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Intentions-Based Reciprocity to Monetary and Non-Monetary Gifts

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Abstract: Social preference models emphasize that perceived intentions motivate reciprocity. However, laboratory tests of this theory typically manipulate perceived intentions through changes in wealth resulting from a sacrifice in pay by another. There is little evidence on whether reciprocity occurs in response to perceived intentions alone, independent of concurrent changes in pay and giver sacrifice (and any associated guilt from that sacrifice). This paper addresses this gap in the literature by implementing a modified dictator game where gifts to dictators are possible, but where gift transactions are also stochastically prevented by nature. This leads to instances of observed gift-giving intentions that yield no sacrifice or change in outcomes. In addition, this study uses both monetary and non-monetary gifts; previous studies typically use only monetary incentives, even though real-world applications of this literature often involve non-monetary incentives such as business or marketing gifts. The results show that on average, dictators reciprocated strongly to just the intention to give a gift, and they also reciprocated similarly to both monetary and non-monetary gifts. These results are consistent with intentions-based models of social preferences and with much of the marketing literature on business gifts.

Keywords: reciprocity; intentions; gift exchange; social norms; business gifts

1. Introduction

Social preference models propose that perceived intentions are a key driver of reciprocity [1,2]. However, economics experiments have typically tested this by varying whether a change in outcome is attributed to another individual or to chance [3–5]. In addition, in these games and in the related gift-exchange game [6–8], the initial giver must sacrifice from their own resources in order to show kindness and induce reciprocation. However, this sacrifice and change in outcome could induce guilt and lead to reciprocity via guilt-driven channels [9–11] instead of via solely an intentions mechanism. To better distinguish between these alternatives, this study tests for the effects of intentions in the absence of concurrent sacrifice and changes in outcomes.

In addition, existing studies have generally used monetary incentives [3–5,12], even though many real-world applications of reciprocity theory involve non-monetary incentives such as business or marketing gifts. The only gift exchange experiment to include non-monetary gifts found that individuals may perceive and respond to a non-monetary gift differently than an increase in wages [13]. More evidence is therefore needed to better understand whether results from experiments that use monetary incentives can extend to non-monetary applications, such as when firms or charities give gifts in hopes of encouraging reciprocal sales or donations.

This study addresses these gaps in the literature by implementing a modified dictator game. Non-dictator subjects will be allowed to give either monetary or non-monetary gifts to their dictator partner. However, nature will stochastically prevent these gift transactions from being completed after the gift offer is announced (this will be referred to as an overturned gift for simplicity). If an overturned

gift generates similar reciprocity as an actual gift transfer (after controlling for wealth distribution preferences), this would imply that perceived intentions can induce reciprocity independent of changes in outcome, giver sacrifice, and any guilt associated with that sacrifice.

One could argue that gifts given in this context might not be perceived as kind, since the gift-giver has an ulterior motive [14–16]. Nevertheless, the ulterior motive is constant throughout the experiment and thus should not confound comparisons of reciprocity between successful gifts and overturned gifts. This study therefore tests whether intentions to give a gift can lead to positive reciprocity even when ulterior motives for the gift are present. This is not unlike reciprocity in many real-world applications, such as business gifts from salespeople, where the ulterior motive is clear and yet consumers still reciprocate positively (and even report increased liking of the giver [17]). Moreover, a lab experiment demonstrated that gift recipients may fail to properly draw inferences about the mental state (i.e., the motives) of a gift-giver [18]. Therefore, there is evidence that the presence of an ulterior motive does not necessarily preclude individuals from inferring positive intentions from a gift.[1]

By using both monetary and non-monetary gifts, this study can build a closer tie between the experimental game theory literature and field experiments in economics and marketing. Field studies have shown that non-monetary gifts can increase firm sales [17,19] and donations to charity [20], but these studies did not implement treatments that identify the exact mechanism behind the observed reciprocity. Although this study lacks the natural context of these field experiments, it is better able to isolate these different mechanisms via direct treatment manipulations, while the use of non-monetary gifts and the presence of an ulterior motive still brings the experiment a step closer to these real-world applications.

This study is loosely related to recent publications on reciprocity in bribery contexts. Pan and Xiao [21] use a three-person bribery paradigm and find that a monetary gift can lead to greater reciprocity than just the intention to give the same gift. However, their bribery game (like some other bribery-inspired games, e.g., Malmendier and Schmidt [22]) only tests for a different form of reciprocity where reciprocation reduces welfare for a third party and not the reciprocator. This is different from my experiment and some of the real-world scenarios it is motivated by, such as marketing gifts to a prospective customer or donor. In addition, intentions in their bribery game still result in sacrifices in pay by the giver even when the bribe is not given. Furthermore, other changes in outcomes often occur at the same time (e.g., a third player successfully giving a gift to the same receiver), leading to competing motives. Thus, the contexts and hypotheses tested in this bribery literature are different from the ones in this study.

This paper is more closely related to experiments that introduce noise into trust games in order to reduce a subject's ability to infer intent from an observed outcome [23,24]. However, these studies do not measure reciprocity to an unambiguous and certain signal of intentions that is unaccompanied by giver sacrifice. Instead, they manipulate perceived intentions by introducing uncertainty and beliefs into the calculations. The only instance where intentions could be signaled clearly required a sacrifice in pay by the giver [24]. The results from this literature therefore imply that perceived intentions likely matter for reciprocity, but only in a context where uncertainty, beliefs, and/or giver sacrifice must also be accounted for (and only for monetary gifts). My experiment will directly measure the effects of intentions independent of both giver sacrifice and uncertainty.

Two papers have similarly introduced noise into a gift-exchange game [8,25]. However, these studies introduced noise in how the reciprocator could respond (i.e., noise in the worker's effort in the gift-exchange game), and they were not designed to measure reciprocity to intentions independent of changes in outcome. Thus, these studies are testing a different set of hypotheses than this experiment.

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[1] Some subjects in this experiment also self-reported inferring kind intentions from a gift, as detailed in the results section.

2. Experimental Design and Procedure

2.1. Gift Items

This experiment used three gift items, all of which were represented in the experiment via images on subject computers. The first, a small-value item, was a pen with a market value of approximately $2. The second, a large-value item, was a duffel bag with a market value of $12. Both of these items are commonly used as marketing gifts by firms and non-profits (in fact, these exact items were selected and purchased from a marketing products catalog).[2] The third item, similar to monetary-based payoffs in other games, was a cash gift of $2, represented as an image of two one-dollar bills. Whenever a subject received an item, a picture of the item was shown. Figure 1 displays the exact pictures that were shown to all subjects.

Figure 1. The item pictures that subjects saw during the experiment. Pictures are of the actual item they would receive. (**a**) A pen; (**b**) A duffel bag; (**c**) $2 in cash.

2.2. Experimental Session Overview

Figure 2 provides an overview of all of the tasks involved in each experimental session. Each of these tasks will be described in turn.

