Nice to You, Nicer to Me: Does Self-Serving Generosity Diminish the Reciprocal Response?

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26 September 2017

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September 26, 2017

Abstract: Reciprocity has been shown to be sensitive to perceived intentions, however, not much is known about the intensity of reciprocal responses to the precise nature of those intentions. For example, a person can strategically appear to be kind while being self-serving or can be selflessly (genuinely) kind. Do these two intentions elicit different reciprocal reactions? We propose a conjecture that self-serving but generous actions diminish the positively reciprocal response, compared to selfless generous actions. We classify actions that increase a recipient's maximum payoff, but by less than the giver's maximum payoff, as being self-serving generous actions, while classifying actions that increase a recipient's maximum payoff by more than the giver's as selfless generous actions. We hypothesize that selfless generous actions are considered more generous than self-serving generous actions, and that self-serving generous actions will therefore result in a diminished reciprocal response. We test this conjecture using two novel experimental designs. We find some evidence that subjects perceive self-serving generous actions as being less generous than selfless generous actions, but no empirical support for our conjecture on the diminished reciprocal response.

Acknowledgements: We would like to thank the editor Lata Gangadharan, Tim Cason, Jeremy Clark, Andreas Nicklisch, Vjollca Sadiraj, Daniel Schunk, Radovan Vadovič, and two anonymous referees for helpful comments, as well as participants of various conferences, workshops, and seminars where this work was presented. This paper is based on Daniel Woods' Masters thesis written at the University of Canterbury (Woods, 2013). Funding was provided by the College of Business and Economics, University of Canterbury and the Macquarie Graduate School of Management.

Keywords: Reciprocity, generosity, self-serving, genuine, experiment, lost wallet game, investment game

JEL Codes: C70, C91

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"If you're helping someone and expecting something in return, you're doing business not kindness."

Unknown.

1. Introduction

Do you care whether a person is genuinely (selflessly) kind to you or whether he just pretends to be so he could reap future benefits? Does your response to his kind action depend on whether his behavior is potentially strategic (self-serving)? There are many everyday situations where the distinction between genuine or strategic intent plays a crucial role in determining the intensity of behavioral response. Consider the following scenario. You are at a restaurant in a country with a tipping culture and the waiter is providing an extraordinary service. You realize that his kindness might be disingenuous and that he might be pretending to be nice in order to extract a higher tip. How do you tip him? Do you care about the possible intentions behind his action, which while being beneficial to you, was potentially more beneficial to him? Do you then elect to not reward him with a higher tip or do you tip well, in excess of what you normally tip, because you had a pleasant dining experience thanks to his service?

Previous research provides vast evidence that many economic transactions are governed by reciprocity (see Fehr & Gächter, 2000; Camerer, 2003; Sobel, 2005; Fehr & Schmidt, 2006; Chaudhuri, 2008 for surveys). Kind and unkind intentions behind actions have been identified as an important driving factor of positively (Cox, 2004; Cox, Sadiraj & Sadiraj, 2008; Falk, Fehr & Fischbacher, 2008) and negatively reciprocal behavior (Blount, 1995; Offerman, 2002). However, not much is known about the intensity of reciprocal responses to the precise nature of those intentions, for example when the level of kindness (or conversely unkindness) increases from 'kind' to 'kinder'. From this perspective, most previous studies could be described as having a 'binary format' in that they consider reciprocal responses to kind actions versus actions with neutral or no intentions, or pitch kind actions against unkind ones. Our research takes a step towards the 'continuous format' as we keep the underlying actions kind, but vary intentions in a way suggested by the above example with the waiter who is being strategically kind.

As reciprocity is particularly sensitive to perceived intentions, distinguishing between genuine and strategic intentions is central for understanding of the origins of reciprocal behavior. In this paper we therefore formally develop a conjecture that a self-serving generous action (a specific type of strategic kind behavior) leads to a weaker positively reciprocal response than a selfless generous action. To pin down the terminology, in the spirit of the Revealed Altruism theory by Cox, Friedman & Sadiraj (2008) we define self-serving generosity as a giver's (henceforth, First Mover or FM) action that directly benefits the recipient (henceforth, Second Mover or SM) by increasing her maximum payoff, while also benefiting the FM by increasing his own maximum payoff by *more* than that of the SM. Similarly, if the action results in a smaller increase (or a decrease) in the FM's maximum payoff, we classify this as

selfless generosity. To investigate reciprocal preferences, we focus on how the SM reacts after a FM chooses either a self-serving or selfless action. Importantly, we keep the underlying FM's action otherwise equally generous in both cases. This gives us a clean test of the strength of reciprocal responses to actions that are selfless and self-serving.

We experimentally test our conjecture in two novel designs that allow us to vary the precise nature of intentions (self-serving or selfless) in a way pinned down by the underlying theory to study the 'primitive' of reciprocity (i.e. the kinder you are to me, the more I am inclined to be kind back). We find some evidence that subjects perceive self-serving generous actions as being less generous than selfless generous actions, which would imply a different reciprocal response from the above basic (naïve) interpretation of reciprocity. Despite that, we find no support for our conjecture on the diminished reciprocal response. Our results suggest a parsimonious refinement of the Revealed Altruism theory, as well as providing some justification for other theories of reciprocity to ignore considerations for self-serving generosity.

2. Relationship to the Literature

Economics experiments demonstrate that if kind or unkind intentions can be attributed to actions, the reciprocal response tends to change compared to a situation where no intentions can be inferred from the same actions (e.g. Charness, 2004; Gneezy, Güth & Verboven 2000; Kritikos & Bolle, 2004). The experimental designs studying the role of intentions thus allow for their presence in one condition and remove them in the control condition by either implementing the choice of the decision-maker exogenously by the experimenter (e.g. Cox, 2004), using a randomizing device (e.g., Cox & Deck, 2005), or by forcing a particular choice through limiting the choice set to one alternative (e.g. McCabe, Rigdon & Smith, 2003). Such designs, however, do not permit conclusions regarding the intensity of reciprocal responses based on the different underlying types of intentions that imply varying levels of kindness.

Bruni, Corazzini & Stanca (2009) go one step further than the earlier papers and vary the *nature of intentions* (rather than completely removing them), by either informing or not informing the FM who has an opportunity to act in a generous way that the SM can reciprocate. They find that the SMs respond to the possible strategic motivation behind FMs' generosity and reward them more when extrinsic motives can be ruled out. In contrast to Bruni et al., our design varies the nature of intentions by changing how much the FM stands to gain relative to the SM, making the generosity selfless or self-serving, all while keeping both parties fully informed about the feasible final allocations. Such a distinction is not possible in the Bruni et al. design as the FM and SM's maximum payoffs are kept constant in their informed and uninformed treatments, meaning they are not different by the 'self-servingness' of their FM's generous actions.

Additionally, our design contrasts with Bruni et al., in that we vary the level of potential strategic motivation, rather than ruling it out entirely. A FM that makes increasingly self-serving (selfless) but generous actions is increasingly more (less) likely to be taking that action for strategic reasons.[2] From this overall perspective, the novelty of our approach to studying reciprocal behavior comes from varying the nature of intentions by means of how much one person stands to gain relative to another without manipulating the informational content. The channel through which the nature of intentions is varied could be important for the reciprocal response. By varying intentions in terms of observable outcomes, there is less uncertainty about the FM's decision; the FM knowingly took an action fully understanding the consequences. Changing the informational content as in Bruni et al. is intuitive from a qualitative standpoint, but hard to pin down in terms of quantitative theory, which is an advantage of our approach.

While there exists evidence that perceived intentions behind actions determine the reciprocal response, the actual intentions, are usually difficult to infer from actions. To this effect Cox, Servátka & Vadovič (2017) conduct an experiment testing whether acts of commission reveal intent to a greater degree and therefore lead to a stronger reciprocal response than acts of omission. They indeed find that acts of commission, which actively impose kindness or harm, generate stronger reciprocity than acts of omission, which represent failures to act kindly or to prevent harm. In their experiment a generous act increases the opportunity set of the SM, but is selfless; hence their data do not (and are not intended to) permit a conclusion as to whether the SM responds to the generous action being self-serving or not.

What intuition do existing models of reciprocity provide regarding the importance of self-serving intentions for reciprocal behavior? Distributional preference theories (e.g. Fehr & Schmidt, 1999; Bolton & Ockenfels, 2000) describe preferences over the final distributions of payoffs, with no considerations for how surplus is generated. Motivations behind actions, such as self-serving generosity, are not considered when decision-makers make their consequential choices; therefore such models do not shed any light on our research question.

Belief-dependent models of reciprocity by Rabin (1993), Dufwenberg & Kirchsteiger (2004), or Falk & Fischbacher (2006) incorporate intentions through the SM's beliefs about the kindness of the FM. For illustration, consider Falk & Fischbacher's (2006) model that defines a kindness term, which is then used to determine the extent of reciprocal response. The kindness term consists of an intention factor and an outcome factor. The intention factor represents how intentional a FM is being in making a decision, as it is based on the presence of other decisions and how reasonable they are. The outcome factor is the difference between the expected payoffs of the FM and

[2] As will become clearer in our design the strategic element stems from the fact that the FMs can receive more back than their outside option. This is possible in all our treatments; in our self-serving treatments, more people will be taking the action for strategic reasons as it is more feasible that they will receive back more.

SM. This outcome factor is calculated given the SM's beliefs about the other's actions (a first-order belief), and their beliefs about the FM's beliefs about the SM's own actions (a second-order belief). If the kindness factor is positive, the action is considered kind, and if the kindness factor is negative, the action is considered unkind. SMs will want to reward kind actions and punish unkind actions, depending on how reciprocal they are. Actions that meet our definition of self-serving generosity could be considered unkind, as the outcome term is negative if we assume that a SM believes the FM expects to receive more than the SM. Falk & Fischbacher's model can therefore predict that SMs will respond positively to selfless generous actions and negatively (or less positively) to self-serving generous actions, meaning we should observe a difference in the reciprocal response of the two types of generous actions, assuming fixed beliefs. Nonetheless, whether an action is considered self-serving or not depends entirely on the SM's beliefs. While in principle it is possible to influence beliefs in experiments (e.g. through framing), in the given scenario they remain outside of our control. Another issue is that the above discussed equilibrium models are complex and have multiple equilibria in higher order beliefs (i.e. beliefs about other's beliefs), making them intractable in many applications (see Cox, Friedman, and Gjerstad, 2007 for a more detailed discussion). Akin to the reason expressed nicely in Charness and Dufwenberg (2006), sec. 5.1, pp. 1591-92, our goal is to test the implication that self-serving actions have for reciprocal reciprocity, not whether we observe equilibrium play in a one-shot setting and whether subjects form correct beliefs in the presence of self-serving and selfless actions. All of the above makes testing our conjecture troublesome in a belief-dependent framework.[3]

Charness & Rabin (2002) incorporate intentions by having the SM lose other-regarding considerations towards the FM if the FM 'misbehaves' by acting inconsistently with some 'social consensus'. However, Charness & Rabin assume SMs have quasi-maximin preferences, according to which people care about social welfare, so it is not clear how the SM would react to a self-serving generous action that always improves social welfare compared to a selfless generous action that would increase social welfare by less, or in some instances decrease social welfare. SMs may then lose other-regarding preferences for those that undertook a selfless generous action, which is the opposite of our conjecture. Furthermore, different assumptions about social consensus would have different implications for the SM's response, therefore an a priori criterion for specifying the social consensus with respect to self-serving generosity would be necessary to make a testable prediction.

