure our credibility, this volunteer supervised the online donations made by the experimenters after the other participants had been dismissed.

At the beginning of the experiment all subjects were assured that we would not be able to connect their name to the decisions they made. Next we asked for a volunteer to help us with administrative issues, mainly allocating the sealed envelopes with payouts at the end of the experiment.

The task. Subjects in the performance treatments (PA and PC, described below) were told that their endowment would depend on their performance on a simple task. They were asked to add up five two-digit numbers[9] using only scratch paper and a pencil. The numbers were randomly drawn and presented to the subjects as in the example below:

12	77	34	55	62	__

The participants were given five minutes to solve as many tasks as possible. We argue that this task was sufficiently tedious to make subjects feel that they *earned* the money recieved. This task, although numerical, is less likely to cue self-interested "economic" thinking than the GMAT questions used in many previous studies.

The charitable giving stage (donation decision). The subjects were not given any indication that this experiment would involve an opportunity for charitable giving until they reached the

[9]This task has been used in various occasions for testing competitiveness (e.g., Niederle and Vesterlund, 2007).

"charitable giving stage". This stage was a one-shot dictator game in which subjects could donate none, some, or all of their endowment to any combination of the available charities in units of 50 Euro cents. All subjects were presented with *Brot für die Welt* (BfdW) – "Bread for the World", a German development aid agency and the World Wild Life Fund for Nature (WWF), a nature conservancy charity. For the *expanded choice set* treatment we also included *Deutsches Rotes Kreuz* (DRK) - the German Red Cross - which operates in similar areas as *BfdW*. Subjects were given information about each of the charities on the computer screen and next had to decide how much (if anything) to donate to each available charity and enter this into the computer.[10] By using multiple charities we reduced the noise surrounding heterogeneous tastes for charities, and gained more useful data on a wider range of subjects.

Treatment 1: Performance / (on-screen) Account (PA). Subjects in the performance treatments were told that the probability of higher earnings increased in the number of tasks correctly completed, but we did not specify exactly how performance translated into payoffs.[11] After completing the task they were told how much this earned them. In *account* treatments they were endowed €5, €7.50 or €10 (shown on their computer screen) but were not yet given cash. They next made their donation decisions. At the end of the experiment they were (anonymously) given envelopes containing their earnings minus their total donations.

Treatment 2: Performance / Cash (PC). As in *PA*, subjects first completed the task and learned how much they earned. However, unlike in the *account* treatments, subjects in *cash* treatments were paid in cash *before* they made giving decisions. After the task stage, the volunteer was prompted to come outside and bring the numbered envelopes containing the

[10]The order of the presentation of the charities, both on the description screens and on the actual donation screen are stratified over subjects, in order to balance any potential order effects.

[11]We did not tell them that their pay was based on *relative* performance. They were instead told that "the more sums you solve, the more likely it is that you will get a higher payment" (in German: "*Je mehr Aufgaben Sie lösen desto wahrscheinlicher ist es, dass sie mehr verdienen*."), because we did not want them to compare themselves to other subjects in making their charitable contributions. Such a comparison might have lead them to believe that that subjects who earned more had a greater obligation to donate. In the treatments of October 2008, the subject who solved the most sums received €10, the second €7.50 and the rest of the subjects in the same session got €5. In the sessions conducted in February, March, and September 2009, the participants who were in the upper tercile of solved tasks received €10, in the middle tercile €7.50 and in the lower tercile €5.

cash earnings into the inner part , where they hand each subject the envelope with his or her subject number on it. The payment envelopes were carefully assembled to look identical and have similar weights.[12] Subjects were instructed to inspect and count the money in private at their computer desks. Next, they made their donation decision(s) by entering these choices on the computer screen. Finally, subjects were asked to put the chosen contributions (in cash) into the donation envelope and seal it.

Treatment 3: Random / on screen Account (RA). In this treatment, subjects were endowed €5, €7.50 or €10 randomly on their computer screen. The donation stage followed, and payments were distributed as in *PA*.