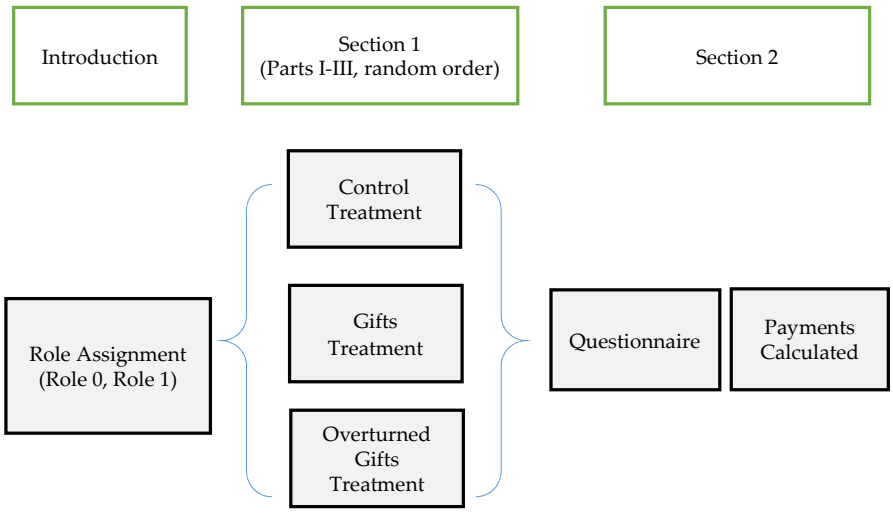

Figure 2. Overview of experimental sessions.

2.3. Dictator Game Procedure

After seating subjects at computers and giving a standard welcome message, subjects were randomly assigned (by zTree, [26]) to one of two roles for the experiment: role 0 or role 1. These corresponded to the dictator and their partner (henceforth referred to as the gift-giver role), although

[2] Since these gift items are predetermined, the exact items in this study are not chosen by the gift-giver and thus reciprocity due to thoughtfulness or consideration in gift choice is theoretically irrelevant in this context. This is a different context from some experiments in the literature [13], and this experiment should be thought of as testing some but obviously not all of the possible motives for reciprocity to non-monetary gifts in the field.

they were only referred to as role 0 and role 1 during the session. Subjects were told that for each task they performed, the computer would randomly and anonymously pair them with a subject of the other role. Subjects were informed that they would retain their role throughout the experiment.

This was followed by Parts I–III, which each corresponded to one of three treatments. The order in which subjects played each of these treatments was randomized, but every subject played all three. Similar within-subjects designs have been used in other modified dictator games [27], and this setup enables more precise measures of reciprocity via subject random effects estimation [28]. Instructions for Parts I–III were printed on handouts for subjects (see Appendix B) and were read to them before each part began.

One of these three parts, the gift treatment, was designed to measure reciprocity to gifts. To begin, the gift-giver was endowed with an item with three-fourths probability. If a gift was endowed, a picture of the item would appear on the gift-giver's screen, but the dictator would not know whether their partner had an item. The gift-giver could choose whether to give the item to the dictator or keep the item for themselves (see Figure 3a). If they selected yes, the dictator was told that their partner had given them an item, and a picture of the item would appear on the dictator's screen (Figure 3b). If the item was not given, the dictator would be told that they did not receive an item from their partner (Figure 3c), and they would be reminded that their partner may not have had an item to give.[3] The dictator then chose an allocation out of a $10 pie. While the dictator made this choice, their partner was asked how much they expected to be given (but this was not reported to the dictator). This was done to elicit some information about beliefs without risking the possibility of influencing dictator behavior.[4] Gift-givers were not informed what their partner allocated that round. Subjects repeated this task eight times, each with a new, random, and anonymous partner. Subjects were told that at the end of the experiment, one of these eight rounds would be selected for payment.

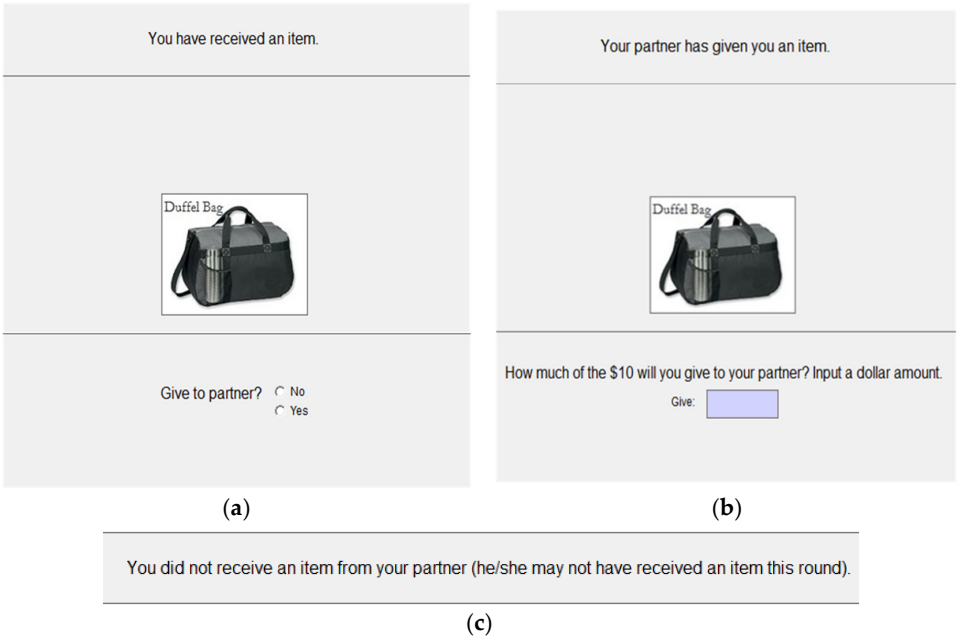

Figure 3. Gift treatment screenshots. (**a**) Gift-giver's screen with item; (**b**) Dictator's screen with item; (**c**) Message to dictator when no gift was given.

[3] This reminder, as well as the possibility of a no-item round, were included to reduce the likelihood that dictators would punish partners when they did not receive gifts.

[4] Soliciting dictator beliefs on partner-expectations prior to allocation would likely have biased the allocation decision. Soliciting dictator beliefs after an allocation would likely have yielded biased responses on partner-expectations (while possibly also influencing dictator allocations in subsequent rounds).

Another of the three parts, the overturned gift treatment, was designed to measure reciprocity to intentions alone. To begin, the gift-giver was again endowed with an item with three-fourths probability, and they again chose whether to give this item to their partner. However, if they gave the item to their partner, nature prevented the gift exchange with one-half probability. When this occurred, it was announced to both players (see Figure 4).[5] Dictators then chose how much of a $10 pie to allocate to their partner. Thus, dictators at times made allocation decisions knowing that their partner had attempted to give them an item but were prevented from doing so by nature. While dictators made their choice, gift-givers were again asked what they expected to be given. As before, gift-givers received no feedback on what their partner allocated in that round. Subjects repeated this task for eight rounds, each time with a new, random, and anonymous partner. Subjects were told that one of these eight rounds would be selected for payment.