[3] Other shortcomings and limitations of belief-dependent models of reciprocity are discussed in Hinz & Nicklisch (2015) who explore the continuity of the reference value (employed by Rabin, 1993 and Dufwenberg & Kirchsteiger, 2004) and the continuity of the intention factor (Falk & Fischbacher, 2006) in a series of mini-ultimatum games. They find that the distance of the observed offers to the proposed reference value provides a poor measure for their kindness and that a pairwise comparison of offers à la Falk & Fischbacher's model cannot explain behavior in richer settings.

An appropriate theory of reciprocity to embed our explorations in would either have, or allow for introducing, considerations for self-serving motivations, and provide us with clear and testable hypotheses on observables. Such a theory, not dependent on beliefs, is that of Revealed Altruism (Cox, Friedman & Sadiraj, 2008, henceforth CFS). In this theory CFS posit that an action that is more generous than another is met with a more (conditionally) altruistic response. CFS define an action from a FM being 'more generous than' (MGT) another to the SM if it meets two conditions. The first condition is that between any two actions, the one that offers the higher SM maximum potential income induces higher generosity. The second requirement is that the increase of the FM's maximum potential income does not exceed the increase in the SM's maximum potential income. In other words, the action cannot be self-serving. As the feasible maximum potential incomes of an opportunity set are properly defined, generosity in this theory is determined in an observable and unambiguous way. This is unlike in psychological games that work with individual's beliefs that are not directly observable, are of a high order, and elicitation of which can be problematic, due to belief elicitation affecting behavior (or vice versa) (e.g. Gächter and Renner, 2010). We therefore use Revealed Altruism as our framework for answering our research question. Revealed Altruism, however, makes no explicit predictions about the reciprocal response to self-serving but generous actions. Rather, it states that actions cannot be self-serving, meaning self-serving generosity is outside the scope of the theory. We therefore expand the Revealed Altruism theory by positing that self-serving actions will elicit a diminished reciprocal response than selfless actions.

Using the maximum payoff in our definition of self-serving generosity follows from Revealed Altruism. The FM's maximum payoff may seem like an unintuitive choice for considering the FM's intentions, as the SM is unlikely to desire an allocation that assigns all available surplus to the FM, which leaves nothing to the SM. In situations where allocations near the FM's maximum payoff are considered unlikely, self-serving and selfless generosity (under the current definition) might be indistinguishable from one another and in such cases one might consider an alternative definition based around the minimum payoffs of the pair, or some other reference payoff instead. Another suggestion could be what the FM believes he will receive back, however, that returns to the issue of (potentially problematic) beliefs. In defense of the maximum payoff, it is plausible that there is correlation between the maximum payoff and the SM's second order belief, the operative belief that the SM would be using to assess the intentions of the FM. If the maximum payoff the FM can receive increases, it is not unreasonable to propose that the FM expects to receive more back. Therefore, the FM's maximum payoff becomes a proxy for the SM's second-order beliefs about the FM's actions, with the main added advantage of being

directly observable.[4] Ultimately, we retain the use of maximum payoffs in our definition while keeping the above issues in mind.

CFS provide some support for their second condition, using data from Andreoni, Harbaugh & Vesterlund's (2003) Carrot & Stick game. In the Carrot & Stick game, the FM can split $2.40 between himself and the SM, with a minimum of 40 cents being sent to the SM. The game has three variants, the Carrot Game, in which the SM can spend 1 cent to reward the FM by 5 cents, the Stick Game, in which the SM can spend 1 cent to punish the FM by 5 cents, and the Carrot & Stick Game, in which the SM can either reward or punish the FM at the rates previously described.[5] If we compare the Stick variant to the Carrot & Stick variant, the same FM split decision differ in their MGT ordering according to the second condition. This is because SMs cannot reward FMs in the Stick variant, making generosity selfless, compared to the potentially self-serving generosity in the Carrot & Stick variant. While CFS do report statistical evidence in support of our conjecture, this is not sufficient to answer our research question. The SM's action sets change between only being able to punish (or do nothing), to being able to punish and reward (or do nothing). Such a change could influence behavior for reasons other than self-serving generosity. For example consider the observed individual behavior between Dictator games where FMs could 'give' money to the SM, or 'take' money from the SM (e.g. Bardsley, 2008; Cappelen et al., 2013; Cox et al., 2016), where the addition of the option to take reversed FM preferences from giving to taking. Such an effect potentially confounds these Carrot & Stick results on self-serving generosity, which motivates our experimental design as a cleaner, specific, test of whether self-serving generosity diminishes the reciprocal response compared to selfless generosity.

3. Theoretical Framework

3.1. Revealed Altruism Theory

The following section provides a comprehensive overview of Revealed Altruism. CFS develop a model of reciprocity in the spirit of neoclassical economic theory. Suppose there are two players, 'me', and 'you'.[6] Let 'my' income be denoted m and 'your' income be denoted y. 'My' preferences over m and y are smooth, convex and strictly increasing in m. Well-behaved preference can be represented by a general utility function denoted $u(m, y)$, which has a positive partial derivative with respect to m, or $\frac{\partial u(m,y)}{\partial m} > 0$, meaning 'my' utility is increasing in m. The partial derivative with

[4] What definition best approximates self-serving generosity under what circumstances is an interesting empirical question in its own right; however, we leave such explorations for future research.

[5] SMs can spend the entirety of the amount they are sent by the FM, but are limited in not reducing the FM's final payoff below zero.

[6] The theory is general (N players), however, the two player case is presented for ease of explanation. We present the theory in its original version, where 'me' represents the SM and 'you' the FM.

respect to *y*, $\frac{\partial u(m,y)}{\partial y}$, could be zero everywhere if 'I' am selfish, or could be positive or negative depending on 'my' benevolence or malevolence, respectively. The marginal rate of substitution of *m* for *y*, is represented in Equation 1.

$$MRS_{my} = \frac{\frac{\partial u(m,y)}{\partial m}}{\frac{\partial u(m,y)}{\partial y}}, \qquad (1)$$

Equation 1 is undefined for selfish preferences (as $\frac{\partial u(m,y)}{\partial y} = 0$), and swings from +∞ to -∞ as preferences pass from slight benevolence to slight malevolence, so it is convenient to instead use willingness to pay, as presented in Equation 2.

$$WTP = \frac{1}{MRS} = \frac{\frac{\partial u(m,y)}{\partial y}}{\frac{\partial u(m,y)}{\partial m}} = w. \quad (2)$$

The willingness to pay, *w*, represents the amount of *m* 'I' am willing to give up in order to increase *y* by one unit. Note that *w* is intrinsic, i.e., it is invariant to monotonic transformations of u(.). A more altruistic than (MAT) preference ordering is defined as follows. Let *A* and *B* be two preference orderings over *m* and *y*. *A* is MAT *B* if, for a given domain *D*, $w_A(m,y) \geq w_B(m,y), \forall (m,y) \in D$, or in other words, 'my' willingness to pay in *A* either exceeds or is equal to 'my' willingness to pay in *B*, at any allocation (*m,y*) from D.

'Your' action creates an opportunity set, *F*. Let y_F^* be 'your' maximum feasible income in *F*, and similarly let m_F^* be 'my' maximum feasible income in *F*. An action that creates an opportunity set *G* is considered MGT an action that creates an opportunity set *F* if it meets the following two conditions presented in Equations 3 and 4:

Condition A. $m_G^* - m_F^* \geq 0$ (3)

Condition B. $m_G^* - m_F^* \geq y_G^* - y_F^*$ (4)

In other words, Condition A states that *G* is MGT *F*, if *G* provides 'me' with at least as much if not more potential income than *F*. Condition B states the set *G* cannot increase 'your' potential income by more than 'mine', compared to *F*.

The Revealed Altruism model includes two axioms, Axiom R and Axiom S. Axiom R refers to reciprocity, the concept of rewarding (or punishing) good (bad) actions. More formally, Axiom R states:

> *"Let the first mover choose the actual opportunity set for the second mover from the collection C. If F,G ∈ C and G is MGT F, then A_G is MAT A_F."*[7]
>
> (Cox, Friedman, and Sadiraj, 2008, p. 40).

[7] A_X is the preference ordering after observing the action that creates opportunity set X, where X ∈ C.

Therefore, if the opportunity set following your action is MGT of an alternative feasible opportunity set, then it will be met with a MAT response. In other words, if 'your' action increases 'my' potential earnings without increasing 'yours' by more, then my choice will be more generous. Axiom S, which however is not relevant for our research question, states that acts of commission elicit stronger reciprocal response than acts of omission.

3.2.Conjecture and Extension of Revealed Altruism

It is with the preceding framework in mind that we present our conjectures on self-serving generosity. Condition B (Equation 4) is related to the proposed concept of self-serving generosity. It effectively states that two opportunity sets cannot be MGT ordered if a generous action is self-serving. Condition B could be interpreted as a domain in which, when it is satisfied, predictions from the Revealed Altruism theory are defined. Outside of this domain the CFS version of the theory does not offer predictions. We next posit how Condition B affects the MGT ordering.

If the inequality of Condition B is not satisfied, this could affect the MGT ordering. We define an action that satisfies Condition B as a *selfless* action. A selfless action potentially benefits the recipient by more (or at least as much) as the proposer, and it is unambiguous that the action is generous. We define an action that violates Condition B as *self-serving*. In the case of a self-serving action, the FM potentially stands to gain more than the SM, so the FM's kind intentions of any generous but self-serving action are not clearly revealed. We propose that if both actions are equal in MGT ordering according to Condition A (i.e. they both make the recipient equally better off), a selfless action is considered MGT a self-serving action. If a MGT action elicits a MAT response, we therefore posit that a selfless action will elicit a MAT response than a self-serving action.

Figure 1 presents our conjecture graphically in terms of opportunity sets over my income (m), and your income (y). Consider the status quo opportunity set F, where $y_F^* = m_G^* = 5$, and three alternative opportunity sets G, H, and I, where $m^* = 6$, but y^* varies. Opportunity sets G and H satisfy both Condition A and Condition B and are thus MGT F. Opportunity set I satisfies by Condition A but not Condition B. Note, we cannot specify an MGT ordering of G, H, and I by Condition A alone, instead we refer to Condition B. We propose that G is MGT F, H, and I; H is MGT F and I; and I is MGT F.