Treatment 4: Random / Cash (RC). In the *RC* treatment the endowments were randomly determined (as in *RA*), and given to the subjects in identical envelopes as in *PC*. The donation and payments procedure also followed *PC*.

3. Results

3.1. Summary Statistics

Table 1 compares the proportion of the endowments donated to any of the two (or three) charities, pooling across choice set treatments.[13] Subjects donated significantly less[14] when they were paid in cash than when their endowment was only shown on the computer screen (13% versus 23% of the total funds, pooling across all other treatments).[15]

Figure 1 shows the cumulative distribution of the share of earnings donated over the earnings and payment treatments. The distribution of contributions under on-screen entitlements

[12] We did this by using coins of different increments. To the extent that small coins are less desirable then bills this would lead to a bias *against* our finding of a tangibility effect. Since payments in performance and random treatments had the same distribution, this should not impact our "earnings effect" findings.

[13] We performed robustness checks and found no significant differences in contribution behavior between the two and three charity choice set treatments. Details available by request.

[14] These differences are significant in Wilcoxon rank-sum tests, as well as in familiar parametric tests (available by request). Because of the aforementioned lack of balance (stemming from no-shows), we also report bootstrapped rank-sum tests in brackets, with each of the 1000 random draws (with replacement) exactly balanced by payment treatment, earnings treatment, choice set treatment, and stake size.

[15] This rate of giving is fairly consistent with results of previous experiments. E.g., in Eckel and Grossman (1996) subjects give 30% of their $10 cash endowment (they were also given a $5 show-up fee).

Table 1: Average proportion contributed by payment regimes

	Payment			
Endowment	*Random*	*Performance*	**Total**	**N**
Account	0.27	0.18	0.23	99
Cash	0.14	0.12	0.13	91
Total	0.21	0.15	0.18	
N	102	88		190

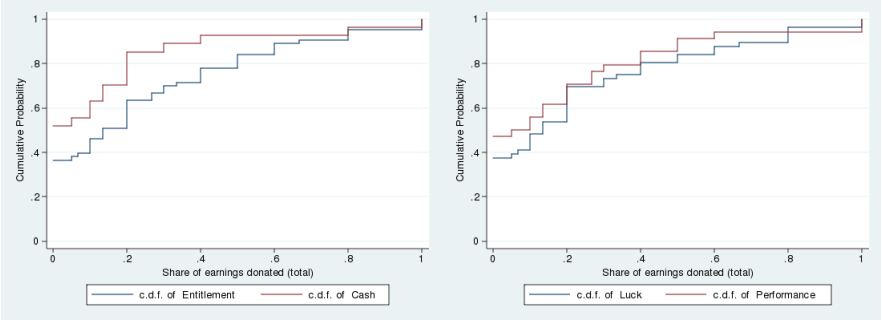
Wilcoxon rank sum tests		
P(Account> Cash)	0.57+	(0.06); [0.05]
P(Random> Performance)	0.58*	(0.05); [0.03]
P(Account/Random > Cash/Performance)	0.64**	(0.01); [0.00]
P(Account/Random > Cash/Random)	0.61+	(0.06); [0.05]
P(Account/Performance > Cash/Performance)	0.53	(0.59); [0.57]
P(Account/Performance > Cash/Random)	0.49	(0.86); [0.85]

p-values for simple rank sum tests in parentheses, +: p< 0.10, *: p<0.05, **: p<0.01

In square brackets: p-values for bootstrapped rank sum tests, 1000 draws, balanced by all treatments and stake sizes.

(RA and PA) stochastically dominates the distribution under cash payments (RC and PC). Similarly, the distribution under random payments (RA and RC) stochastically dominates the distribution under performance-based earnings (PA and PC).