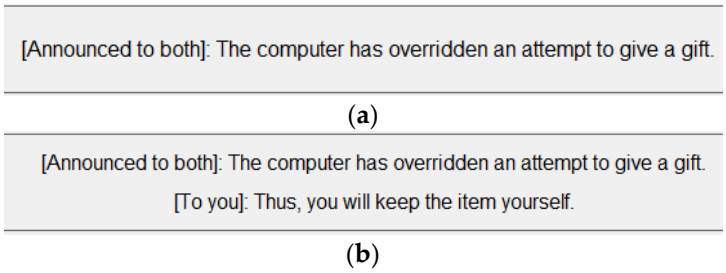

Figure 4. Overturned gift messages. (**a**) Message to dictator; (**b**) Message to gift-giver.

Another of the three parts was the control treatment that did not include any gift-giving by subjects. This treatment was designed to control for preferences over total wealth distribution. In this treatment, one of the two players was endowed with an item with three-fourths probability. If the dictator was the one endowed with an item, their partner was then informed exactly what item the dictator received (see Figure 5). This is identical in wealth and information to the gift treatment when an item is given to the dictator by the gift-giver.[6] This measures how much the dictator allocates simply due to this wealth distribution between players. Without this counterfactual, reciprocity would be confounded with preferences over wealth distribution [29–34]. As before, dictators were then asked to make their allocation, while gift-givers were asked what they expected to receive. Gift-givers were not told what their partner allocated in that round. Subjects repeated this task for eight rounds, each time with a new, random, and anonymous partner. Subjects were told that one of these eight rounds would be selected for payment.

Parts I–III consisted of these three treatments in random order, and at the end of the experiment, one round from each treatment (three rounds in total) were selected for payment. More detailed explanations of session implementation are included in Appendix C to aid replication purposes.

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[5] Importantly, dictators were not told the item type that was overturned. This design was chosen because displaying the overturned item would make the overturned gift screen appear too similar to the screen for actual gift transfers, potentially causing confusion. The implemented design ensures that the intend-to-give message is visually distinct from actual gift transfers beyond just a simple line of text. In addition, splitting overturned gifts by item type would have made it difficult to obtain sufficient overturned-gift observations for some items. This design naturally restricts some of the analysis, but it still allows estimation of reciprocity to just the intent to give a gift. As the results will demonstrate, there is still a strong response to intentions in this context.

[6] In some control treatment rounds, the gift-giver is the one endowed with an item. In these rounds, the dictator does not know whether their partner has an item. This is identical in wealth and information to when no gift is given. In rounds with an overturned gift, the dictator knows their partner has an item, so information is not identical to this scenario; however, when comparing an overturned gift round with this control round, this difference in information would only bias against finding a positive effect of intentions on allocations.

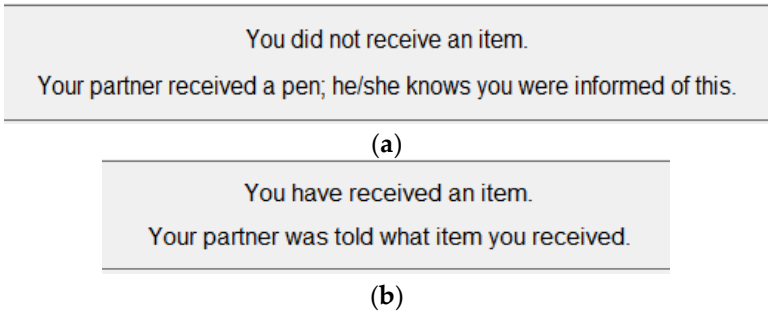

Figure 5. Control treatment messages. (**a**) Message to gift-giver about dictator's item; (**b**) Message to dictator about what their partner knows.

2.4. Questionnaire

After the three treatments but before being informed of pay outcomes, subjects filled out a questionnaire. This elicited basic demographics such as age, gender, and race, as well as a scale of Machiavellianism [35]. Subjects were also asked to guess the value of the pen and duffel bag, to rate the desirability of each on a scale of 1–5, and to explain what likely motivated each player's actions in the game. Finally, subjects were asked to estimate the number of previous economics experiments they had participated in.

2.5. Subject Recruitment

Three sessions were run at CASSEL (California Social Science Experimental Laboratory) in the UCLA Public Affairs building in 2013. Subjects from the CASSEL subject pool were recruited via e-mail. Each session recruited a maximum of 50 subjects, and 90 subjects in total participated across the three sessions, yielding 45 dictators. The CASSEL lab closed in 2013, and as a result a fourth session was run at Caltech SSEL (Social Science Experimental Laboratory) using the SSEL subject pool. Results including these Caltech subjects are included in Appendix A as a robustness check; for simplicity, the main results in the paper will focus on subjects from just the CASSEL subject pool.[7]

3. Results

There were 90 subjects (45 dictators) across the three UCLA sessions. Each dictator made 24 allocations (eight in each of the gift, overturned gift, and control treatments) for a total of 1080 dictator allocations. For descriptive statistics about subject traits (e.g., age, gender, etc.), see Appendix A.

3.1. Summary Statistics

Figure 6 shows gift-giver behavior by treatment and item. Across all items, gift-givers gave away their item slightly less than half the time. They almost always gave away the pen and rarely gave away the cash gift. Giving patterns were fairly similar between both gift and overturned gift treatments, suggesting that the 50% chance of having a gift overturned did not have a large influence on a gift-giver's choice. (Figure 6 displays whether gift-givers attempted to give a gift, regardless of whether it was overturned or not). Since gift-givers did not see dictator allocations until the end of the experiment, their gift-giving rates would not have been influenced by dictator allocations.

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[7] This Caltech session yielded 14 dictators. Adding these subjects yields the same (and in fact more statistically significant) results, but since the subject pool is different, analysis including them is reserved for Appendix A. Note that all SSEL subjects were Caltech undergraduates with verified current Caltech IDs, and all CASSEL subjects were UCLA undergraduates with verified current UCLA IDs, so it is unlikely there were any subjects who belonged to both subject pools.

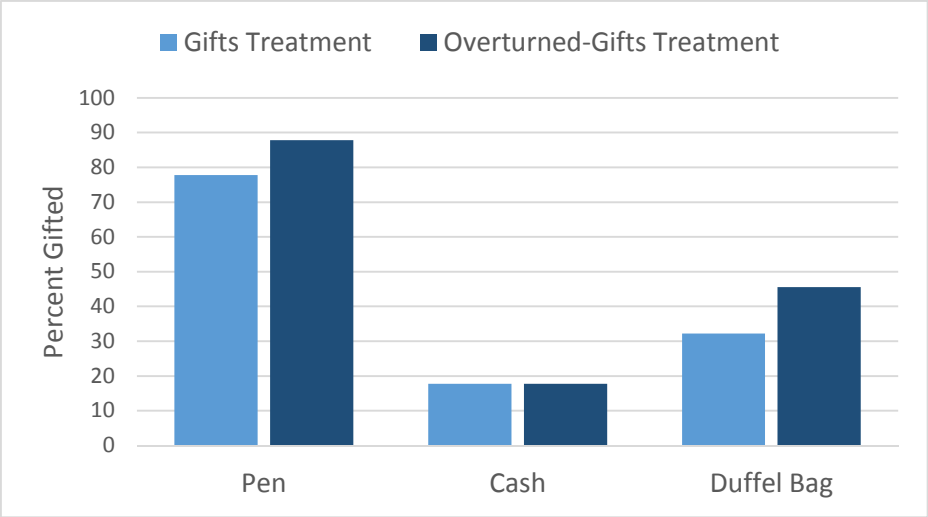

Figure 6. Gift-giving behavior by item and treatment. All gift-givers received exactly two chances to gift each item in both the gift and overturned gift treatments.