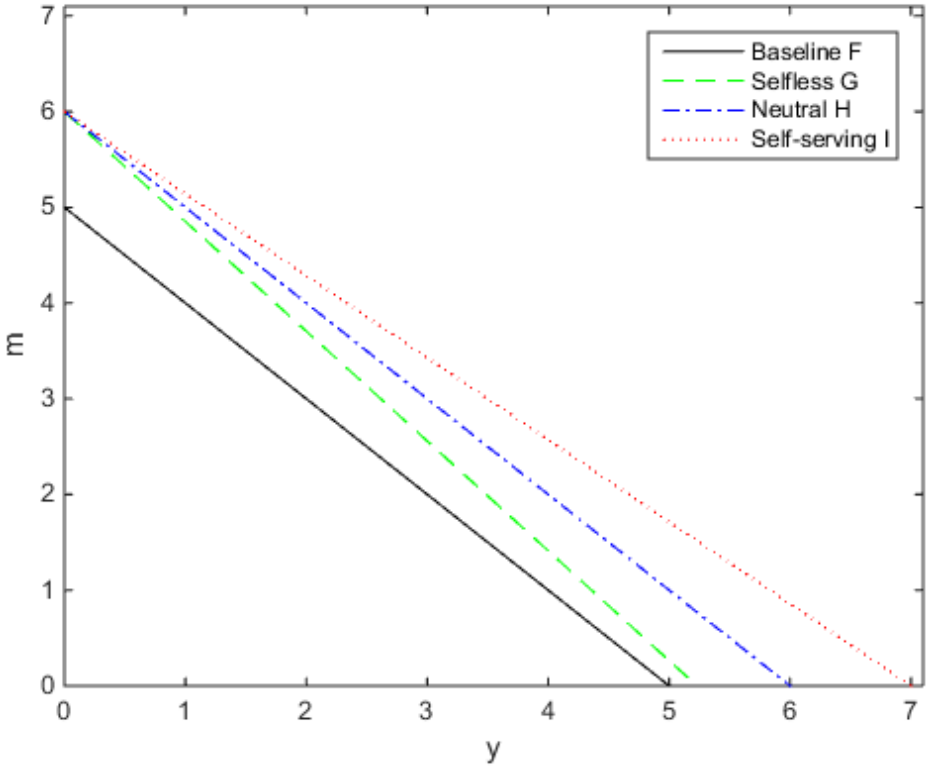

Figure 1 – Illustration of selfless and self-serving generosity.

4. Experiment 1

In general it is quite difficult to acquire data on reciprocal behavior from everyday situations, due to the private nature of many interactions. Even if such interactions were observable, it would be difficult to infer intent, as there are numerous other considerations at play. For example, interaction between two parties is often subject to repetition, meaning motivations may include reputation-building. Even in one-shot interactions, there are motivations such as social norms or social pressure that could confound any attempt to investigate the impact of self-serving actions on reciprocity. A solution is to conduct a one-shot interaction in controlled laboratory conditions. The non-repeated nature of the interaction strips away some motivations not related to the research question, and a sufficiently calibrated design removes any remaining confounds, leaving only the motivations in question to be studied.

4.1.Design

To explore our conjecture, we employ the Lost Wallet Game, henceforth LWG (Dufwenberg & Gneezy 2000). In the LWG, presented in Figure 2, a First Mover can choose either IN or OUT. If the FM chooses OUT, he receives his outside option x, and the Second Mover receives nothing. If the FM chooses IN, then $20 is made

available for the SM to split between the pair with *y* going to the FM and 20-*y* to the SM.

Our objective is to test whether a violation of Condition B affects the reciprocal response, and in order to do so, we hold all other factors that could affect the MGT ordering constant, mainly Condition A. In the LWG Condition A is constant regardless of *x*, as the SM always stands to gain up to \$20. We can make changes to Condition B by varying *x*, which will vary how beneficial it is to the FM to choose IN, and subsequently how selfless or self-serving choosing IN is.

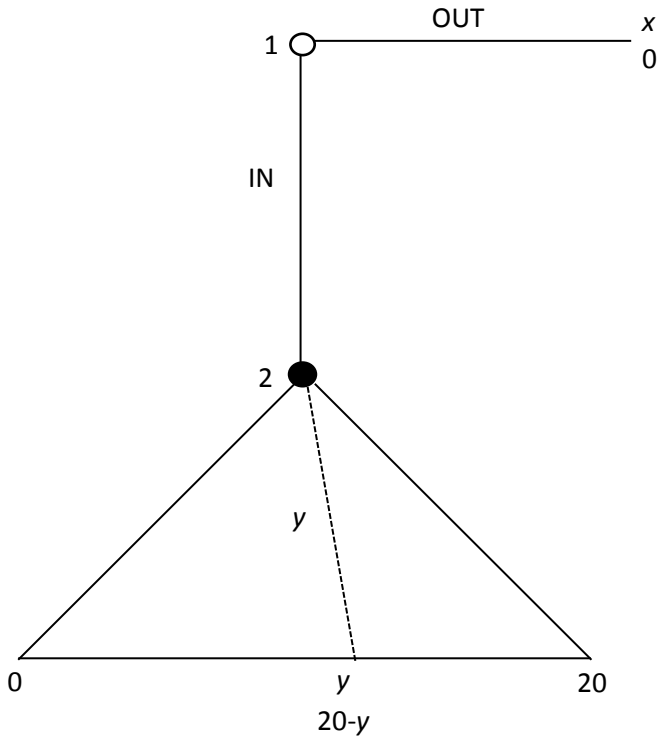

Figure 2 – The Lost Wallet Game

As the FM stands to gain up to 20-*x*, for all positive *x,* choosing IN is selfless, as the FM's maximum potential income will always be less than the SM's (who always stands to gain up to 20). In order to make choosing IN self-serving, we implement a negative *x*, which makes our implementation of the LWG novel and unique. We therefore propose two treatments, a Selfless treatment where *x*=4, and a Self-Serving treatment where *x*=-2.[8] Intuitively, a FM is being selfless when he gives up \$4 to choose IN, compared to when he gains \$2 by choosing IN. By the theory, in the *x*=-2

[8]In Dufwenberg and Gneezy (2000), *x* took the values of 4, 7, 10, 13, and 16. In our experiment we chose *x*=4 for replication purposes, and *x*=-2 as it was the first negative instance counting down in steps of three from *x*=4.

treatment, choosing IN is self-serving, as the FM stands to gain up to $22, whereas the SM stands to gain only up to $20. The negative outside option is implemented by a reduction in the subject's show-up fee.

Additionally, in a questionnaire administered at the end of the experiment we elicit non-incentivized beliefs about generosity and on other subject's actions. We elicit subject perceptions about generosity using a 5-point scale (Likert 1932), where 1 is not generous and 5 is very generous. We ask both the FMs and SMs whether they considered the FM's choice of IN to be generous. This manipulation check allows us to shed further light on the potential MGT ordering.

4.2. Hypotheses

The crux of our experiment, testing whether potential self-serving considerations behind generous behavior are important to reciprocity, comes down to the following three main hypotheses. For the ease of explanation, we present the hypotheses (and results) in parallel to the Revealed Altruism theory, i.e. we first establish support for whether the MGT ordering holds and only then focus on the MAT response, which is directly related to our research question.

We conjecture that choosing IN in the Selfless x=4 treatment is MGT to choosing IN in the Self-Serving x=-2 treatment, as the two actions are of equivalent MGT ordering by Condition A, but vary by Condition B. Firstly, if an action has a higher MGT ordering, then we expect FMs and SMs to perceive that action as being more generous.[9]

> *H1: FMs and SMs will perceive choosing IN as being more generous in the Selfless treatment x=4 than in the Self-Serving x=-2 treatment.*

Secondly, since choosing IN is self-serving for the FM when x=-2, and selfless for the FM when x=4, we expect that more FMs will choose IN when doing so is self-serving:

> *H2: FMs will choose IN more often in the Self-Serving x=-2 treatment than in the Selfless x*=4 *treatment.*

Finally, in the theory of Revealed Altruism a MGT action elicits a MAT response. In our design SMs should therefore allocate a larger proportion of the $20 to the FM ($y$) if choosing IN is indeed MGT.

> *H3: SMs will choose a higher* y *in the Selfless x=4 treatment than in the Self-Serving x=-2 treatment.*

[9] Note that while MGT and MAT orderings are defined as weak relations (see Cox et al., 2008), we formulate our hypotheses as strong inequalities, for a more conservative test of our proposed conjecture.

4.3. Procedures

The experiment was run in the New Zealand Experiment Economics Laboratory at the University of Canterbury. 154 student subjects, recruited using the online recruitment system ORSEE (Greiner 2015), participated, with 74 subjects (= 37 observations) in the Self-Serving x=-2 treatment, and 80 subjects (= 40 observations) in the Selfless x=4 treatment. Subjects participated in one treatment only, making this design between-subjects. Subjects were on average paid NZ$ 18.78, with all sessions lasting approximately 50 minutes.[10] In a session, subjects were checked-in, signed a consent form, and then handed neutrally framed instructions (included in Appendix A). They were given approximately three minutes to read the instructions by themselves, after which the instructions were read aloud while also projected onto a screen at the front of the lab. Subjects made their decisions in a program implemented in z-Tree (Fischbacher 2007). Each terminal was randomly assigned a pair and role by the software. FMs chose IN or OUT by selecting the relevant radio button on the computer screen. If a FM chose OUT, he would receive \$$x$ (x=4 or x=-2 depending on treatment) and the SM would receive nothing. The \$-2 outside option was enforced by reducing the FMs' \$5 show-up fee to \$3, so instead of receiving \$5 in addition to their experiment earnings, they would receive \$3. As the FMs were making their decision, SMs chose how much money to allocate to the FM, y, conditional on the FM choosing IN, i.e. the game was played using the strategy method (Selten 1967, Brandts & Charness 2011). If a FM chose IN, then the division of the \$20 the SM decided on would be enacted. After all subjects had completed their decisions, they were informed they were to receive \$5 for filling out a questionnaire in addition to their show up fee. This previously unannounced payment for filling out the questionnaire was to increase subjects' effort in the questionnaire, which contained the generosity perception elicitation as the first question. Finally, subjects were asked to come to the payout room one by one to receive their earnings in private, and then left the lab. The experimenter was aware of an individual's payout, making the social distance protocol single-blind.

4.4. Results

Table 1 reports summary statistics and tests for both FMs and SMs. Recall that both FMs and SMs were asked on a 5-point scale how generous they thought choosing IN was (with 5 being very generous, and 1 being not generous). H1 predicts that choosing IN when doing so is selfless will be considered more generous than choosing IN when it is self-serving. FMs reported an average generosity perception of 3.83 in the Selfless x=4 treatment, and 3.14 in the Self-Serving x=-2 treatment. The averages are in the direction posited by H1, and the difference is statistically significant with the Mann-Whitney 2-sided test reporting p=.029.[11]

[10] Minimum wage in New Zealand was NZ\$13.50 per hour at the time of the experiments.
[11] We adopt a conservative approach and report 2-sided tests throughout the paper.