Figure 1: Cumulative distribution functions of share of earnings donated



On the other hand, as Table 2 demonstrates, the performance treatment has a stronger extensive margin effect; subjects are significantly less likely to donate at all if they have earned their endowment through their performance. To give an intuitive spin, some people may feel more comfortable keeping all of their money if they think they have earned it and thus deserve it, while those who do feel compelled to donate find that giving away cash feels

Table 2: Number of subjects who donated by treatment

	Payment		**Endowment**	
	Random	*Performance*	*Account*	*Cash*
Donated	N *(column %)*	N *(col. %)*	N *(col. %)*	N *(col. %)*
No	37 *(36%)*	44 *(50%)*	42 *(42%)*	39 *(43%)*
Yes	65 *(64%)*	44 *(50%)*	57 *(58%)*	52 *(57%)*
Total number	102	88	99	91
p-values of tests				
Pearson χ^2	0.06		0.95	
Fisher's exact	0.07		1.00	

more "costly" in terms of sacrificed consumption than giving away money on a computer screen. This result is confirmed by our Probit regressions with controls for endowment size, gender, and the larger choice set (see Appendix 4).

3.2. Multivariate Analysis

To control for observable (random) differences in treatment assignment we regress total donations on controls for observable heterogeneity and treatment interactions.[16] These regressions (Table 3) again suggest that cash treatments reduced generosity. The effect of cash is negative and significant in the Account treatments, and negative but insignificant in Performance treatments (summed coefficient: *Cash*+*cash* $\times$ *perform*).[17] Donations were also lower when subjects were paid according to their performance, but this effect was not statistically significant here. The coefficients on higher stake sizes (€7.5 or €10) are small and not significant: subjects who earn more do not tend to donate more. In line with some previous work, (e.g., Eckel and Grossman, 1998, List, 2004) women donated more than men. Total donations were not significantly different when a third charity was included. The interaction

[16]We use a standard OLS specification for familiarity and comparability reasons. We also use a Poisson specification, both because our data resembles count data (in increments of 50 cents) and because this specification deals with corner-solution (non-negative) data without being as sensitive to non-normality and heteroskedasticity as a standard Tobit regression Gourieroux et al. (1984); Arabmazar and Schmidt (1981). In online Appendix 5 (table 5) we find similar results using a fractional regression specification. The cash and performance results are similar in zero-inflated Poisson regressions (available by request).

[17]The "Cash" dummy is also strongly significant in a univariate linear (-0.66; p-value: <0.01) or Poisson regression (-0.66, marginal effect; p-value: <0.01) (details by request).

effects are not significant, although their positive sign and magnitude suggest that the treatments have a sub-additive effect – the summed coefficient (*Cash+perform+cash* × *perform*) representing the effect of cash and performance combined is very close to the coefficient on cash alone.

Table 3: Poisson and OLS regression of total donations

			Add. contr.		Gender contr.	
	(1)	(2)	(3)	(4)	(5)	(6)
	Psn.	OLS	Psn.	OLS	Psn.	OLS
Pay cash	-0.84*	-0.84*	-0.68*	-0.84*	-0.88*	-0.89**
	(0.33)	(0.33)	(0.31)	(0.34)	(0.38)	(0.34)
Pay by performance	-0.54	-0.54	-0.44	-0.54	-0.56	-0.58
	(0.39)	(0.39)	(0.33)	(0.40)	(0.42)	(0.41)
Cash × performance	0.51	0.44	0.41	0.44	0.69	0.56
	(1.02)	(0.49)	(0.82)	(0.50)	(1.14)	(0.52)
Third charity			0.34	0.26	0.42	0.26
			(0.33)	(0.25)	(0.40)	(0.24)
Stake: 7.5			-0.18	-0.13	-0.14	-0.08
			(0.36)	(0.29)	(0.44)	(0.28)
Stake: 10			0.10	0.093	0.10	0.10
			(0.37)	(0.32)	(0.44)	(0.31)
Female					0.63*	0.53*
					(0.32)	(0.25)
Combined coefficients (sums raw coefficients, not marginal effects)						
Cash+perform+cash × perform	-0.76*	-0.94**	-0.76*	-0.94**	-0.73*	-0.92**
	(0.32)	(0.37)	(0.32)	(0.37)	(0.31)	(0.36)
Cash+cash × perform	-0.39	-0.40	-0.39	-0.40	-0.35	-0.34
	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.38)
Observations	190	190	190	190	190	190
R^2		0.048		0.056		0.079
Pseudo R^2	0.033		0.039		0.055	

Heteroskedasticity-robust standard errors in parentheses.