Figure 7 shows average dictator allocations in each treatment-item pairing. In the control treatment, dictators on average allocated more when receiving any item from the computer than when receiving no item at all, indicating preferences over wealth distribution. When dictators received any item from their partner, they allocated more than when they received the same item from the computer, indicating reciprocity to all items. Reciprocity to the cash gift and the duffel bag had the largest magnitude effects on average, although relative to perceived dollar value, the cash gift likely had a larger effect. Finally, average allocations doubled in response to overturned gifts relative to rounds where the dictator also ended up with no item, indicating reciprocity to just the intention to give a gift. On average there was also no punishment effect when the partner gave no item, since allocations are similar when no gift was given, regardless of whether the gift would have come from the computer or their partner. For histograms of allocations by treatment and item, instead of just averages, see Figure A1 in Appendix A.

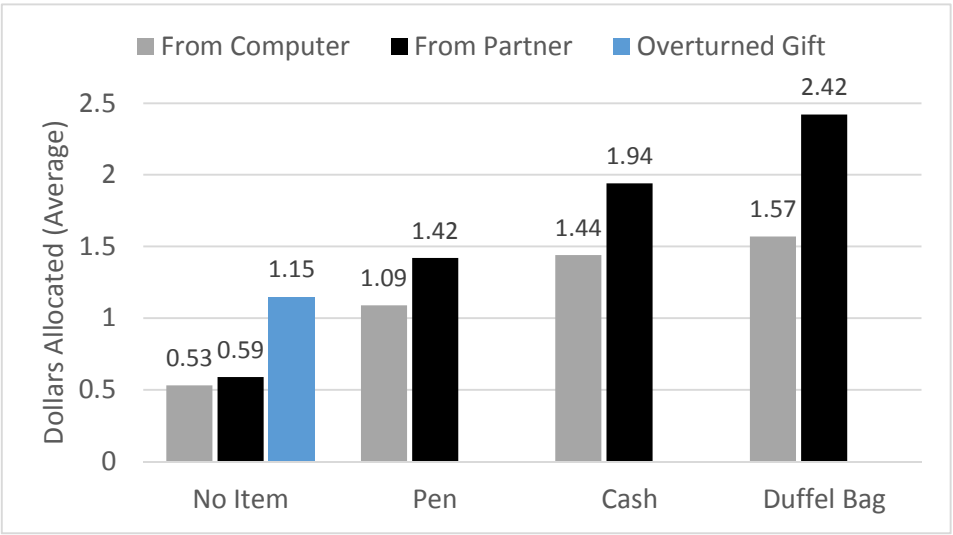

Figure 7. Average allocations by item and treatment. There are unbalanced numbers of observations from each dictator within each group; some dictators may factor more heavily into one average than another (for this reason, no standard error bars are shown, and the figure should be interpreted as only providing a general sense of dictator allocations).

3.2. Regression Analysis

To properly account for within-subject variance, the main results use ordinary least squares (OLS) regression models at the subject-round level with subject random effects.[8] The dependent variable is dollars allocated by the dictator. The main model, Model (1) in Table 1, includes indicators that identify what item the dictator ended up with for that subject-round. These indicators estimate the change in a dictator's allocation caused by simply having additional wealth in the form of each item. These are also interacted with another indicator that specifies whether the item was received from their partner (as opposed to from the computer); these interactions represent reciprocity to the gift-giver once wealth effects are controlled for. The model also includes an indicator representing whether a gift was overturned. Note that if a gift was overturned, the dictator necessarily has no item in their possession. Finally, treatment indicators are added to ensure that, conditional on the above variables, baseline allocations did not differ across treatments. Standard errors are clustered by subject. Models (2)–(5) modify Model (1) in ways that will be explained in subsequent sections.

Table 1. Allocation by Type of Gift Received.

DV = Dollars Allocated	**(1)**	**(2)**	**(3)**	**(4)**
Specification [a]	OLS	OLS	Tobit [b]	OLS
Dictators (Rounds)	45 (1080)	45 (1080)	45 (1080)	45 (1080)
R^2	0.120	0.120		0.120
	Treatment Indicators			
Gift	0.190 (0.125)	0.190 (0.125)	0.435 (0.348)	0.187 (0.123)
Overturned Gift	−0.057 (0.076)	−0.057 (0.076)	−0.265 (0.305)	−0.060 (0.075)
	Item Indicators (Wealth Effects)			
Has pen	0.560 *** (0.148)	0.560 *** (0.148)	1.463 *** (0.351)	0.563 *** (0.148)
Has $2 item	0.915 *** (0.209)	0.915 *** (0.209)	2.125 *** (0.409)	0.918 *** (0.208)
Has bag	1.037 *** (0.230)	1.037 *** (0.230)	2.256 *** (0.429)	1.048 *** (0.229)
	*Gift-from-Partner * Item Indicators (Reciprocity Effects)*			
Given pen	0.283 * (0.164)	−0.336 * (0.204)	0.739 * (0.398)	0.289 * (0.164)
Given $2	0.453 ** (0.198)	−0.167 (0.219)	1.042 ** (0.414)	0.469 ** (0.202)
Given bag	0.617 ** (0.292)	−0.008 (0.305)	0.922 * (0.547)	0.619 ** (0.292)
Gift overturned	0.619 *** (0.167)		1.910 *** (0.446)	0.630 *** (0.169)
Intention-to-give		0.619 *** (0.167)		
Partner Expectation				−0.006 (0.008)
Constant	0.529 *** (0.139)	0.529 *** (0.139)	−1.945 *** (0.658)	0.544 *** (0.133)
Subject Random Effects	YES	YES	YES	YES

[a] Ordinary least squares (OLS) standard errors (SEs) are robust and clustered by subject. Tobit SEs are bootstrapped (1000 re-samples) and re-sampled by cluster; [b] Tobit is left-censored at 0 (676 left-censored observations); * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

3.2.1. Wealth Effects

Model (1) affirms that preferences over wealth distribution significantly influenced dictator allocations. The baseline allocation corresponding to when the dictator had no item was $0.53 (the constant term), with no significant difference by treatment (the treatment indicators). Simply possessing the pen increased dictator allocations by another $0.56 on average, approximately doubling the amount allocated relative to possessing no item. Possessing the duffel bag increased allocations by $1.04. Possessing the two-dollar cash item increased allocations by $0.92. All three wealth effects are significant at the $p < 0.01$ level, suggesting that dictators had clear wealth distribution preferences on average.