SMs reported an average generosity perception of 3.30 in the Selfless x=4 treatment, and 2.73 in the Self-Serving x=-2 treatment. The averages are in the direction posited, and are statistically significant at the 10% level, with the Mann-Whitney test reporting p=.064.[12] Our non-saliently elicited data thus provides evidence for H1, that both FMs and SMs consider choosing IN as being more generous when x=4 than when x=-2, in line with our conjecture.

Table 1 – Experiment 1 Summary Statistics and Tests

Panel A: First Movers				
Treatment	**Fraction that chose IN (percentage)**	**Fisher's exact test**	**Mean Generosity Perception (standard deviation)**	**Mann-Whitney test**
***x*=-2 (Self-Serving)**	35/37 (95%)	.049	3.14 (1.42)	.029
***x*=4 (Selfless)**	31/40 (78%)		3.83 (1.03)	
Panel B: Second Movers				
Treatment	**Mean *y* (standard deviation)**	**Mann-Whitney test**	**Mean Generosity Perception (standard deviation)**	**Mann-Whitney test**
***x*=-2 (Self-Serving)**	5.42 (3.64)	.992	2.73 (1.35)	.064
***x*=4 (Selfless)**	5.45 (3.52)		3.30 (1.11)	

All reported p-values are two-sided.

[12] A Mann-Whitney test on pooled data from both FMs and SMs reports p=.007.

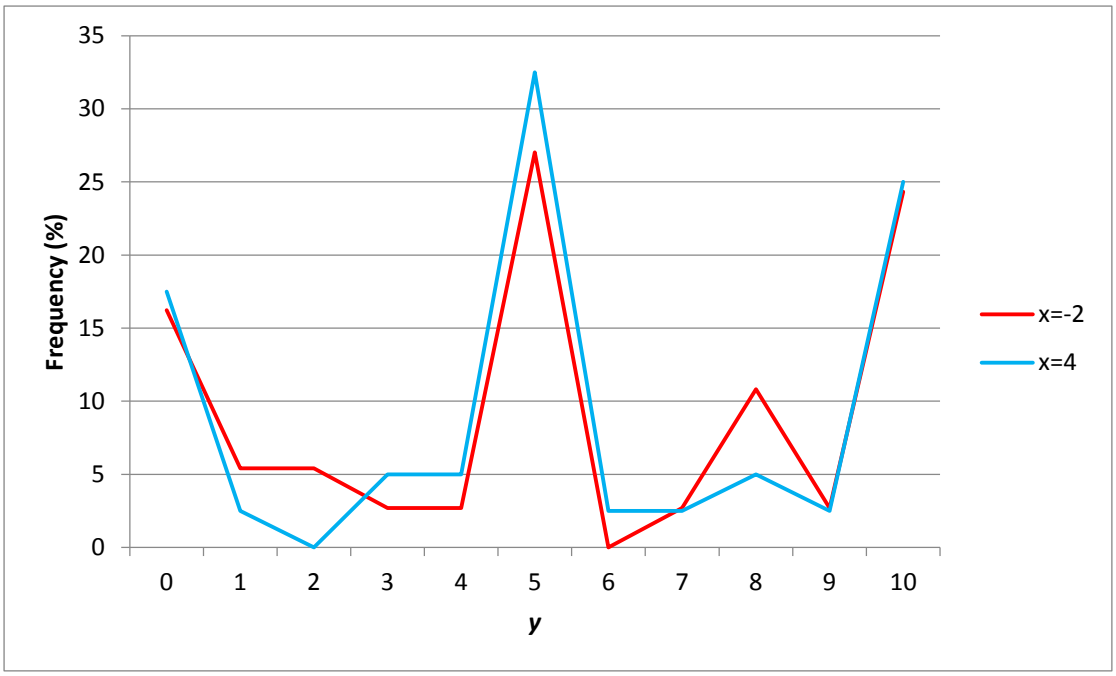

Figure 3 – Experiment 1 SMs *y* decision by treatment

Choosing IN in the current experiment was designed to be self-serving for the FM when x=-2, and selfless for the FM when x=4. H2 therefore predicted that more FMs will choose IN when doing so is self-serving than when it is selfless. Evidence presented in Panel A of Table 1 supports H3 as 95% of FMs chose IN in the x=-2 treatment and 78% of FMs chose IN in the x=4 treatment. This difference is statistically significant according to the Fisher's exact test (p=.049).

Our design thus passes an important manipulation check: Both FMs and SMs consider choosing IN to be more generous in the Selfless x=4 treatment than in the Self-Serving x=-2 treatment. FMs additionally choose IN more often when doing so is self-serving.

We now move onto SM behavior. If SMs consider IN being of differing levels of generosity, i.e. H1 is supported, then our reciprocity conjecture implies that SMs will subsequently be inclined to allocate more to the FM when IN is considered more generous, as predicted by H3.

However, Table 1 reports no evidence in favor of H3. On average, SMs allocate 5.42 to FMs in the Self-Serving x=-2 treatment, and 5.45 in the Selfless x=4 treatment, and there is no statistically significant difference (p=.992). We therefore reject H3 that SMs choose a higher y in the Selfless x=4 treatment than in the Self-Serving x=-2 treatment.

3.5 Discussion

H1supports our conjecture, based on a proposed revision of MGT ordering, as both FMs and SMs perceive our selfless treatment to be more generous than our self-serving treatment. However, H3 does not support our MGT revision, as it has not borne out that an MGT action has elicited a MAT response, an important part of Revealed Altruism. Our finding of evidence in support of H1 but finding a lack of support for H3 is puzzling. SMs consider FMs to be less generous when they choose IN in our self-serving treatment, however, this elicits no difference in reciprocal response towards FMs.

However, the empirical evidence of the LWG has shown that typically varying x does not have an effect on y (Dufwenberg & Gneezy, 2000; Servátka & Vadovič 2009; Cox, Servátka & Vadovič, 2010; and also the no negotiations treatments in Dufwenberg, Servátka & Vadovič, 2017), which seems to indicate a lack of positive reciprocity in this game. In light of this, Experiment 1 was perhaps a too conservative test of our conjecture. Our findings may be an artefact of the LWG itself. Cox, Servátka & Vadovič (2010) hypothesize that the observed lack of positive reciprocity could be driven by the opportunity set of the SM being invariant to the size of the foregone outside option of the FM, a notable difference with the Investment Game (Berg, Dickhaut & McCabe, 1995) where each dollar sent by the FM enlarges the SM opportunity set and where positive reciprocity is one of the driving factors of SM behavior (Cox, 2004). We therefore find it prudent to check the robustness of our findings in a different experimental design.

5. Experiment 2

5.1.Design

For Experiment 2 we use the Investment Game (Berg, Dickhaut & McCabe, 1995). Unlike the Lost Wallet Game, Investment Game experiments have found SM behavior to be responsive to choices made by the FM (Johnson & Mislin, 2011), and will thus present a less conservative test of our conjecture. Recall that in order to test whether self-serving generosity leads to a weaker reciprocal response, we must hold Condition A constant while varying Condition B. One way of achieving this is in the Investment Game is to use different exchange rates on amounts kept by the SM, and amounts returned to the FM by the SM. To implement such exchange rates we adapt the procedures used by Andreoni & Miller (2002) for use in the Investment Game. Figure 4 presents our design.

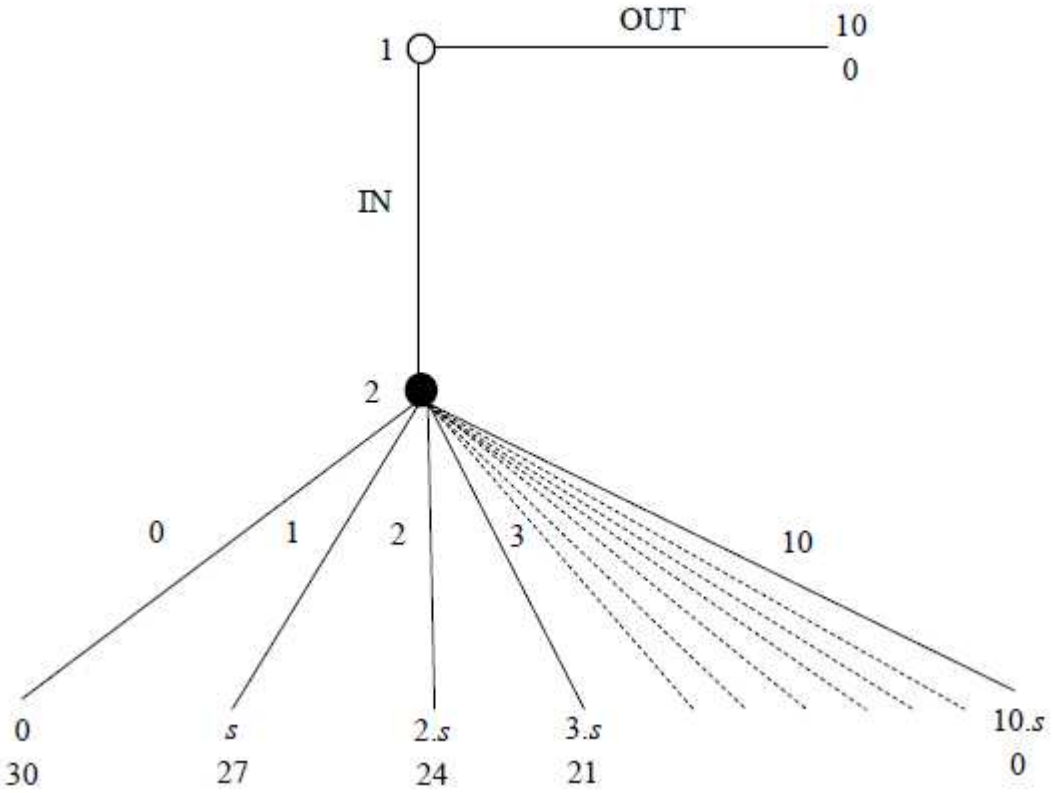

Figure 4: Our Modified Investment Game

The FM starts with ten tokens, and can choose IN or OUT. If the FM chooses OUT, then he earns ten points. If the FM chooses IN, then the ten tokens are made available for the SM to split. In all treatments, tokens that the SM holds for herself earn her three points per token, which holds Condition A constant across treatments.[13] Tokens that the SM decides to send to the FM earn the FM *s* points, depending on treatment. SMs can send any integer amount of tokens from 0 to 10. By varying *s* we control how selfless or self-serving a FM choosing IN is. Note that the 'channel' in which we alter Condition B is different from Experiment 1, where the outside option was varied. The 'channel' is altered as a robustness check of Experiment 1, to avoid the potential artefactual issues of the LWG described previously. Choosing IN when *s*=4 is neither selfless nor self-serving by our definition, as FMs have a maximum potential gain of 30 (they forgo 10 points to choose IN), which is the same as the SM's maximum potential gain of 30 (which is invariant in *s*). Values of *s* smaller than 4 result in the choice of IN being selfless, and values larger than 4 result in the choice of IN being self-serving. We fix our Selfless treatment as *s*=2, and our Self-Serving treatment as *s*=6, to ensure sufficient and equal distance from our what would be 'neutral' *s*=4.