+ p<0.10, * p<0.05, ** p<0.01 for tests using heteroskedasticity-robust standard errors (for all columns)

All regressors are dichotomous (0,1) variables, dy/dx for discrete change of dummy variable reported.

Marginal effects evaluated at Account/Random, Female, Stake = 7.5, two charity choice set.

As we show in online Appendix 5, table 3, the tangibility and windfall effects on donations are similar across charities, and our results also hold for a fractional response (Papke and Wooldridge, 1996) regression of "share donated".

4. Conclusion

Our experiment is the first to document the tangibility effect; its magnitude appears at least as strong as the windfall effect, although the latter has a stronger effect at the extensive margin. Furthermore, by using a charitable giving context and a relatively neutral real-effort task, we add to the evidence that the legitimacy (*absolute* desert) of experimental subjects' *own* assets affects their other-regarding behavior.

Our findings do not imply that experimenters should *always* use "tangible" cash. In the context of our experiment, we cannot say which contribution level is more externally valid. Whether the differences are because seeing money cues self-interest, because cash causes a more careful consideration of trade-offs, or because parting with cash is more painful, either frame (cash or endowment) may have external validity.[18] In the field many decisions are made without physical cash, as credit cards and electronic payments have become dominant in many markets. However, researchers must be *aware* of this framing effect and take it into account. This distinction is important: economic experiments vary greatly along both dimensions, often simultaneously. As noted in Hoffman et al. (1994), comparisons *between* experimental results must take into account differences in the decision medium.[19] To the extent that future income is less tangible than present income, our results agree with Breman Breman (2006), who offers field experimental evidence that people are more generous with the former than with the latter. Similarly, our result that the payment instrument matters may also be generalizable to real-world decision making, particularly over intangible "warm-glow" goods such as charitable donations.

[18] On the one hand, cash is obviously better if it leads to greater experimenter credibility. However, this is unlikely to have been a driver of our results, as all of our subjects had previously participated in economic experiments. On the other hand, cash may lead to other extraneous effects; e.g., it may cue subjects to consider the nuisance carrying around earnings that include "small change".

[19] In comparing their dictator results to those of Forsythe et al. (1994), Hoffman et al note that "other aspects of the double blind procedures require experimental examination to identify what is driving the outcome; an envelope containing the cash might be an important factor." Our analysis confirms this speculation.

References

Arabmazar, A. and P. Schmidt (1981). Further evidence on the robustness of the Tobit estimator to heteroskedasticity. *Journal of Econometrics 17*(2), 253–258.

Breman, A. (2006). Give more tomorrow: Evidence from a randomized field experiment. *Unpublished manuscript*.

Carlsson, F., H. He, and P. Martinsson (2009). Easy come, easy go-The role of windfall money in lab and field experiments. *rapport nr.: Working Papers in Economics 374*.

Cherry, T., P. Frykblom, and J. Shogren (2002). Hardnose the Dictator. *American Economic Review 92*(4), 1218–1221.

Cherry, T., S. Kroll, and J. Shogren (2005). The impact of endowment heterogeneity and origin on public good contributions: Evidence from the lab. *Journal of Economic Behavior and Organization 57*(3), 357–365.

Cherry, T. and J. Shogren (2008). Self-interest, sympathy and the origin of endowments. *Economics Letters 101*(1), 69–72.

Clark, J. (2002). House Money Effects in Public Good Experiments. *Experimental Economics 5*(3), 223–231.

Cookson, R. (2000). Framing effects in public goods experiments. *Experimental Economics 3*(1), 55–79.

Eckel, C. and P. Grossman (1996). Altruism in anonymous dictator games. *Games and Economic Behavior 16*(2), 181–191.

Eckel, C. and P. Grossman (1998). Are women less selfish than men?: Evidence from dictator experiments. *The Economic Journal 108*(448), 726–735.

Forsythe, R., J. Horowitz, N. Savin, and M. Sefton (1994). Fairness in simple bargaining games. *Games and Economic Behavior 6*(3), 347–69.

Gourieroux, C., A. Monfort, and A. Trognon (1984). Pseudo maximum likelihood methods: applications to Poisson models. *Econometrica 52*(3), 701–720.

Harrison, G. (2007, December). House money effects in public good experiments: Comment. *Experimental Economics 10*(4), 429–437.

Hoffman, E., K. McCabe, K. Shachat, and V. Smith (1994). Preferences, property rights, and anonymity in bargaining games. *Games and Economic Behavior 7*(3), 346–380.

Hoffman, E., K. McCabe, and V. Smith (1996). Social Distance and Other-Regarding Behavior in Dictator Games. *American Economic Review 86*, 653–660.

Hoffman, E. and M. Spitzer (1985). Entitlements, rights, and fairness. *Journal of Legal Studies 14*(2), 259–297.

Kahneman, D., J. Knetsch, and R. Thaler (1991). Anomalies: The endowment effect, loss aversion, and status quo bias. *The Journal of Economic Perspectives 5*(1), 193–206.

Kroll, S., T. Cherry, and J. Shogren (2007). The impact of endowment heterogeneity and origin on contributions in best-shot public good games. *Experimental Economics 10*(4), 411–428.

List, J. (2004). Young, Selfish and Male: Field evidence of social preferences*. *The Economic Journal 114*(492), 121–149.

Locke, J. (1988). *Two Treatises of Government*. Cambridge: Cambridge University Press.

Loomes, G. and P. Burrows (1994). The impact of fairness on bargaining behaviour. *Empirical Economics 19*, 201–221.

Mazar, N., O. Amir, and D. Ariely (2008). The Dishonesty of Honest People: A Theory of Self-Concept Maintenance. *Journal of Marketing Research 45*(6), 633 – 644.

Mittone, L. and M. Ploner (2006). Is it just legitimacy of endowments? an experimental analysis of unilateral giving. *CEEL Working Paper N. 02/2006*.

Niederle, M. and L. Vesterlund (2007). Do women shy away from competition? do men compete too much? *Quarterly Journal of Economics 122*(3), 1067–1101.

Oberholzer-Gee, F. and R. Eichenberger (1999, May). Focus effects in dictator game experiments.

Oberholzer-Gee, F. and R. Eichenberger (2004). Fairness in extended dictator game experiments. *Manuscript*.

Oxoby, R. and J. Spraggon (2008). Mine and yours: Property rights in dictator games. *Journal of Economic Behavior and Organisation 65*, 703–713.

Papke, L. and J. Wooldridge (1996). Econometric methods for fractional response variables with an application to 401 (k) plan participation rates. *Journal of Applied Econometrics 11*(6), 619–632.

Rabin, M. (1993). Incorporating Fairness into Game Theory and Economics. *American Economic Review 83*(5), 1281–1302.

Ruffle, B. (1998). More is better, but fair is fair: Tipping in dictator and ultimatum games. *Games and Economic Behavior 23*(2), 247–265.

Rutstrom, E. and M. Williams (2000). Entitlements and fairness: an experimental study of distributive preferences. *Journal of Economic Behavior and Organization 43*(1), 75–89.

Sheffrin, H. and R. Thaler (1988). The behavioral life-cycle hypothesis. *Economic Inquiry 26*, 609–643.

Thaler, R. (1985). Mental accounting and consumer choice. *Marketing Science 4*, 199–214.

Thaler, R. and E. Johnson (1990). Gambling with the house money and trying to break even: The effects of prior outcomes on risky choice. *Management Science 36*, 643–660.

Vohs, K., N. Mead, and M. Goode (2006). The psychological consequences of money. *Science 314*, 1154–1156.