[8] Random effects assumptions are met because all covariates are determined exogenously to the dictator (i.e., by their randomly selected partner or by the experimenter). Subject fixed effects yield similar results for all regressions in this paper, and the Hausman test comparing coefficients between the random effects and fixed effects models always suggests there is no systematic difference in their coefficients ($p > 0.99$ for Model (1)).

3.2.2. Reciprocity to Gifts

Model (1) indicates that, beyond simple wealth effects, receiving an item from the gift-giver caused dictators to reciprocate and allocate even more. Receiving a duffel bag from the gift-giver increased dictator allocations by another \$0.62. This effect was in addition to the \$1.04 average increase in allocation caused by simply possessing the item; thus, receiving a duffel bag for the gift-giver increased allocations by a net of \$1.66 on average relative to rounds with no gift. Receiving the cash item or pen from the gift-giver instead of from the computer increased allocations by \$0.45 and \$0.28 respectively, although the latter effect is only significant at the $p < 0.10$ threshold. (It reaches the $p < 0.05$ threshold when including the aforementioned Caltech subjects; see Table A2 in Appendix A).

3.2.3. Reciprocity to Intentions

When gifts are overturned, dictators reciprocated by allocating \$0.62 more to their partner, more than doubling allocations relative to other rounds where the dictator also ended up with no item. In Model (2), the overturned gift indicator is replaced with an "intentions" indicator that takes a value of 1 for any instance where intention-to-give is announced or a gift is exchanged (this simply rearranges the indicator variables into different categories). Not surprisingly, since the overturned gift indicator has such a large coefficient in Model (1), this alternate regression suggests that intentions alone drove all of the observed reciprocity effects on average. In fact, receiving the pen led to marginally less reciprocity than an overturned gift; subjects may have underestimated the likelihood that the overturned gift was a pen, or they may not have given much thought to the identity of the overturned gift. Regardless, the results from both Models (1) and (2) clearly indicate a highly significant effect of intentions on dictator choice.

3.2.4. Punishment for Not Giving

Models (1) and (2) also suggest that dictators did not punish their partners if they did not receive a gift. The treatment indicators are insignificant, suggesting that dictators allocated similarly when they received no item in a round, regardless of which treatment they were in and whether gifts (or overturned gifts) from the gift-giver were even possible in that round. This lack of punishment is likely because dictators were reminded in every no-gift round that their partner may not have had an item to give.

3.2.5. Tobit Specification

Model (3) re-runs Model (1) using a panel Tobit specification that is left-censored at 0. Since dictators could not allocate less than \$0, there is significant "piling" at 0 which could skew the results of a linear model. The Tobit specification corrects for this and yields similar results. Reciprocity to the duffel bag does drop to marginal significance ($p = 0.08$). However, note that there were 10 dictators (out of 45) that always allocated \$0 in all 24 rounds. These dictators have no within-group variation; in the random-effects Tobit specification, they are dropped from the within-group analysis, thus leading to biased results. For this reason, main results in this paper use OLS specifications.

3.2.6. Gift-Giver Expectations

Model (4) examines whether controlling for the gift-giver's expectations over allocations can account for the observed effects. Blanco et al. [11] suggest that guilt-based reciprocity is driven by individuals acting according to how they believe others expect them to act; if a dictator's beliefs about their partner's expectations are correlated with their partner's actual expectations, then this variable may predict reciprocity. However, Model (4) shows that conditional on other factors, partner-expectations are not a predictor of allocations, and all effects from Model (1) are still the same after controlling for partner-expectations. (As explained in a previous footnote, dictator beliefs

were not elicited directly out of concern that doing so would bias dictator behavior, so gift-giver expectations were elicited instead).

3.2.7. Effects by Treatment Type

Model (5) in Table 2 examines whether dictators responded differently to gifts in different treatments. Since gifts in the overturned gift treatment are prevented with 50% probability, it is possible that dictators could view these gifts as less kindly intended, since they were given with the knowledge that the item might not actually exchange hands (in other words, the ulterior motives may differ between these two treatments). Thus, Model (5) pools all items into a single category and interacts each treatment with an indicator for whether the dictator received any gift from the gift-giver in that round. (Sample sizes are too small to test interactions between each gift item and treatment condition separately).

Table 2. Allocation by Treatment.

DV = Dollars Allocated	**(5)**
Specification [a]	OLS
Dictators (Rounds)	45 (1080)
R^2	0.119
Treatment Indicators	
Gift	0.183 (0.120)
Overturned Gift	−0.049 (0.083)
Item Indicators (Wealth Effects)	
Has pen	0.455 *** (0.132)
Has $2 item	0.922 *** (0.219)
Has bag	1.134 *** (0.222)
*Gift-from-Partner * Treatment Indicators*	
Gift * Gift treatment	0.449 *** (0.165)
Gift * Overturned Gift treatment	0.402 ** (0.197)
Partner's gift was overturned	0.611 *** (0.173)
Constant	0.529 *** (0.139)
Subject Random Effects	YES

[a] All SEs are robust and clustered by subject. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Model (5) in Table 2 yields similar wealth and reciprocity effects as in Model (1). In addition, a gift from the gift-giver increased allocations by $0.45 on average in the gift treatment and $0.40 on average in the overturned gift treatment (difference is not significant: $p = 0.80$). Thus, the 50% possibility of a gift-return in the overturned gift treatment did not influence dictators to respond any differently to gifts. Moreover, as displayed in Figure 6, the mix of gifts given in the gift and overturned gift treatments are relatively similar, so dictators are responding to a similar mix of gifts in both treatments. Model (5) not surprisingly also displays similar overturned gift effects as Model (1), as well as a similar lack of punishment effects. Altogether, Model (5) is consistent with the idea that the ulterior motives involved in this context are not significantly interfering with reciprocity.

3.3. Dictator Types

Table 3 displays the number of dictators who allocated $0 in all rounds within a specific item-treatment pair. Twenty-six out of 45 dictators always allocated $0 in the Control treatment when they received no item. In standard dictator games, these subjects would be considered economically rational types, or "econs" for short (Thaler 2000). However, nine of these econs (35% of the 26) allocated positive amounts when the computer gave them an item, suggesting that they have other-regarding preferences in wealth distribution. In the gift treatment, 11 of the 22 that allocated $0 when receiving no item instead allocated positive amounts when receiving a gift from their

partner. This suggests that approximately 15% of these subjects have reciprocity-based other-regarding preferences (not necessarily 50%, since 35% may be attributed to wealth distribution preferences). Finally, intentions alone in the Overturned Gift treatment shifted about 30% of econs into allocating positive amounts. This occurred even though an overturned gift should inform dictators that their partner has a gift, thus causing wealth effects to work against increasing allocations.

Table 3. Number of Dictators Allocating $0 by Item and Treatment.