[13] Note the deliberate similarity to the Investment Game, where amounts invested by the FM are typically tripled for the SM to split.

Table 2 – Separation of Effects in Experiment 2

	Fixed *s*=6	Random *s*=2	Random *s*=6
Fixed *s*=2	Selflessness Effect Self-Serving Effect Surplus Maximization	**Selflessness Effect**	Selflessness Effect Surplus Maximization
Fixed *s*=6	---	Self-Serving Effect Surplus Maximization	**Self-Serving Effect**
Random *s*=2	---	---	**Surplus Maximization**

Fixed *s*=2 and *s*=6 represent the respective treatments where *s* is fixed prior to the FM's decision. Random *s*=2 represents the random treatment when *s*=2 eventuated. Random *s*=6 represents the random treatment when *s*=6 eventuated.

Andreoni & Miller (2002) report that in their Dictator Game, a non-trivial number of subjects exhibit a desire to maximize surplus. If SMs behave similarly in our experiment, then they will want to allocate more tokens to FMs when *s*=6, as this maximizes surplus. This is a potential confound, and acts in the opposite direction of our hypothesis. In order to control for this, we implement a third Random treatment where there is a 50% chance *s*=2 eventuates, and a 50% chance *s*=6 eventuates. SMs are familiar with the nature of the FM's decision. They are also informed which outcome has eventuated prior to their own decision, whereas FMs are not. The expected value of *s* is 4, so a FM choosing IN is being neither selfless nor self-serving in the Random treatment.[14] Taking advantage of this fact, and comparing SM reaction to selfless or self-serving FM actions (where *s* is fixed prior to the FM decision to be 2 and 6, respectively) to the corresponding neutral FM actions, we can separate out the confound of surplus maximization, as well as individually identify a SMs response

[14] CFS do not mention moves that are determined by chance, so we assume that MGT ordering is determined by the expected value of m_G^* and y_G^*. Such an assumption seems natural and intuitive, and a similar approach is used by Sebald (2010) to extend Dufwenberg & Kirchsteiger (2004).

to a selfless or self-serving FM action. Table 2 presents this identification of potential effects, with isolated effects in bold.

5.2.Hypotheses

Our conjecture remains the same as in Experiment 1, while our hypotheses change to fit the design of Experiment 2. Both FMs and SMs should perceive our Selfless *s*=2 treatment to be more generous than both our neutral Random treatment and Self-Serving *s*=6 treatment. FMs and SMs should also consider our neutral Random treatment to be more generous than our Self-serving *s*=6 treatment.

H4: FMs and SMs will perceive choosing IN as being more generous:

- *in the Selfless s=2 treatment than in the Random treatment,*
- *in the Random treatment than in the Self-serving s=6 treatment.*

Just as in Experiment 1, we expect the FMs' behavior to correspond to how self-serving or selfless choosing IN is.

H5: FMs will choose IN more often:

- *in the Self-serving s=6 treatment than in the Random treatment,*
- *in the Random treatment than in the Selfless s=2 treatment,*
- *in the Self-serving s=6 treatment than in the Selfless s=2 treatment.*

In terms of SM behavior, we focus our analysis on the isolated effects presented in Table 2. For the selflessness effect, we predict that SMs will allocate more to the FM in the Selfless *s*=2 treatment than when *s*=2 eventuates in the Random treatment. Such a reaction would be triggered by the FM being selfless by choosing IN when *s* is fixed to be 2, as opposed to having neither selfless nor self-serving intentions by choosing IN in the Random treatment. According to our conjecture, SMs may want to reward selflessly generous FMs more than neutrally generous FMs.

An important consideration in our design is that because of the different token redemption rates (which determine the total number of points) between our treatments, the number of tokens allocated to the FM might vary because of the different redemption rate, rather than due to a change in reciprocity. Therefore, rather than stating our hypotheses using the number of tokens allocated to the FM, we formulate them in terms of the percentage of the total surplus.

H6: SMs will allocate more surplus to the FM in the Selfless s=2 treatment than when s=2 eventuates in the Random treatment.

For the self-serving effect, we expect that SMs will allocate more of the surplus to the FM in the Random treatment where *s*=6 eventuates than in the Self-Serving *s*=6 treatment, as SMs may wish to reward neutrally generous FMs more than self-serving generous FMs.

H7: SMs will allocate more surplus to the FM when s=6 eventuates in the Random treatment than in the Self-Serving s=6 treatment.

Finally, for the surplus maximization effect, we compare the two possible states of nature in the Random Treatment. We expect that SMs will allocate more of the surplus to the FM when *s*=6 eventuates, as it maximizes surplus, and it is relatively cheaper to do so.

H8: In the Random treatment SMs will allocate more surplus to the FM when s=6 eventuates than when s=2 eventuates.

5.3.Procedures

Experiment 2 was also run in the New Zealand Experiment Economics Laboratory at the University of Canterbury. 222 subjects participated in total, with 64 subjects (= 32 observations) in the Selfless *s*=2 treatment, 64 subjects (= 32 observations) in the Self-Serving *s*=6 treatment, and 94 subjects (= 47 observations) in the Random treatment. None of the Experiment 2 subjects participated in Experiment 1. Subjects were paid on average NZ$ 17.69, with all sessions lasting approximately 50 minutes. The procedures used in Experiment 2 replicate those of Experiment1. FMs made their IN or OUT decision by selecting the relevant option on their screen. If a FM chose OUT, then he would earn 10 points and his paired SM would earn 0 points. While the FMs were making their decisions, SMs were deciding how many of the 10 tokens to send to the FM and how many to keep for themselves, provided the FM chose IN. Tokens sent to the FM earns him *s* points and tokens kept by the SM earns her 3 points. In the Random treatment, FMs were not informed of the realization of *s* when making their decision. SMs were informed of the realization of *s* before making their decision, and only made a decision for that realization of *s*. If a FM chose IN, then the proposed division of tokens by the SM would be implemented. After all subjects had completed their decisions, they were informed they were to receive $5 for filling out a questionnaire. As in Experiment 1, the additional payment was to increase subject's effort in the questionnaire, of which, the generosity perception elicitation was presented first. After subjects had completed the questionnaires, they were asked to come one by one to the payout room to receive their earnings in private, where the points earned in the experiment were exchanged at the preannounced rate of $.60 NZD per point.

5.4.Results

Table 3 reports summary statistics and statistical tests on both FM and SM behavior in Experiment 2. Because of our focus on isolating individual effects, we provide pairwise comparisons and statistical tests.

Table 3 – Experiment 2 Summary Statistics and Tests

Panel A: First Movers

Treatment	**Fraction that chose IN (percentage)**	**Fisher's exact test**		**Mean Generosity Perception (st. dev.)**	**Mann-Whitney test**	
Selfless *s*=2	14/32 (44%)	.131	.001[a]	3.94 (1.01)	.205	.249[b]
Self-Serving *s*=6	21/32 (66%)		.187	3.59 (1.13)		.702
Random	38/47 (81%)	.001[a]		3.70 (0.95)	.249[b]	

Panel B: Second Movers

Treatment	**Mean Surplus allocated to FM (st. dev.)**	**Mann-Whitney**		**Mean Generosity Perception (st. dev.)**	**Mann-Whitney test**	
Selfless *s*=2	27.0% (19.1%)	.013	.245[c]	4.03 (1.03)	.082	.277[e]
Self-serving *s*=6	40.4% (24.6%)		.142[d]	3.56 (1.13)		.855
Random *s*=2	20.7% (19.7%)	.0001	.245[c]	3.72 (1.19)	.277[e]	
Random *s*=6	50.1% (23.2%)		.142[d]			

In the Selfless treatment s is fixed at 2; in the Self-Serving treatment s is fixed at 6. In the Random treatment s=2 and s=6 eventuates with 50% probability each.

Statistical tests of differences are grouped in the same cell corresponding to the treatments in the same rows. Where this is not possible, p-values are reported twice in the same rows as the corresponding treatments, and paired using a letter superscript.

All reported tests are 2-sided.

We start by exploring subject's non-salient generosity perceptions. H4 predicts that choosing IN in the Selfless treatment will be perceived to be more generous than in the Random treatment, that choosing IN in the Selfless treatment will be perceived to be more generous than in the Self-Serving treatment, and that choosing IN will be perceived to be more generous in the Random treatment than in the Self-Serving treatment.

FMs reported an average generosity perception of 3.94 in the Selfless treatment, 3.70 in the Random treatment, and 3.59 in the Self-serving treatment. While the averages are in the hypothesized directions, there are no statistically significant differences between any of our treatments. We find no evidence in support of H4 from FM behavior.

SMs reported an average generosity perception of 4.03 in the Selfless treatment, 3.72 in the Random treatment, and 3.56 in the Self-Serving treatment. As with FMs, the averages are in the hypothesized direction, but unlike FMs, there exists a weak statistical difference between the Selfless and Self-Serving treatments, with the Mann Whitney test reporting p=.082.[15] Therefore, there is some weak evidence in support of a part of H4, which is the most relevant comparison from the perspective of our research question. Regarding our findings on the differences in generosity perceptions being weaker in Experiment 2 than in Experiment 1, we note that it may be due to the increased complexity of Experiment 2, or the calibration providing a smaller magnitude of separation in generosity perception.

We continue the analysis with the FM behavior. 44% of FMs chose IN in the Selfless treatment, 81% in the Random treatment, and 66% in the Self-Serving treatment. The Fisher's exact test only reports a statistically significant difference in FM behavior between the Random and Selfless treatment (p=.001) with the difference between the Selfless and Self-Serving treatment being marginally insignificant (p=.131). We therefore find some evidence in support of a part of H5.

As mentioned earlier, when analyzing SM decisions, it is helpful to report the percentage of the surplus allocated to the FM instead of the number of tokens for the comparison between treatments with differing token redemption rates. For comparability between all SM results, we report this percentage for all SM token allocations. We now test our hypotheses on SM behavior, starting with isolating the 'selflessness effect'. Hypothesis H6 predicts that SMs will allocate more of the surplus to the FM in the Selfless treatment than in the neutral Random treatment, where the same token redemption rate s=2 eventuates. The intuition behind the hypothesis is that SMs may want to more highly reward those FMs who exhibit selfless generosity, than those FMs who exhibit neutral generosity.

[15] Using pooled data of both FM and SM generosity perceptions, a Mann-Whitney test reports: for H4 p=.073; for H5 p=.035; and for H6 p=.536. Note that such an approach provides support for H5 and mild evidence in support of H4.

In the Selfless *s*=2 treatment, SMs on average allocated 27.0% of the surplus to FMs, while in the Random treatment where *s*=2 eventuated SMs on average allocated 20.7%. The averages are in the hypothesized direction, however, this result is not statistically significant (p=.245). Therefore, we find no evidence in support of H6.

In a similar line of reasoning to the selflessness effect, we hypothesize that SMs will respond by diminishing their reciprocity due to the 'self-servingness' effect as they may not want to reward FMs whose intentions could be self-serving. Hypothesis H7, capturing this effect, predicts that SMs will allocate more surplus in the neutral Random treatment where *s*=6 has eventuated by chance compared to the Self-serving *s*=6 treatment.

In the Self-Serving *s*=6 treatment, SMs on average allocated 40.4% of the surplus to FMs, while in the Random treatment where *s*=6 eventuated SMs on average allocated 50.1%. The averages are in the hypothesized direction, however, the difference is marginally insignificant (p=.142). Therefore, we find no evidence in support of H7.

Our last hypothesis, H8, explores the need for our Random treatment to control for the possibility that SMs may wish to maximize surplus by allocating more to the FM when the redemption rate is higher. To do this, we compare the SM response to *s*=2 and *s*=6 within our Random treatment. As the FM's intentions are constant in the Random treatment, we can focus on the effect of the differing *s*.

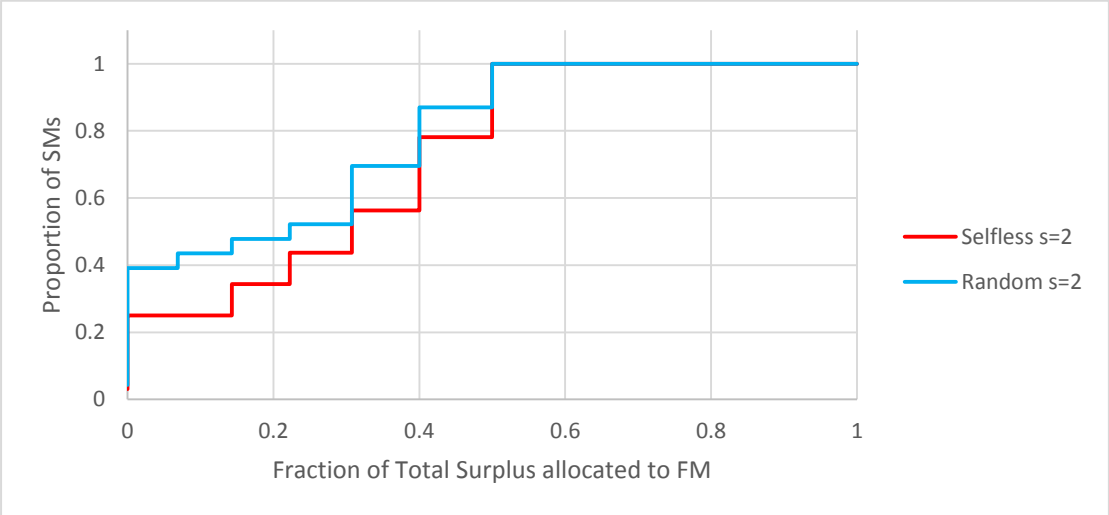

Figure 5 – Experiment 2 - SM surplus allocation decision CDFs when *s*=2 by treatment

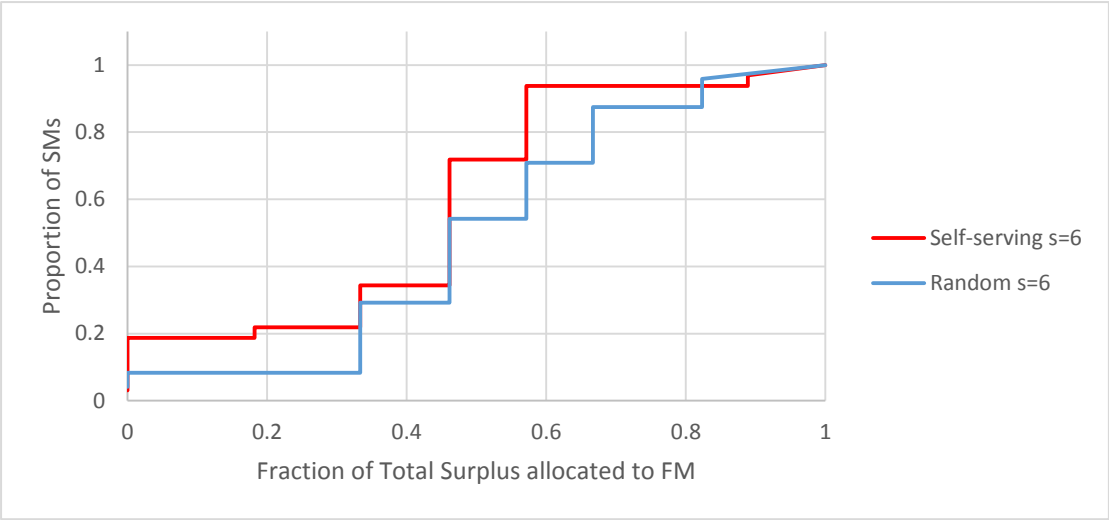

Figure 6 – Experiment 2 - SM surplus allocation decision CDFs when *s*=6 by treatment

When *s*=6 eventuated in the Random treatment, SMs allocated on average 50.1% of the surplus to FMs, whereas SMs allocated on average 20.7% when *s*=2 eventuated. This result is statistically significant (p=.0001), and provides strong evidence for H8. This finding justifies the need for our Random treatment to control for surplus maximization effects. If this effect was not controlled for, a confounded test of a combined selfless and self-serving effect, which directly compares SM behavior between our Selfless and Self-Serving treatment, would report a statistically significant difference (p=.013). To use this as evidence of self-serving generosity affecting reciprocity would be erroneous due to the presence of the surplus maximization effect. Alternatively, to separate out selfless and self-serving effects, one could introduce a treatment where *s*=4 without a random element. Such an approach would also be erroneous due to the presence of the surplus maximization effect.

6. An Incentivized Elicitation of Generosity Perceptions

Recall that in Experiment 1 we found that, despite no difference in the SM's reciprocal response between our treatments, SMs perceived a FM's choice of IN to be of differing levels of generosity. This contrast between generosity perception and reciprocal response warrants further investigation, as it raises into question the fundamentals of the general concept of positive reciprocity, *"... a tendency to respond to perceived kindness with kindness..."* (Sobel, 2005, pp 392). However, the generosity perception elicited in Experiment 1 was not incentivized, so in order to support our findings we conduct an additional generosity perception elicitation.[16]

[16] We thank an anonymous referee for suggesting this additional elicitation.

In addition, our observations from Experiment 2 were that the mean SM perception of generosity matched our conjecture, however (weak) statistical significance was only found for one of the three comparisons. Again, the generosity perceptions were not incentivized, so an additional incentivized elicitation could confirm whether the non-incentivized elicitation was the reason for the lack of statistical significance of these observations. Furthermore, since the generosity perceptions were elicited after the decisions, it is possible that they were not considered at the time when the decisions were made and only prompted by the generosity survey question itself.

We use a within-subject design for the incentivized elicitation, i.e. we ask subjects to consider the generosity of a FM choosing IN of both the Self-Serving x=-2 and Selfless x=4 treatments of Experiment 1, as well as the Self-Serving s=6, Random, and Selfless s=2 treatments of Experiment 2. As subjects see all treatments in the incentivized elicitation it alleviates potential concerns that by chance a group of subjects in one treatment have different ratings tendencies than another. At the same time, it is possible that an 'experimenter demand effect' may be present, in that subjects would change their generosity perceptions based on what changes in the treatment.[17] In order to minimize this, we ask subjects to consider the generosity perception of the subjects that participated in Experiment 1 and 2, who did not observe the other treatments, and this point is emphasized in the instructions.[18] Subjects' answers were incentivized based on how closely they could guess the average generosity perception using a quadratic scoring rule. Subjects were asked to consider only the SM's generosity perceptions, as it is the SM's perceptions that are important for the reciprocal response. Subjects were also read the instructions from Experiments 1 and 2, and were asked to complete incentivized control questions, to ensure their understanding of the environment that the previous subjects participated in.

55 subjects, recruited through ORSEE (Greiner 2015), participated in the incentivized elicitation of generosity perceptions, which was conducted in the Vernon L. Smith Experimental Economics Laboratory at the Macquarie Graduate School of Management.[19] Subjects were on average paid AU$ 23.94 and sessions lasted under 2 hours. In a session, subjects were informed that they would be considering the generosity perceptions of the subjects that had participated in the previous experiments, and were first presented with Experiment 1. The instructions from Experiment 1 were read aloud, which was followed by control questions where subjects could earn AU$ 0.50 for each correct response. Then, subjects were asked to guess the average generosity perception of SMs that participated in Experiment 1 in each different treatment. They were rewarded for this guess based on a quadratic scoring rule of the form: $20 - 15 * (guess - actual)^2$. This process was repeated for Experiment 2, the instructions were read aloud, incentivized control questions

[17] For a further discussion of the experimenter demand effect, see Zizzo (2010).
[18] The instructions for the robustness check are included in Appendix C.
[19] As the elicitation is run using a within-subject design rather than being directly compared to earlier treatments, different subject pools should not play a role.

were answered, and then the generosity perception guesses were elicited. At the end of the session, one of the guesses was randomly selected to be actually paid out, which was known to subjects before making their guesses, in order to control for a portfolio effect.

The results of the incentivized elicitation are presented below in Table 4, with the results pertaining to Experiment 1 in Panel A, and Experiment 2 in Panel B. As the design was within-subject, the data are paired and thus the Wilcoxon signed-rank test was used. As Table 4, Panel A, shows, subjects in the robustness check were, on average, able to predict the average generosity perception of the previous subjects reasonably well in Experiment 1. There is evidence of a difference of generosity perceptions between the x=-2 and x=4 treatments at the 10% level, providing additional support for H1. Panel B reports that subjects on average did not guess that the previous subjects in Experiment 2 perceived the different treatments as being of different levels of generosity. One might think that this result could be driven by confusion or a lack of understanding of the more complicated environment of Experiment 2, especially considering subjects in the incentivized elicitation did not actually participate in the original experiment. A lack of understanding of instructions of Experiment 2, however, was likely not an issue. Panel C presents data from only those subjects who scored above 80% on the Experiment 2 control questions, as these subjects are more likely to have understood the environment. However, this restriction does not change the overall findings. Recall that in Experiment 2, amongst SMs there was only a statistically significant difference between the s=2 and s=6 treatments at the 10% level, so the results of incentivized elicitation do not differ substantially from the original experiment.

Taken together, generosity perceptions in both Experiment 1 and the additional incentivized generosity perceptions point out that choosing IN was perceived to be less generous when x=-2 than when x=4, as suggested by our accepted definitions of self-serving and selfless generosity, confirming that the Lost Wallet Game is indeed an appropriate environment to test our conjecture.

While the design of Experiment 2 employs the same definitions and therefore ex ante satisfies the theoretical requirements for testing our conjecture, we find only a weak statistical difference in generosity perceptions between the Selfless and Self-Serving treatments in the actual experiment and no statistical differences in the robustness check. As hypothesized earlier, the increased complexity of the Investment Game environment or its calibration might make the differences in generosity levels (of choosing IN) less salient in the existing treatments.