Treatment	No Item	Received Item [a]
Control (From Computer)	26 (58%)	17 (38%)
Gift	22 (49%)	11 (24%)
Overturn [c]	32 (71%)	12 (27%)
Overturned Gift Rounds	18 (40%) [b]	N/A
Always Zero	10 (22%)	

[a] Two of these subjects in the Control and one in the Gift did not always give zero under no item; [b] One subject always gave $0 to overturned gifts, but not in control rounds with no items; [c] In this treatment, the no-item column corresponds to no item attempted.

3.4. Subject Self-Reported Motives

In the questionnaire, dictators were asked why they thought their partner gave them a gift, as well as what motives they had for their allocation choices. To quantify their text responses, nine Amazon Mechanical Turk masters-rated workers coded these responses to determine whether guilt or kindness factored into dictator motives.[9] The workers were blinded to the purpose of the study and were asked to simply evaluate the statements. To ensure the workers were coding carefully, ten fabricated responses that very clearly did or did not mention intentions and/or guilt were mixed into the responses; workers coded these statements accurately.

On average, 25% of dictator statements were rated by the coders as mentioning kindness, gratitude, or positive intentions as reasons for why they reciprocated to gifts.[10] On the other hand, 20% of dictators on average were coded as listing guilt or obligation as a reason. These responses are consistent with both intentions-based and guilt-based models of reciprocity, suggesting that both mechanisms may have played a role in the observed results. Most importantly, these results affirm the possibility that positive intentions may still be inferred from gifts in a setting where ulterior motives are present.

4. Discussion

This study used a gift-giving task embedded within a dictator game to test for motivators of reciprocity. Subjects reciprocated on average to just the intention to give a gift, and reciprocity to intentions was at least as large in magnitude on average as actually receiving any of the three items (after controlling for wealth distribution preferences). These results are consistent with intentions-based models of reciprocity and suggest that purely the intent to give a gift (without any actual giver sacrifice) can be enough to generate reciprocity. In addition, although it may seem surprising that some individuals reported inferring kind intentions when an ulterior motive was present, this is consistent with research showing that gift recipients do not always draw proper inferences on what the gift-giver's motives are [18].

These results do not rule out the possible role of guilt-based reciprocity at the individual level in this or other contexts. Alternative models suggest that guilt, obligation, and expectations can also motivate reciprocity [9–11], and some dictators referenced these motives when explaining their choices. Nevertheless, this design is better able to distinguish between guilt and intentions mechanisms than

[9] The first CASSEL session did not include these questions, as they were added after the first session; to make up for this, I asked the mTurk workers to also code the responses submitted by subjects in the Caltech session.

[10] The exact wording of the category was "gratitude/appreciation/to repay their kindness."

existing studies; unlike standard gift-exchange, moonlighting, and trust games, this study demonstrates that reciprocity occurs even in the absence of any potential guilt effects resulting from giver sacrifice. Most importantly, even if guilt-based mechanisms are still present in this context, this would not contradict the main result that intent to give a gift, independent of giver sacrifice or changes in outcomes, can be enough to engender reciprocity, and furthermore that some dictators reported inferring kindness from an overturned gift.

Subjects also reciprocated to both monetary and non-monetary gifts in this context. Unlike some previous studies [13], cash gifts generated more statistically significant and larger magnitude effects than the equivalent non-monetary gift (a pen in this case), even though subjects estimated the pen as having a market value of approximately $2 on average.[11] However, subjects may have simply assigned very different utility values to the pen and cash, leading to this discrepancy. In addition, subjects estimated the duffel bag at over seven times the dollar value of the pen ($14.70), yet reciprocity to the duffel bag was only approximately twice that of the pen. As such, there may be decreasing marginal returns, in terms of reciprocity, to increasing the value of a gift. This is potentially consistent with evidence in psychology [36] that the value of a gift does not have great impact on an individual's emotional response to a gift. This could help explain why firms often use small-value items such as pens, keychains, calendars, or the like as promotional gifts. However, in this context, none of the gifts proved worthwhile for the gift-giver on average, which differs from the returns to gifts in field experiments [17,20]. Follow-up research should therefore continue to bridge the gap between results from laboratory experiments and results from marketing and economics field studies.

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Appendix A

Table A1 below displays basic descriptive statistics of the dictators. Forty-three out of 45 dictators were aged between 18 and 22, and all 45 self-reported as UCLA students (all subjects also showed UCLA IDs upon check-in). The population skewed female. On average, subjects reported having participated in four economics experiments prior to this one. Most (34 out of 45) had participated in five or fewer experiments. Subjects scored an average of 58 on the Machiavellianism scale.

Table A1. Dictator demographics.

Variable	Mean	SD	Min	Max	Median
Age	20.18	2.48	18	33	20
Male	0.38	0.49	0	1	0
Previous Experiments	4.07	4.49	0	18	3
Machiavellianism	58.71	6.57	45	76	59

Figure A1 below shows the distribution of dictator allocations by treatment and item across all sessions. The first graph in the upper left displays allocations when the dictator received no item in the gift and overturn treatments, and no intention-to-give was announced (the gold, borderless bars); the hollow, black-outlined histogram in the same graph displays when gifts were attempted but

[11] Subjects in the first session were not asked to estimate values of items, so estimates are based on subjects from the remaining sessions.

overturned. Together, they demonstrate that the intention-to-give shifted the distribution of allocations slightly to the right. In the next three graphs, the gold-filled histogram displays allocations when the dictator received an item from the computer, while the hollow histogram displays allocations when the same item was instead from the gift-giver. These show that dictators allocated more when receiving the item from the gift-giver instead of from the computer.

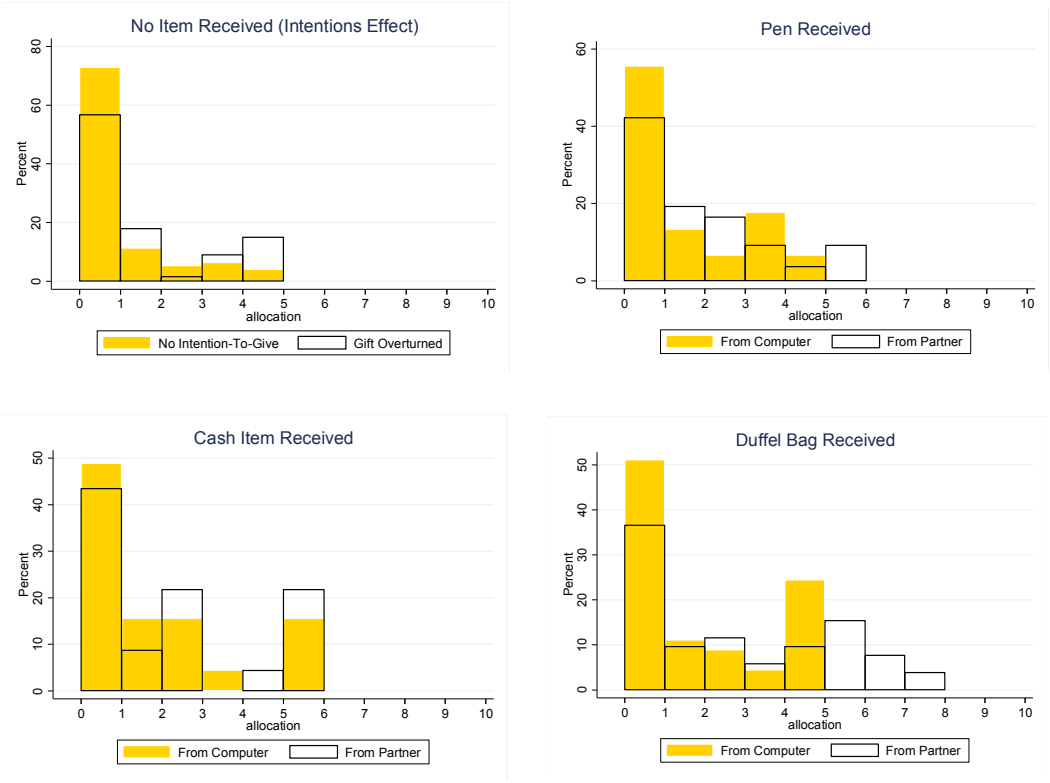

Figure A1. Distribution of Allocations by Item and Treatment.

Table A2 shows Model (1) when including an additional 14 dictators from a session run at the Caltech SSEL laboratory. Since the CASSEL lab at UCLA shut down in 2013, one additional session at Caltech was run to obtain a larger sample size. Since it is from a different subject pool, this is only included as a robustness check. Including these additional 14 dictators re-affirms all of the results in the paper. The reciprocity effect for the small-item gift is now significant at the $p < 0.05$ threshold (it was previously at the $p < 0.10$ threshold), and the reciprocity effect for the cash-item gift is now significant at the $p < 0.01$ threshold (it was previously at the $p < 0.05$ threshold).

Model (A2) in Table A3 shows the main regression when including main effects of age, gender, and experience. Given the increase in explanatory variables, I include the Caltech dictators to maximize power. I also use binary categories for simplicity; *older* is an indicator for being 20+ (mean = 0.52), *experienced* is an indicator for having participated in 5+ experiments (mean = 0.44), and *male* is in an indicator for gender (mean = 0.39, which is 0.01 higher now that the Caltech subjects are included). Results demonstrate that those who are male and experienced with experiments will allocate marginally less overall ($p < 0.10$ threshold), but the main results are all robust to controlling for these variables. Model (A3) adds the Machiavellianism scale [35] alongside the other demographic results, but this measure shows no effect on allocations. When including the Machiavellianism scale, *male* reaches significance at the $p < 0.05$ threshold and *experienced* no longer reaches significance at the $p < 0.10$ threshold, suggesting both of these effects may be somewhat fragile to the exact specification.

The Machiavellianism scale was chosen to test whether willingness to take advantage of others (for one's own gain) would correspond to allocation decisions, including reciprocity. Other scales such

as greed or materialism may also be relevant, but this study focused on only Machiavellianism in order to limit the number of psychological scales that subjects had to take. Model (A4) demonstrates that the same reciprocity effects, including to intentions, still hold even when limiting the regression to those that scored above the median on the Machiavellianism scale (although results on the cash gift are no longer significant due to small sample sizes). Thus, those that are "more Machiavellian" amongst this subject pool still demonstrated strong reciprocity to intentions. However, note that these subjects do not score all that high on the scale (max score of 76).

Table A2. Allocation by Type of Gift Received (Includes Caltech Session).

DV = Dollars Allocated	**(A1)**
Specification [a]	OLS
Dictators (Rounds)	59 (1416)
R^2	0.119
Treatment Indicators	
Gift	0.170 * (0.099)
Overturned Gift	−0.039 (0.059)
Item Indicators (Wealth Effects)	
Has pen	0.566 *** (0.127)
Has $2 item	0.939 *** (0.178)
Has bag	1.151 *** (0.222)
*Gift-from-Partner * Item Type (Reciprocity Effects)*	
Given pen by partner	0.260 ** (0.128)
Given cash by partner	0.612 *** (0.191)
Given bag by partner	0.557 ** (0.262)
Partner's gift was overturned	0.596 *** (0.145)
Constant	0.604 *** (0.148)
Subject Random Effects	YES

[a] All OLS SEs are robust and clustered by Subject. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table A3. Main Results with Demographic Controls (Includes Caltech Session).

DV = Dollars Allocated	**(A2)**	**(A3)**	**(A4)**
Specification [a]	OLS	OLS	OLS
Subjects	All	All	High Mach
Dictators (Rounds)	59 (1416)	59 (1416)	24 (576)
R^2	0.180	0.196	0.208
Treatment Indicators			
Gift	0.170 * (0.099)	0.170 * (0.099)	0.215 (0.155)
Overturned Gift	−0.038 (0.059)	−0.038 (0.059)	−0.040 (0.107)
Item Indicators (Wealth Effects)			
Has pen	0.566 *** (0.127)	0.566 *** (0.127)	0.554 ** (0.228)
Has $2 item	0.939 *** (0.178)	0.939 *** (0.178)	1.138 *** (0.321)
Has bag	1.151 *** (0.223)	1.151 *** (0.223)	1.408 *** (0.371)
*Gift-from-Partner * Item Type (Reciprocity Effects)*			
Given pen by partner	0.259 ** (0.128)	0.259 ** (0.129)	0.391 * (0.209)
Given cash by partner	0.611 *** (0.191)	0.611 *** (0.191)	0.272 (0.237)
Given bag by partner	0.555 ** (0.263)	0.555 ** (0.263)	0.859 ** (0.373)
Partner's gift was overturned	0.594 *** (0.145)	0.594 *** (0.145)	0.977 *** (0.261)
Demographic Controls			
Older	0.252 (0.326)	0.285 (0.327)	
Experienced	−0.580 * (0.305)	−0.602 ** (0.298)	
Male	−0.531 * (0.307)	−0.437 (0.275)	
Machiavellianism		−0.023 (0.020)	
Constant	0.934 *** (0.267)	2.203 * (1.188)	0.238 *** (0.779)
Subject Random Effects	YES	YES	YES

[a] All OLS SEs are robust and clustered by Subject. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Appendix B

These instructions were provided to subjects as handouts. Each part or section of the session began with the instructor reading the instructions that corresponded to that specific task only.

Appendix B.1. Introduction

Welcome and thank you for participating. At this point, please don't talk to any other participants and please turn off all cell phones and electronics. If you haven't yet, please remove all personal materials from your desk (you may place them underneath your seat).

If at any point you have any questions, please raise your hand and I will come to your seat to answer your question in private. This will avoid delaying the experiment or disrupting others' during a task.

Today's session is a study on individual judgment and decision-making. Over the course of the experiment, you will make choices in a series of tasks, and these choices may impact your earnings as well as the earnings of others. In this particular study, you may also earn various items. If earned, these items will be handed out, along with your monetary earnings, at the end of the experiment by the lab manager.

For today, the items you can earn in this experiment are: a duffel bag; a pen; and/or an additional $2 in cash.

There is no deception or dishonesty in this experiment. It is an anonymous experiment; your name will not be used in any way by the researchers conducting this study.

You have a set of handouts in front of you. Please *do not* flip through these until you are instructed to do so. In addition, please do not mark up your handouts, and do not take them with you when you leave.

Appendix B.2. General Instructions: Section 1

In Section 1 of this experiment, the computer will randomly assign you to one of two roles: **role 0** or **role 1**. You will retain this designation throughout Section 1.

The computer will randomly pair you with one other participant. If you are a role 0, you will be paired with a role 1. If you are a role 1, you will be paired with a role 0. You will remain anonymous to one another. Each role will have different responsibilities. These responsibilities will vary as we progress through the experiment, although **role 0** will always be designated the primary decision-maker. As the experiment progresses, you will also be re-matched with a new, random partner for each task; that random partner will always be of a different role than you.

Please click "OK" on your screen to learn your role assignment.

Appendix B.3. Section 1 Continued

In this part of Section 1, for each pair of people, the computer will give **role 1** people an item with probability $\frac{3}{4}$. With probability $\frac{1}{4}$, neither person will receive an item. If you receive an item, you will see a picture of the item on your screen. Your partner will *not* know whether you received an item.

If an item was given to **Role 1,** he or she may choose to keep this item or instead give it to their role 0 partner by clicking the "Give" button. If this is chosen, the picture of the item will disappear from role 1's screen and will show up on role 0's screen.

Following this, the role 0 person will be designated the primary decision-maker in the pair. He or she will be given $10. Role 0 must then choose how much of the $10 to give to their role 1 partner. The amount that **role 0** keeps will be his or her earnings for this part of the experiment (plus any item that they received). The amount that role 0 gives to role 1 will be **role 1**'s earnings for this part of the experiment (plus any item that they kept).

Note to **role 0s**: If a role 0 does not receive a gift, it may be because role 1 did not receive an item from the computer this round. In addition, role 1s receive at most only one item per round, and they can only give the item that they received that round.

You will repeat this task for eight rounds; each round will be with a new, randomly matched person of the proper role type. At the end of the experiment, one of these rounds will be selected for payment. Items and cash earned in the selected round will be paid to you at the end of the experiment.

Appendix B.4. Section 1 Continued

In this part of Section 1, for each pair of people, the computer will give **role 1** people an item with probability $\frac{3}{4}$. With probability $\frac{1}{4}$, neither person will receive an item. If you receive an item, you will see a picture of the item on your screen. Your partner will *not* know whether you received an item.

If an item was given to **Role 1**, he or she may choose to keep this item or instead gift it to their role 0 partner by clicking the "Give" button. However, if "Give" is chosen, the computer will override this choice with probability $\frac{1}{2}$ and force role 1 to keep the item. If the computer overrides the choice, it will be announced to both players that a gift was attempted but overridden by the computer. If the computer instead allows the item to be given, then the picture of the item will disappear from role 1's screen and appear on role 0's screen.

Following this, the role 0 person will be designated the primary decision-maker in the pair. He or she will be given \$10. Role 0 must then choose how much of the \$10 to give to their role 1 partner. The amount that **role 0** keeps will be his or her earnings for this part of the experiment (plus any item that they received). The amount that role 0 gives to role 1 will be **role 1's** earnings for this part of the experiment (plus any item that they kept).

Note to **role 0 s**: If a role 0 does not receive a gift, it may be because role 1 did not receive an item from the computer this round. In addition, role 1s receive at most only one item per round, and they can only give the item that they received that round.

You will repeat this task for eight rounds; each round will be with a new, randomly matched person of the proper role type. At the end of the experiment, one of these rounds will be selected for payment. Items and cash earned in the selected round will be paid to you at the end of the experiment.

Appendix B.5. Section 1 Continued

In this part of Section 1, for each pair of people, the computer will give either person an item with probability $\frac{3}{4}$. With probability $\frac{1}{4}$, neither person will receive an item. If you receive an item, you will see a picture of the item on your screen. If you received an item, it means your partner did not.

If you did not receive an item, the computer *may or may not* announce whether your partner received an item and what that item is. If you received an item, the computer will tell you whether your partner is aware that you received an item.

Following this, the role 0 person will be designated the primary decision-maker in the pair. He or she will be given \$10. Role 0 must then choose how much of the \$10 to give to their role 1 partner. The amount that **role 0** keeps will be his or her earnings for this part of the experiment (plus any item that they received). The amount that role 0 gives to role 1 will be **role 1's** earnings for this part of the experiment (plus any item that they received).

You will repeat this task for eight rounds; each round will be with a new, randomly matched person of the proper role type. At the end of the experiment, one of these rounds will be selected for payment. Items and cash earned in the selected round will be paid to you at the end of the experiment.

Appendix B.6. Section 2: General Instructions

In Section 2, you will answer a series of questions about yourself. Most of these will be multiple choice questions, but several will also require open text responses. Please respond carefully and truthfully. When you finish the questionnaire, the computer will randomly select three rounds from Section 1 (one from each part) to determine your pay for the experiment. You must finish the questionnaire to be paid in full.

Note: When typing your responses to the open-ended text questions, please make sure to hit the "ENTER" key when you are done typing. Otherwise, the computer will not record your response.

Upon hitting "ENTER," the text that you entered should disappear; this confirms that your response was recorded by the computer.

When you are finished with the questionnaire, please wait patiently at your seat. I will pay you according to your computer ID while you are at your seat. When all subjects are finished, I will dismiss you.

Appendix C. Detailed Procedure

For replication purposes, more details about the actual implementation of the study are below:

Upon arrival, subjects took a seat at a computer alongside a packet of handouts (Appendix B) and an ID number printed on a slip of paper. Subjects were read the instructions on the handouts, which explained that they could earn certain items in this experiment alongside their pay. They were also instructed that the study was anonymous and did not contain any deception. Next, they were told that each subject would be assigned to either role 0 or role 1 for all of Section 1 of the experiment. Subjects then clicked on a button in zTree and received their role assignment. They were then instructed to enter their unique, printed ID number into zTree, as they would later be paid according to those IDs.

The instructions for Part I of Section 1 were then read out loud. Part I could be the gift treatment, the overturned gift treatment, or the control treatment; the ordering was randomized across sessions, although the first section was never the overturned gift treatment (as this treatment had the most complex instructions). The instructions explained the exact decisions that each role would face, including the exact information that each role would have about the other. For instance, in the gift treatment, the gift-givers were told that if they kept the item for themselves, the dictator would not be able to tell whether they had withheld an item from them. Subjects were also informed that one round in Part I would be selected for payment. A similar procedure took place when subjects reached Part II and Part III of Section 1 (comprising the remaining two treatments).

Upon finishing Part III, subjects moved on to Section 2, the questionnaire. After subjects completed the questionnaire, the computer selected three rounds for payment (one from each treatment) and informed subjects of their earnings. Subjects were paid according to their unique, printed ID number. Gift items that were earned during the experiment were handed out by the lab manager simultaneously with payment.

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