Importantly, however, the results of Experiment 2 confirm those of Experiment 1 in that self-serving generosity does not lead to weaker reciprocity than selfless generosity. Thus from the statistical point of view this corroborated evidence increases the confidence in our findings compared to a situation if the data were generated in one environment only.

Table 4 – Incentivized Generosity Perceptions Statistics and Tests

Panel A: Experiment 1				
Treatment	**Actual Average**	**Guess Average**	**Wilcoxon test**	
***x*=-2**	2.73	2.79	p=.080	
***x*=4**	3.30	3.16		
Panel B: Experiment 2 (Full sample)				
Treatment	**Actual Average**	**Guess Average**	**Wilcoxon test**	
***s*=2**	4.03	3.24	p=.903	p=.622[a]
***s*=6**	3.56	3.21		p=.815
Random	3.72	3.21	p=.622[a]	
Panel C: Experiment 2 (Restricted sample)				
Treatment	**Actual Average**	**Guess Average**	**Wilcoxon test**	
***s*=2**	4.03	3.29	p=.744	p=.712[a]
***s*=6**	3.56	3.17		p=.182
Random	3.72	3.36	p=.712[a]	

Statistical tests of differences are grouped in the same cell corresponding to the treatments in the same rows. Where this is not possible, p-values are reported twice in the same rows as the corresponding treatments, and paired using a letter superscript.

All reported tests are 2-sided.

7. Conclusion

Life is full of examples where people pretend to be kind, but do so because their exhibited kindness has the potential to benefit them. Do beneficiaries of such kind actions care about their self-serving nature and take it into account when responding? Our intuition, supported by prior empirical evidence on the importance of intentions, tells us they might care. We set out to study whether self-serving generosity, which is a particular type of kindness, affects reciprocal behavior. The novelty of our approach lies in manipulating the nature of intentions (as opposed to only removing them), which is central to understanding of reciprocal preferences.

Utilizing the framework of Revealed Altruism, we developed a conjecture on how selfless and self-serving generosity impacts reciprocal behavior. We defined actions that satisfied Revealed Altruism's Condition B to be selfless, and actions that violated Condition B to be self-serving. We proposed that self-serving but generous actions are

less generous than selfless actions, and should therefore elicit a diminished reciprocal response. Using novel designs that varied whether an action was selfless or self-serving while holding other generosity considerations constant, we found no difference in reciprocal response to selfless and self-serving (but equally generous) offers. This is despite the fact that subjects generally considered our selfless and self-serving treatments to be of differing levels of generosity.

Most theories of (positive) reciprocity can generally be condensed down to a 'primitive', a desire to reward generous actions. It follows that the desire to reward would increase with how generous the action is; meaning our finding of differences in generosity perception (in particular in Experiment 1) but not reciprocal behavior is puzzling. Our original elicitation of subjects' generosity perception was non-salient, however, we replicated the differences in generosity perception with an elicitation utilizing a salient proper scoring rule (see Schlag, Tremewan & van der Weele, 2015, for a review). Our findings suggest there is more work to be done on the channels through which reciprocity operates, in order to fully understand this important economic phenomenon.

SM behavior in our experiments may be explained by SMs giving FMs the 'benefit of the doubt', and assuming FMs have selfless rather than self-serving intentions when both could be present, a finding akin to Cox & Deck (2006). A design where FMs must choose a self-serving generous option over a selfless or neutral generous option could control for the 'benefit of the doubt'. However, such a design would likely require the use of 'inefficient strategies' (Dufwenberg & Kirchsteiger, 2004), characterized by the existence of another strategy that for at least one player increases material payout without reducing the payout of other players. In such a design, our conjectured effects would be confounded by inefficient alternatives potentially not being considered credible.

Apart from providing empirical evidence that people do not seem to respond to self-serving intentions, our results have important theoretical implications. In particular, our data suggest that Revealed Altruism might not need the restriction of Condition B in a MGT ordering, as it appears to have no impact on the MAT response and that a 'MGT light' ordering (proposed by CFS, p. 36), which only includes Condition A, may be sufficient. Such a refinement of Revealed Altruism would increase the parsimony of the theory, without reducing its descriptive and predictive properties. A SM appears to only consider what a generous action means for her own payoff, and this consideration appears to dominate any ulterior intentions a FM may have. However, we also acknowledge that more research would be required to confidently remove Condition B from Revealed Altruism, such as robustness checks over different sets of parameters and environments (e.g. consider a situation when the FM could gain \$1M while the SM only \$1), investigating Condition B's effects over negative reciprocity, and investigating the potential interaction effects of Conditions A and B.

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Appendix A – Experiment 1 Instructions

Instructions in ( ) are relevant to the x=-2 treatment, and [ ] to the x=4 treatment.

No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Show up Fee
Every participant will get $5 as a show up fee, and in addition you may earn money in the experiment. All the money will be paid to you in cash at the end of the experiment.

Anonymity
You will be divided randomly into two groups, called Group 1 and Group 2. Each person in Group 1 will be anonymously paired with a person in Group 2. No one will learn the identity of the person he/she is paired with.

Structure of the experiment
This experiment is computerised, meaning you will be entering your decisions on the computer in front of you. If you have any trouble entering your decisions, please raise your hand to alert the experimenter who will assist you.

The Group 1 Decision Task

Each person in Group 1 will have two options:

- (To choose OUT and receive $-2, which will be subtracted from their show up fee.) [To choose OUT and receive $4.] In this case the paired Group 2 person with whom he/she is paired makes no decision.
- To choose IN. In that case the paired person in Group 2 will get to split $20 between the pair. That is, the person in Group 2 will decide how much of the $20, between $0 and $20, to give to the person in Group 1, and how much to keep.

Group 1 persons enter their decisions by selecting the relevant option on the screen, followed by clicking OK.

The Group 2 Decision Task

If the Group 1 person chooses IN, then $20 will be made available to split between the two paired persons. The split will be determined by the Group 2 person. Each Group 2 person will be asked to decide how much money out of $20 to give to the Group 1 person with whom he/she is paired. Group 2 persons are asked to enter their decision in the relevant text box followed by clicking OK. Note that this decision by the Group 2 person will only be relevant if the Group 1 person chose IN.

Payment of Show up Fees and Experiment Earnings
All participants are asked to sit patiently until the end of the experiment. Once all Group 2 persons have made their decisions, you will be presented with a summary screen of your earnings. Click OK after you have seen this screen, so other

participants cannot see your decisions. You will then be prompted to complete a Questionnaire. After the Questionnaire, you will be asked one by one to enter the payment room at the back of the lab for the payment of your earnings. Because your decision is private, we ask that you do not tell anyone your decision or your earnings either during or after the experiment. We also ask you to not gather near the lab after you receive your payment.

Are there any questions?

Appendix B – Experiment 2 Instructions

(Fixed Treatments)

No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Show up Fee
Every participant will get $5 as a show up fee, and in addition you may earn money in the experiment. All the money will be paid to you in cash at the end of the experiment.

Anonymity
You will be divided randomly into two groups, called Group 1 and Group 2. Each person in Group 1 will be anonymously paired with a person in Group 2. No one will learn the identity of the person he/she is paired with.

Structure of the experiment
This experiment is computerised, meaning you will be entering your decisions on the computer in front of you. If you have any trouble entering your decisions, please raise your hand to alert the experimenter who will assist you.

Tokens and Points
The currency used in this experiment are Tokens. As you make decisions with these Tokens, you and your paired person will earn points. Every point that people earn in this experiment will be worth 60 cents. For example, if you earn 8 points you will make $4.80 from the decision part of the experiment.

The Group 1 Decision Task

Each person in Group 1 will have two options:

- To choose OUT and receive 10 Tokens, earning 10 points. In this case the paired Group 2 person with whom he/she is paired makes no decision, and earns 0 points.
- To choose IN. In that case the paired person in Group 2 will get to split 10 Tokens between the pair. That is, the person in Group 2 will decide how many of the 10 Tokens, to pass to the person in Group 1, and how many to hold for themselves. Tokens that are passed or held will earn different amounts of points, which is explained in the Group 2 Decision Task.

Group 1 persons enter their decision by selecting the relevant option on the screen, followed by clicking OK.

The Group 2 Decision Task

If the Group 1 person chooses IN, then 10 Tokens will be made available to split between the two paired persons. The split will be determined by the Group 2 person. Each Group 2 person will be asked to decide how many Tokens out of 10 to pass to the Group 1 person with whom he/she is paired, and how many Tokens to hold for

themselves. Each Group 2 person must distribute all 10 Tokens, that is, the number of Tokens they pass and the number of Tokens they hold must sum to 10.

- Tokens that are passed will earn their paired Group 1 person **s** points per Token.
- Tokens that are held (i.e. the remainder of the 10 Tokens that are not passed) will earn the Group 2 person **3** points per Token.

Group 2 persons enter their decisions in the relevant text box, followed by clicking OK. Note that this decision by the Group 2 person will only be relevant if the Group 1 person chose IN.

Payment of Show up Fees and Experiment Earnings
All participants are asked to sit patiently until the end of the experiment. Once everybody has made their decisions, you will be presented with a screen instructing you to wait. Do not click OK until the experimenter asks you to do so. You will then answer a questionnaire, followed by a summary of your earnings, and finally another questionnaire. Once this is complete, you will be asked one by one to enter the payment room at the back of the lab for the payment of your earnings. Because your decision is private, we ask that you do not tell anyone your decision or your earnings either during or after the experiment. We also ask you to not gather near the lab after you receive your payment.

Are there any questions?

(Random Treatment)

No Talking Allowed
Now that the experiment has begun, we ask that you do not talk. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in private.

Show up Fee
Every participant will get $5 as a show up fee, and in addition you may earn money in the experiment. All the money will be paid to you in cash at the end of the experiment.

Anonymity
You will be divided randomly into two groups, called Group 1 and Group 2. Each person in Group 1 will be anonymously paired with a person in Group 2. No one will learn the identity of the person he/she is paired with.

Structure of the experiment
This experiment is computerised, meaning you will be entering your decisions on the computer in front of you. If you have any trouble entering your decisions, please raise your hand to alert the experimenter who will assist you.

Tokens and Points

The currency used in this experiment are Tokens. As you make decisions with these Tokens, you and your paired person will earn points. Every point that people earn in this experiment will be worth 60 cents. For example, if you earn 8 points you will make $4.80 from the decision part of the experiment.

The Group 1 Decision Task

Each person in Group 1 will have two options:

- To choose OUT and receive 10 Tokens, earning 10 points. In this case the paired Group 2 person with whom he/she is paired makes no decision, and earns 0 points.
- To choose IN. In that case the paired person in Group 2 will get to split 10 Tokens between the pair. That is, the person in Group 2 will decide how many of the 10 Tokens, to pass to the person in Group 1, and how many to hold for themselves. Tokens that are passed or held will earn different amounts of points, depending on which Situation occurs, which is explained in the Group 2 Decision Task.

Group 1 persons will not be informed which Situation has occurred prior to making their decision. Group 1 persons enter their decision by selecting the relevant option on the screen, followed by clicking OK.

The Group 2 Decision Task

If the Group 1 person chooses IN, then 10 Tokens will be made available to split between the two paired persons. The split will be determined by the Group 2 person. Each Group 2 person will be asked to decide how many Tokens out of 10 to pass to the Group 1 person with whom he/she is paired, and how many Tokens to hold for themselves. Each Group 2 person must distribute all 10 Tokens, that is, the number of Tokens they pass and the number of Tokens they hold must sum to 10.

The software will generate a random number to determine which Situation will occur. There is a 50% chance of Situation A occurring, and a 50% chance of Situation B occurring.

If **Situation A** occurs, then tokens will earn points in the following way:

- Tokens that are passed will earn their paired Group 1 person **2** points per Token.
- Tokens that are held (i.e. the remainder of the 10 tokens not passed) will earn the Group 2 person **3** points per Token.

If **Situation B** occurs, then tokens will earn points in the following way:

- Tokens that are passed will earn their paired Group 1 person **6** points per Token.
- Tokens that are held (i.e. the remainder of the 10 tokens not passed) will earn the Group 2 person **3** points per Token.

Group 2 persons will be informed which Situation has occurred, and then asked to enter their decisions in the relevant text boxes, followed by clicking OK. Note that

this decision by the Group 2 person will only be relevant if the Group 1 person chose IN.

Payment of Show up Fees and Experiment Earnings

All participants are asked to sit patiently until the end of the experiment. Once everybody has made their decisions, you will be presented with a screen instructing you to wait. Do not click OK until the experimenter asks you to do so. You will then answer a questionnaire, followed by a summary of your earnings, and finally another questionnaire. Once this is complete, you will be asked one by one to enter the payment room at the back of the lab for the payment of your earnings. Because your decision is private, we ask that you do not tell anyone your decision or your earnings either during or after the experiment. We also ask you to not gather near the lab after you receive your payment.

Are there any questions?

Appendix C – Generosity Perceptions Elicitation Instructions

Instructions

Welcome to the experiment. Now that the experiment has begun, we ask that you do not talk. If you have a question, please raise your hand and the experimenter will approach you and answer your question in private.

Every participant will get $5 as a show up fee, and in addition you can earn additional money in the experiment depending on your decisions. All the money will be paid to you in cash at the end of the experiment.

Structure:

There are two parts to this experiment. In both parts you will be considering subjects who have participated in previous experiments, and asked to guess their responses to certain questions. You will be given the instructions that those subjects received, but you will not be participating directly in the situation those instructions describe. The subjects who participated in the previous experiments were undergraduate students recruited for an economic experiment in a similar way that you have been recruited for this session today.

Part 1:

Please refer to the instructions contained in the envelope marked '1'. These are the instructions that subjects received, but with $\boldsymbol{X}$ in the place of the actual dollar amounts. Two different experiments were run, where either $\boldsymbol{X = -2}$, or $\boldsymbol{X = 4}$. A subject in the $X = -2$ experiment did not know that X was 4 in another experiment (and vice versa), or even that X was to be changed in another experiment.

Subjects in one experiment did not participate in the other experiment, or in any of the other experiments described in Part 2.

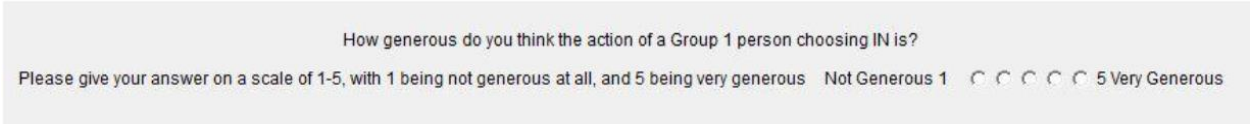
Please read the instructions from envelope 1 now. **We ask you to answer some comprehension questions** to ensure your understanding of the situation the instructions describe. **For every one of these questions you answer correctly, you will receive an additional $0.50.**

Part 1 (continued):

Subjects made their decisions as described in the instructions, but did not observe the outcome. In other words, Group 1 people did not see their paired Group 2 person's split, and Group 2 people did not see whether their paired Group 1 person chose IN or OUT. After all subjects had made their decisions, the following announcement was verbally made:

"The decision part of the experiment is over. We now ask you to answer a couple of questionnaires for which we will pay you an additional $5. Please answer the questions as accurately as possible. You will be asked some questions about the decisions you just made, then a summary of your earnings will appear."

The first question they were asked was presented in the following way:



We ask you to guess what you think the average response by Group 2 people to this question was.

The average is calculated by adding up each value for each subject (which takes the value 1 if they filled in the leftmost circle, 2 if they filled in the second circle, 3 if they filled in the center circle, 4 if they filled in the fourth circle, and 5 if they filled in the rightmost circle), and then dividing by the total number of subjects. For example, if there were 3 subjects, and they selected values corresponding to 2, 1 and 5, then the value you would be trying to guess would be: $\frac{2+1+5}{3} = 2.67\ (2\ decimal\ places)$.

You may enter any number between 1.00 and 5.00, up to two decimal places. You will be rewarded for the accuracy of your guess of the average response of the subjects who had previously participated in this experiment. Your guess will be rewarded in the following way:

$$Your\ payoff = 20 - 15 \times (your\ guess - average\ response\ of\ previous\ subjects)^2$$

You cannot earn a negative payoff; if the above formula returns a negative number your payoff for that guess will be zero. For your convenience, a non-exhaustive table of payoffs is given in Table A as a function of how accurate your guess is, as well as Graph A, which graphically illustrates the above function.

It is important that you understand how your payoff depends on errors.

Note, the closer your guess is to the average response of the Group 2 subjects to the above question, the higher payoff you will receive for your guess. An exact guess will earn you $20, but any errors will reduce your payoff, and larger errors will reduce your payoff by increasingly larger amounts. If your error is very large, then your payoff for that guess will be zero.

We ask you to consider the responses of Group 2 subjects in the $X = -2$ experiment separately from those in the $X = 4$ experiment, so you have 2 guesses to make for this part. Remember, that subjects in one experiment did not know about the other experiment, and only saw the instructions with either $X = -2$ or $X = 4$, not both.

In today's session, we ask you to make 5 guesses in total between Part 1 (2 guesses) and Part 2 (3 guesses). However, for your final payoff, **we will randomly select only one of your 5 guesses to add to your payoff** for the guessing tasks. Therefore do your best and make each guess carefully as any of them could determine your payoff.

Part 2:

Please refer to the instructions in the envelope marked '2'. These are the instructions that the previous subjects received, with changes which will be described in the following sentences. Three experiments were run, which had different instructions relating to the yellow and green highlighted text present in your copy of the instructions. The non-highlighted text was the same across all experiments. Two of the experiments had only the yellow highlighted text, and not the green highlighted text, with either $\boldsymbol{Y = 2}$ or $\boldsymbol{Y = 6}$. The third experiment had only the green highlighted text, and not the yellow highlighted text. We will refer to these three experiments as **Yellow Y=2, Yellow Y=6, and Green** respectively.

Subjects in the Yellow experiments did not know that Y changed between the Yellow experiments, nor were they aware of the Green experiment. Similarly, subjects in the Green experiment were not aware of either of the Yellow experiments.

Subjects only participated in one experiment, and did not participate in any of the experiments described in Part 1.

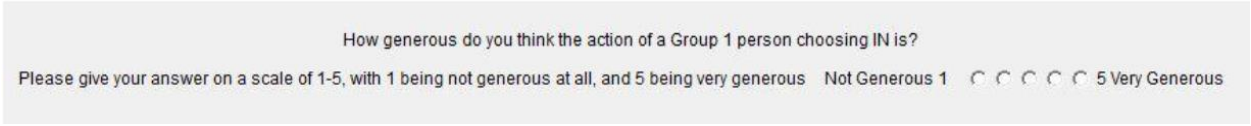
Please read the instructions from envelope 2 now. **We ask you to answer some comprehension questions** to ensure your understanding of the situation the instructions describe. **For every one of these questions you answer correctly, you will receive an additional $0.50.**

Part 2 (continued):

Subjects made their decisions as described in the instructions, but did not observe the outcome. In other words, Group 1 people did not see their paired Group 2 person's split, and Group 2 people did not see whether their paired Group 1 person chose IN or OUT. After all subjects had made their decisions, the following announcement was verbally made:

"The decision part of the experiment is over. We now ask you to answer a couple of questionnaires for which we will pay you an additional $5. Please answer the questions as accurately as possible. You will be asked some questions about the decisions you just made, then a summary of your earnings will appear."

The first question they were asked was presented in the following way:



We ask you to guess what you think the average response by Group 2 people to this question was.

The average is calculated by adding up each value for each subject (which takes the value 1 if they filled in the leftmost circle, 2 if they filled in the second circle, 3 if they filled in the center circle, 4 if they filled in the fourth circle, and 5 if they filled in the rightmost circle), and then dividing by the total number of subjects. For example, if there were 3 subjects, and they selected values corresponding to 2, 1 and 5, then the value you would be trying to guess would be: $\frac{2+1+5}{3} = 2.67\ (2\ decimal\ places)$.

You may enter any number between 1.00 and 5.00, up to two decimal places. You will be rewarded for the accuracy of your guess of the average response of the subjects who had previously participated in this experiment. Your guess will be rewarded in the following way:

$$Your\ payoff = 20 - 15 \times (your\ guess - average\ response\ of\ previous\ subjects)^2$$

You cannot earn a negative payoff; if the above formula returns a negative number your payoff for that guess will be zero. For your convenience, a non-exhaustive table of payoffs is given in Table A as a function of how accurate your guess is as well as Graph A, which graphically illustrates the above function.

It is important that you understand how your payoff depends on errors.

Note, the closer your guess is to the average response of the Group 2 subjects to the above question, the higher payoff you will receive for your guess. An exact guess will earn you $20, but any errors will reduce your payoff, and larger errors will reduce your payoff by increasingly larger amounts. If your error is very large, then your payoff for that guess will be zero.

We ask you to consider the responses of Group 2 subjects in the Yellow Y=2 experiment separately from the Yellow Y=6 experiment and the Green experiment, so you have 3 guesses to make for this part. Remember, that subjects in one experiment did not know about the other

experiments, and only saw one set of the instructions (either the Yellow Y=2, Yellow Y=6 or Green).

In today's session, we ask you to make 5 guesses in total between Part 1 (2 guesses) and Part 2 (3 guesses). However, for your final payoff, **we will randomly select only one of your 5 guesses to add to your payoff** for the guessing tasks. Therefore, do your best and make each guess carefully as any of them might determine your payoff.

Supplementary Material

Table A:

Error	0	.1	.2	.5	.75	.95	1	1.2	2
Payoff	20.00	19.85	19.4	16.25	11.56	6.46	5.00	0	0

Graph A: