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Abstract

Both the law and culture distinguish between acts of commission that overturn the status quo and acts of omission that uphold it. This distinction is of central importance when it comes to reciprocal actions. A stylized fact of everyday life is that acts of commission elicit stronger reciprocal responses than do acts of omission. We report experiments that directly test whether this stylized fact characterizes behavior in controlled experiments. We compare reciprocal responses to both types of acts in experiments using binary, extensive form games. Across three experiments, we examine the robustness of our results to different ways in which the status quo can be induced in experiments. The data show a clear difference between effects of acts of commission and omission by first movers on reciprocal responses by second movers.

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1. Introduction

Does it make a difference whether a bad or good outcome results from an act of commission or an act of omission by another person?[1] In this paper we compare reciprocal responses to acts of commission, that actively impose harm or kindness, and acts of omission which represent failures to prevent harm or to act kindly. We use three experiments to test a hypothesis that acts of commission induce stronger reciprocal responses than comparable acts of omission.

Each experiment has two treatments in which we compare the behavior in two games that vary in their initial endowments, which creates the distinction between the first mover's acts of commission that alter the initial endowments and acts of omission that keep them unaltered. Importantly, we keep the terminal payoffs in both games identical. This gives us a clean test of the empirical significance of opportunities and payoffs that result from acts of commission that change the status quo versus acts of omission that preserve it.

To investigate reciprocal preferences, we focus on what happens after a first mover chooses to uphold or overturn the status quo, that is, what is the reaction of another person to this choice. Data from the experiment provide support for the importance of discriminating between acts of commission and omission by a first mover in theoretical modeling of reciprocal behavior.

2. Relationship to the Literature

We complement several established streams of literature. The work of psychologists has focused on the *omission bias* which occurs when individuals judge harmful commissions, such as igniting a fire, as worse than the corresponding harmful omissions, such as failing to extinguish or report a fire. Spranca, Minsk, and Baron (1991), in a series of hypothetical payoff experiments employing multiple decision scenarios, find that subjects' ratings are associated with judgments that omissions do not cause outcomes.

[1] For example, a waiter may be rewarded with an extremely large tip for going out of his way to serve a customer but might not be punished with a small tip for choosing not to fulfill an extraordinary request.

One of the proposed explanations for the omission bias is loss aversion (Tversky and Kahneman, 1992) and the closely related phenomenon of status quo bias (see e.g., Samuelson and Zeckhauser, 1988; Kahneman, Knetsch, and Thaler, 1990). If the status quo is perceived as a reference point then individuals might be motivated to maintain the status quo in order to avoid possible losses from overturning it. Baron and Ritov (1994) explore this conjecture and argue that only part of the omission bias can be attributed to loss aversion. In another study, Ritov and Baron (1995) examine the connection between omission bias and anticipated regret. Because regret is triggered by relative disadvantages resulting from actions rather than inactions, it imposes a natural psychological cost to acts of commission.

The common feature of these psychology experiments is that they involve a single decision maker whose choices do not affect others. Moreover, responses in these studies do not have economic consequences. This stands in sharp contrast to our experiments in which interactions between pairs of subjects have economic consequences for both individuals.

A series of recent papers examine the omission bias in the context of decisions affecting others and thus invoking social preferences. Hayashi (2013) finds that omission bias tends to be "self-serving." In his experiment, dictators who were randomly assigned favorable endowments are less willing to reallocate money toward the recipient than when the initial endowment was less favorable. In contrast, Gärtner and Sandberg (2014) find no omission bias in their experiment and argue that much of this effect could be attributed to preference for default options. Grossman (2014) studies decisions of dictators who could choose to remain uninformed about the payoff consequences of their actions for the matched recipients. He finds that subjects strongly respond to default options – i.e., whether the default is set on revealing the recipient's payoffs or keeping them hidden.

Our paper is concerned with reciprocal behavior, not dictator game behavior. And we experiment with reciprocal behavior in a context in which there is no default option: first movers must make choices, as must second movers.

While the economics literature recognizes intentions to be a driving factor for both positive reciprocity (Cox, Sadiraj, and Sadiraj, 2008; Falk, Fehr, and Fischbacher, 2008) and negative reciprocity (Blount, 1995; Offerman, 2002), the typical experimental designs focus on the "proof of concept" that intentions matter, rather than on identifying conditions under which the intent behind actions is revealed. The common element of such designs is that they allow for the presence of intentions in one condition and remove their presence in the control condition by either implementing the choice of the "first mover" exogenously by the experimenter (e.g., Cox, 2004), using a randomizing device (e.g., Cox and Deck, 2005), or by forcing a particular choice through limiting the choice set to a singleton (e.g., McCabe, Rigdon, and Smith, 2003).

Bruni, Corazzini, and Stanca (2009) vary the nature of intentions via withholding information that there is a second stage of the game from their subjects. Their experiment employs a two-stage game in which a first mover chooses how much of his 20-token endowment to send to a second mover. The amount sent is multiplied by 3 whereas the amount kept remains unchanged. In the second stage, the second mover faces an identical decision using his own endowment. When the first mover does not know that there is the second stage, the motivation for his generosity is purely intrinsic. However, when the first mover knows that the second mover can reciprocate his generous action, the first mover's motivation can be intrinsic or extrinsic. Bruni et al. find that the second movers respond to possible motivation behind the first movers' generosity and, consistently with the previous literature, reward them more when extrinsic motives can be ruled out.

In a related study Brandts and Solà (2001) study the importance of perceived intentions and distribution of outcomes. Their experiment consists of a series of mini-ultimatum games, in which the proposer has only two options. One of these options is held fixed at (380, 80), while the other option systematically (i) increases/decreases the equality of payoffs with respect to the fixed benchmark and (ii) varies whether the higher payoff goes to the proposer or the recipient. The rejection rates for the fixed benchmark are the lowest when the foregone option gave a lower payoff to the recipient than the benchmark and the highest when the foregone option gave the recipient a higher payoff than to the proposer and the payoffs were less asymmetric in terms of their equality than the fixed benchmark. In contrast to Brandts and Solà, our experimental games differ in terms of the status quo but keep the monetary payoffs at terminal nodes the same.

There are some previous papers that suggest the relevance of the distinction between acts of commission and omission in reciprocal relationships. For instance, in the labor market relationship, a wage increase is reciprocated more strongly when it is an active decision of the firm rather than a higher legal minimum wage (Charness, 2004). In the Stackelberg mini-game (Cox, Friedman, and Sadiraj, 2008), the Leader's choice of quantity is binary and the feasible set varies with treatment: $q_L = \{6, 9\}$ or $q_L = \{9, 12\}$. After learning about the Leader's quantity decision, the Follower then chooses from the set $q_F = \{5, 6, \ldots, 11\}$. By making a given output choice by the Leader be the smaller in one situation (hence more generous to the Follower) and in another situation be the larger one (hence less generous), this design allows for a joint test of effects of reciprocity and status quo but does not separately identify the effects of acts of commission or omission.[2]

3. Experimental Design and Protocol

We first explain the abstract form of the game and, subsequently, explain alternative economic implementations of the game. In what follows we use game trees to represent the games, however it is important to note that in our experiments *subjects were not shown game trees*. Subject instructions and response forms that show exactly how the games were presented to the subjects are contained in electronic supplementary material.

3.1 Abstract Game Tree

All of our experimental treatments involve the game that can be represented by the tree diagram in Figure 1a. In the ordered pairs of payoffs (*a*,*b*) at the terminal nodes, the number *a* is the dollar payoff of Player A and the number *b* is the dollar payoff of Player B. Player A chooses Left or Right at the top node. If Player A chooses Left then Player B has a feasible set with two (ordered pairs of) payoffs, both of which favor Player A. If Player A chooses Right then one of the two (ordered pairs of) payoffs is the equal split where each player gets 10.

Player A may choose Left or Right based on her evaluation of the four alternative ordered pairs of payoffs at the terminal nodes and her expectations about Player B's behavior. Player B may make his choice between Left or Right on each branch solely on the basis of his evaluation of the payoffs on that branch, as predicted by purely

[2] Other joint tests for effects of reciprocity and status quo are reported in Cox, et al. (2009), Cox and Hall (2010), and Cox, Ostrom, Sadiraj, and Walker (2013).

consequentialist models of preferences. Alternatively, Player B may have reciprocal preferences that cause him to base his choices partly on an evaluation of the Player A choices that would make one side or the other side of the tree relevant for payoffs. A negatively reciprocal Player B might punish Player A for moving Left, and thereby making the equal split unavailable, by choosing (9,3) on that side of the tree. A positively reciprocal Player B might reward Player A for moving Right, and thereby making the equal split available, by choosing (12,9) on that side of the tree.

FIGURES 1.a and 1.b and 1.c ABOUT HERE

An experiment could be run with a protocol that instantiates the game as described above. But such an experiment would not be able to elicit the possible behavioral relevance of endowments that define the *status quo ante* Player A's opportunity to act. Neither could that approach elicit the possible relevance of acts of commission vs. acts of omission that are defined in relation to those endowments. Such an approach could not elicit the possible behavioral relevance of differences in responses to such acts because they lead to the same payoffs. In order to study the behavioral significance of such distinctions we embed the game form in Figure 1a in two alternative economic contexts that differ in the assignment of endowments *ex ante* Player A's opportunity to act.

3.2 Endowments and Acts of Commission vs. Acts of Omission

Figures 1.b and 1.c have the same ordered pairs of money payoffs at their corresponding terminal nodes. However, because of the different endowments in the two games, in order to reach a terminal node with given money payoffs (x, y), Player A and Player B must choose a different sequence of actions in our two treatments.

In the Give or Pass Game (treatment $T_{15,5}$), shown in Figure 1.b, the first mover (Player A) has an endowment of 15 dollars and the second mover (Player B) has an endowment of 5 dollars. These unequal endowments define the *status quo ante* Player A's opportunity to act in this treatment. Player A has two possible moves: she can choose "No Change from (15,5)", that is make no change in the unequal endowments, or she can choose (to) "Give 5" out of her 15 dollar endowment to equalize the now-altered endowments at

(10,10). If Player A chooses "No Change from (15,5)" then Player B has two possible choices: he can choose "No Decrease" or he can choose (to) "Decrease by 6" the endowment of Player A at a cost to himself of 2 dollars. These possible choices in treatment $T_{15,5}$, and the money payoffs they yield, are shown on the left side (or leg) of Figure 1.b. If Player A decides to Give 5 to Player B then Player B has two possible choices: she can choose "No Increase" or she can choose (to) "Increase by 2" the endowment of Player A at a cost to herself of 1 dollar. These possible choices in treatment $T_{15,5}$, and the money payoffs they yield, are shown on the right side (or leg) of Figure 1.b.

In the Take or Pass Game (treatment $T_{10,10}$), shown in Figure 1.c, both Player A and Player B have 10 dollar endowments. These equal endowments define the *status quo ante* Player A's opportunity to act in this treatment. Player A has two possible moves: she can choose "No Change from (10,10)", that is make no change in the equal endowments, or she can choose (to) "Take 5" out of Player B's 10 dollar endowment to imbalance the now-altered endowments at (15,5). If Player A chooses "No Change from (10,10)" then Player B has two possible choices: she can choose "No Increase" or she can choose (to) "Increase by 2" the endowment of Player A at a cost to herself of 1 dollar. These possible choices in treatment $T_{10,10}$, and the money payoffs they yield, are shown on the right side (or leg) of Figure 1.c. If Player A chooses "Take 5" then Player B has two possible choices: he can choose "No Decrease" in the modified endowments or he can choose (to) "Decrease by 6" the modified endowment of Player A at a cost to himself of 2 dollars. These possible choices in treatment $T_{10,10}$, and the money payoffs they yield, are shown on the left side (or leg) of Figure 1.c.

3.3 Implementation as One-Shot Games

In our experiments subjects play a one-shot game. The first mover (Player A) chooses between No Change and Give 5 or between Take 5 and No Change, depending on the game. The second mover (Player B) is asked to use the strategy method; hence, without knowing Player A's choice, Player B makes a choice conditional on each of Player A's two possible choices. Many subjects play the game in the same session. At the end of the experiment, pairs of A and B player subjects are formed randomly and their choices determine payoffs.

4. Implications of Theoretical Models for Play in the Two Treatments

Consequentialist social preferences models (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2002; the text version of Charness and Rabin, 2002; Cox and Sadiraj, 2007) imply that play will be the same in the Give or Pass game as in the Take or Pass game because they have the same end node payoffs. The different consequentialist models may have different implications about which of the ordered pairs of payoffs at the terminal nodes will be preferred by Player B. But all of these models represent social preferences in which an agent's utility of alternative allocations of material payoffs depends only on the (absolute and relative) amounts of the payoffs themselves, not on the agents' actions that may be necessary to generate the allocations in any particular game. Therefore, all of these models imply that Player B will make the same choice between two final payoff allocations, (a,b) or (c,d), in treatment $T_{15,5}$ as in treatment $T_{10,10}$, thus providing a testable hypothesis for our experimental design.

Hypothesis CP. The distribution of play across the four terminal nodes is the same in treatments $T_{15,5}$ *and* $T_{10,10}$.

Any data pattern significantly different from that specified in Hypothesis CP would be inconsistent with ("reject") consequentialist social preferences models.

Revealed altruism theory (Cox, Friedman, and Sadiraj, 2008) extends neoclassical preference theory to include reciprocal preferences by adding Axiom R and Axiom S. Many properties of revealed altruism theory and its parametric special case (Cox, Friedman, and Gjerstad, 2007) were tested in the two cited papers. The experimental design in the present paper provides a direct test of the empirical content of Axioms R and S. Axiom R implies that Player B's preferences are more (resp. less) altruistic if Player A moves Right (resp. Left) in either of our treatments because the feasible set {(10,10), (12,9)} is more generous (to Player B) than the feasible set {(15,5), (9,3)}.[3] Axiom S says that the effect of Axiom R is stronger when a generous (or ungenerous) act overturns the status quo than when the same act merely upholds the status quo. Together, Axioms R and S imply that Player B's preferences are most altruistic when Player A moves Right in game $T_{15,5}$ (a generous act of commission) and least altruistic when Player A moves Left in game $T_{10,10}$ (an ungenerous act of commission). Neoclassical preference axioms together with Axioms R and S imply that more Players B will prefer ("No Decrease", "Increase by 2") for strategy method response in treatment $T_{15,5}$ than in treatment $T_{10,10}$. In this way, the axioms of revealed altruism theory imply an alternative to Hypothesis CP, which is:

Hypothesis SQ. The frequency of play of nodes with payoffs (15,5) and (12,9) is greater in treatment $T_{15,5}$ *than in treatment* $T_{10,10}$.

Any data pattern significantly different from that specified in Hypothesis SQ would be inconsistent with ("reject") the empirical implications of the axioms of revealed altruism theory.

An interesting question is whether data from our treatments can be used to test psychological game theoretic models. The most likely candidate is the widely-used model of sequential reciprocity in Dufwenberg and Kirchsteiger (2004). Application of the Dufwenberg and Kirchsteiger (D&K) model to our $T_{15,5}$ and $T_{10,10}$ games reveals that any pattern of Player B behavior would be consistent with that model.[4] Hence data from our treatments cannot be used to test the D&K model. In contrast, the stylized facts about behavior contained in data from our experiments (reported in section 6) could inform an

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[3] Cox, Friedman and Sadiraj (2008) defines a partial ordering of feasible sets (More Generous Than) and a partial ordering of preferences (More Altruistic Than). Axiom R states a relationship between the two partial orderings. See the cited paper for formal development of the theory.

[4] The authors appreciate the generosity of Martin Dufwenberg in engaging in detailed private communication about the D&K model. A detailed explanation of why any pattern of Player B behavior in our experiment would be consistent with the D&K model is available from the authors on request. An extension of our experimental design to include beliefs elicitation could have testable implications for the D&K model.

extension of the D&K model in which perceptions of what is "kind" are made dependent on the (status-quo) endowment of the game.

5. Three Experiments

Out in the field the status quo arises naturally from established property rights. In a laboratory setting, however, subjects encounter stylized decision problems in which they often lack clear ex-ante expectations. In our experiments three different design features are used to induce status quo:

(i) *Initial endowments*: subjects start off playing the game with initial money balances of \$15 or \$5 in treatments $T_{15,5}$ and \$10 each in treatments $T_{10,10}$. Feasible actions are possible changes in these initial money balances.

(ii) *Labeling of actions*: we label actions that do not cause any change in payoffs as "no change in payoffs" and actions that lead to changes in payoffs as "give/take x" or "increase/decrease by y".

(iii) *Entitlements*: in Experiment 1 the initial endowments are assigned randomly. In Experiments 2 and 3 endowments are earned. We use a two-day experimental procedure which has subjects earn their monetary endowments in a real-effort task on Day 1 of the experiment. Experiment 2 employs a tournament format in which higher endowments are received for better performance. In Experiment 3 we randomly assign subjects into different sessions and ask everyone in a given session to attain the same target performance level. The higher the target level in a session, the higher the amount earned.

The first two design features complement one another and provide a natural way of establishing the status quo. By (i) and (ii) the status quo (our treatment variable) is set by the initial endowments that will subsequently be changed or preserved by Player A via feasible actions. As for feature (iii), *ex ante* it is not clear whether the strength of property rights interacts with the labeling of actions as "give" or "take" and "decrease" or "increase". Several previous studies have found a notable effect of earned vs. randomly assigned endowments on subsequent behavior in dictator games (Cherry, Frykblom, and Shogren, 2002; Oxoby and Spraggon, 2008), bargaining games (Hoffman, McCabe, Shachat, and Smith, 1994; Rutström and Williams, 2000**),** public good games (Clark, 2002 and Harrison, 2007) and other games involving reciprocal considerations (Danková and Servátka, 2015). Experiments 2 and 3 therefore serve as robustness checks with respect to the procedure by which entitlements are induced. Their designs mimic two common labor market compensation practices, tournaments and absolute performance targets.

In addition we used a two-day format that separates the earnings task from the strategic play of the game. The intention was to give subjects some time to "bond" with the earnings so they better perceive them as their own property rather than "house money" (Thaler and Johnson, 1990; Cárdenas, De Roux, Jaramillo, and Martinez, 2014; Danková and Servátka, 2015)

We conducted four one-day sessions in Experiment 1, six two-day sessions in Experiment 2 and five two-day sessions in Experiment 3. The treatments were implemented in a between-subjects design. All sessions were run manually using the strategy method (Selten, 1967; Brandts and Charness, 2011).

Experiment 1 presents a test in which initial endowments (and thus also the roles) were randomly assigned by the experimenter. In what follows we refer to Experiment 1 treatments as RANDOM $T_{15,5}$ and RANDOM $T_{10,10}$. In treatment RANDOM $T_{15,5}$ subjects play Give or Pass Game $T_{15,5}$ with endowments (15, 5), presented in Figure 1.b and in treatment RANDOM $T_{10,10}$ they play Take or Pass Game $T_{10,10}$ with endowments (10, 10), presented in Figure 1.c.

In Experiment 2 subjects compete in a tournament which places them in three different groups based on their relative performance in the quiz. Individuals with better performance receive higher endowments. The subjects were recruited for a two-day experiment. On Day 1 of the experiment each participant was asked to answer the same set of 40 math questions, selected from the GMAT test bank. The quiz score was the number of questions the subject answered correctly minus 1/4 of a point for each incorrect answer. After everyone completed the computerized quiz (programmed in Visual Basic), the final scores were ranked from the highest to the lowest and ties were resolved randomly. Once the complete ranking of the participants had been determined, the participants who scored in the top 25% received an IOU certificate for $15, those in the middle 25-75%

received a $10 certificate, and those in the bottom 25% received a $5 certificate. These certificates provided the endowments for Day 2 participation. Subjects who earned $15 or $5 were invited to the same session on Day 2 while subjects who earned $10 were all invited to a session that started at a different time on Day 2.

The two different Day 2 sessions constituted our experimental treatments TOURNAMENT $T_{15,5}$ and TOURNAMENT $T_{10,10}$. Day 2 sessions used procedures identical to Experiment 1 with the only difference that the endowments were earned on Day 1. In treatment TOURNAMENT $T_{15,5}$ this implied that the roles were also determined based on subjects' performance on Day 1. In treatment TOURNAMENT $T_{10,10}$ the subjects were assigned to be either Player A or Player B in a random way.

In Experiment 3 (treatments TARGET $T_{15,5}$ and TARGET $T_{10,10}$) subjects performed the same earning task of solving GMAT problems, except that their assignment to roles was random. On Day 1 of the experiment participants were asked to correctly answer 10, 20 or 30 problems, depending on which session they were recruited for. There was no penalty for providing an incorrect answer and no time limit; everyone completed the earnings task in their session. For reaching one of the three target performance levels they received an IOU certificate for $5, $10, or $15, respectively. These certificates provided the endowments for Day 2 participation. The rest of the procedures were identical to Experiment 2.

All sessions were held in the New Zealand Experimental Economics Laboratory (NZEEL) at the University of Canterbury. A total of 416 undergraduate subjects participated in the study. On average, a one-day session lasted about 60 minutes including the initial instruction period and payment of subjects. A two-day session lasted about 120 minutes. The experimental earnings, denoted in $, were converted into cash at the 3 to 4 exchange rate: $3 (or 3 lab $) equals 4 New Zealand dollars, henceforth NZD. In Experiment 1 subject payments included a 5 NZD show up fee. In Experiments 2 and 3 the show up fee was 10 NZD (i.e., 5 NZD for each of the two days), all paid at the end of the Day 2 session. The payoff protocol was double blind.

6. Results

As the main focus of the current paper is on a particular aspect of reciprocal behavior, we begin by first presenting the behavior of B Players in Table 1 and defer the discussion of A Player's behavior until the next section.

6.1 Tests for Differences in B Players' Behavior across the Three Experiments

Recall that B Players' choices were elicited by the strategy method. Each Player B thus made two choices, one for each of the two subgames. However, we cannot simply compare the choice-frequencies at the terminal nodes because use of the strategy method makes the choice data not independent across nodes within a subgame. Nevertheless, each subject's chosen strategy (a pair of choices, one for each subgame) is an independent observation. Therefore, we first classify the behavior of each subject into one of four possible strategies: 1. No Decrease-No Increase (ND-NI); 2. No Decrease-Increase by 2 (ND-IB2); 3. Decrease by 6-No Increase (DB6-NI); 4. Decrease by 6-Increase by 2 (DB6-IB2). Then, we run Fisher's exact test on the strategies rather than the choices.

To assess the impact of earned endowments on Player B reciprocal responses, we compare their behavior in the respective treatments using the data presented in Table 1.

TABLE 1 ABOUT HERE

We begin by testing the impact of endowment protocols in the $T_{15,5}$ treatments. Fisher's exact tests, reported in the first two rows of Table 2 reveal that there are no differences in B Players' behavior whether their endowments represent a windfall gain and are randomly assigned or earned in a tournament or by reaching a target performance (p=0.897 and 0.882, respectively). Given that, it is not surprising that the (tournament or target) type of earning procedure does not influence their decisions (p=0.606). A similar pattern emerges for the $T_{10,10}$ treatments where the respective p-values are 0.488, 0.500, and 0.520, suggesting that a random assignment of endowments was sufficient to establish strong enough property right entitlement effects on subjects' reciprocal behavior. Moreover, it also provides evidence that the tournament procedure in Experiment 2 did not incidentally select different reciprocal types into different treatments based on their GMAT performance.

TABLE 2 ABOUT HERE

6.2 Tests Using Pooled Data

Given that we do not find any differences in B Players' behavior across the three experiments, we pool all data and perform tests for the overall effect. Table 3, split into panels, presents data and statistical tests from individual Experiments 1-3 as well as pooled data on Player B's behavior according to the distribution of play. As can be easily seen from the table, the data are consistent with reciprocity. Our next question is whether the observed difference in play between the two games is statistically significant. We compare Player B's behavior in two ways: (i) for the whole game tree; and (ii) for corresponding subgames.

As before, we run Fisher's exact test on the strategies rather than the choices. This implements the test of the null (Hypothesis CP) that the distribution of play across the four terminal nodes is the same in treatments $T_{15,5}$ and $T_{10,10}$. The test rejects the null in favor of Hypothesis SQ with very high significance (p<0.001).

TABLE 3 ABOUT HERE

A tougher test of Hypothesis SQ would be to test its implication in each individual subgame. In particular, for the subgame on the left side of the game tree it implies that the frequency of "Decrease by 6" will be higher in treatment $T_{10,10}$ than in $T_{15,5}$. The one-sided Fisher's exact test detects a statistically significant difference between frequencies with which the Decrease by 6 choice was selected in the two treatments (p=0.011). For the subgame on the right side the prediction is the frequency of Increase by 2 is higher in treatment $T_{15,5}$ than $T_{10,10}$. The one-sided Fisher's exact test detects a statistically significant difference (p<0.001).

6.3 The Effect of Endowment Allocation Procedures on A Players' Behavior

We next briefly discuss the differences in A Players' behavior who show a great sensitivity to procedures under which the initial endowments were allocated. Table 4 summarizes and compares their behavior in our three experiments. We observe a significant difference in A Players' behavior between the two treatments in all three experiments (p=0.001 for RANDOM $T_{15,5}$ vs. RANDOM $T_{10,10}$; p=0.016 for TOURNAMENT $T_{15,5}$ vs. TOURNAMENT $T_{10,10}$ and p=0.09 for TARGET $T_{15,5}$ vs. TARGET $T_{10,10}$). We also find a significant difference in frequencies of choosing to Give 5 between RANDOM $T_{15,5}$ treatment, where the windfall initial endowments were assigned randomly by the experimenters, and treatments TOURNAMENT $T_{15,5}$ and TARGET $T_{15,5}$ where the endowments were earned (p=0.028 and p=0.004, respectively). The evidence that A Players were less generous when they had to earn their endowments is in line with previous findings by Cherry, Frykblom, and Shogren (2002), Oxoby and Spraggon (2008), and Carlsson, He, and Martinsson (2012). We do not find any differences in A Players' behavior between TOURNAMENT and TARGET treatments (p=0.614).

TABLE 4 ABOUT HERE

Comparison of treatment RANDOM $T_{10,10}$ with TOURNAMENT $T_{10,10}$ and TARGET $T_{10,10}$ reveals that the frequency of Take 5 is higher when the endowments are assigned randomly than when they are earned (p=0.001 and p=0.028, respectively), indicating that A Player subjects honor property rights created by performance in the math quiz. Despite the fact that there appears to be more taking when the endowments were earned by reaching a target output than in a tournament (50% vs. 34.3%, respectively), the Fisher's exact test does not detect a significant difference between TOURNAMENT $T_{10,10}$ and TARGET $T_{10,10}$ treatments (p=0.232).

7. Discussion

We have reported three experiments with two instantiations of a simple two player game. The respective terminal node payoffs are the same in the Take or Pass Game as in the Give or Pass Game. But the games begin with different endowments and require different actions to arrive at the same payoff. The endowment for a game is the *status quo ante* Player A's choice between No Change — an act of omission that preserves the endowment — and Give or Take — an act of commission that changes the endowment to the profit of one player and cost to the other. Most importantly, the left-hand subgame in one treatment is selected by Player A's selfish act of commission (Take 5) while in the other treatment it is selected by making No Change in the endowment. Similarly, the right-hand subgame in one treatment is selected by a generous act of commission (Give 5) while in the other treatment it is selected by making No Change in the endowment.

Our data analysis mainly focuses on second mover behavior. Does reciprocal behavior vary in predictable ways in response to acts of commission versus acts of omission in our experiments? Our answer is "yes". Data from the experiment provide support for the importance of discriminating between acts of commission and omission by a first mover in theoretical modeling of reciprocal behavior. The data support the prediction in Hypothesis SQ that in our treatments (see Figure 1): *The frequency of observation of nodes with payoffs (15,5) and (12,9) is greater in treatment* $T_{15,5}$ *than in treatment* $T_{10,10}$. This pattern of play reflects central features of revealed altruism theory (Cox, Friedman, and Sadiraj, 2008); if we had observed any other pattern of play, the empirical relevance of that theoretical model would have been called into question. That model had previously performed well in tests using data from several types of experiments reported in papers by various researchers.[5] But the experiment reported herein is the first

[5] The model has previously done well with tests of data obtained from experiments reported in Huck, Muller, and Normann (2001), Andreoni, Harbaugh, and Vesterlund (2003), Cox (2004), Cox, Friedman, and Sadiraj (2008), Cox, Ostrom, Walker, et al. (2009), Cox and Hall (2010), and Cox, Ostrom, Sadiraj, and Walker (2013).

one designed to stress-test the idiosyncratic implications of the model's Axioms R and S that account for Hypothesis SQ.[6]

The primary difference between Experiment 1 and Experiments 2 and 3 is the saliency of entitlements to endowments. Based on previous experimental evidence on earned endowments and behavior, we conjectured that *earned* endowments could be key to the intensity of reciprocal reactions towards acts of commission. In everyday life the money in one's wallet is in most cases earned and regarded by the owner as being well deserved. People routinely exchange their time and effort for wages to which they form a strong sense of ownership or entitlement. In the laboratory, we cannot ask subjects to play with their own money and therefore entitlements are not easily established. In our Experiments 2 and 3 we approached this problem by splitting the experiment into two days and having subjects earn their endowments on Day 1 of the experiment. Not only did the subjects have to work for the endowments but they also had some time between the earning part and the game part to develop a sense of ownership of their earnings (Strahilevitz and Loewenstein, 1998). Earned endowments significantly affected giving and taking by first movers but to our surprise did not have a significant effect on second movers' reciprocal responses.

Our data show that subjects with reciprocal preferences are quite sensitive to acts of commission, i.e., acts that overturn the status quo. In our experiments we have developed a procedure that makes the status quo salient rather naturally. It involves an experimental design with specification of endowments and feasible actions that make acts of commission, such as giving or taking, stand in contrast with acts of omission, such as not giving or not taking when there is an opportunity to do so.

One can ask whether this approach would be generally effective for establishing a status quo in experiments. Experience, habits, customs and norms are likely to play an important role in some contexts. From this perspective field experimentation might be another fruitful avenue for future research on the empirical significance of acts of commission vs. acts of omission. The field has the advantage that both the status quo and

[6] There may be other models that are also capable of rationalizing both the data reported herein and the data from the many experiments included in the papers listed in footnote 5, but that is a question beyond the scope of the present paper.

entitlements to endowments arise naturally. However, the complexity and richness of the field environment might make it difficult for researchers to identify the status quo conditions that are perceived by participants.

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Figure 1a. Abstract Game Tree

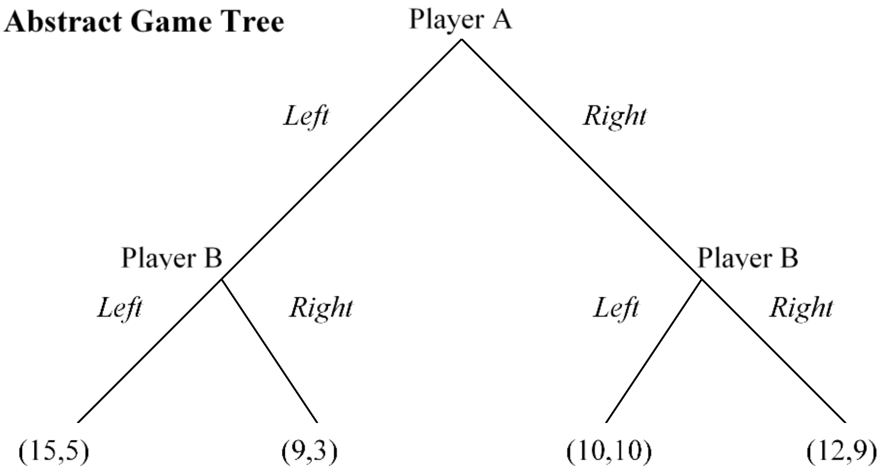

Figure 1b. Give or Pass Game $T_{15,5}$ with Endowments (15, 5)

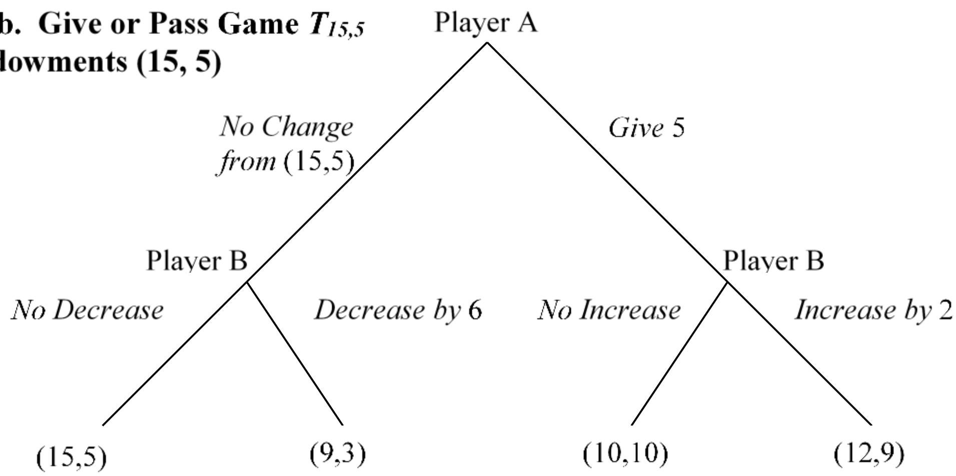

Figure 1c. Take or Pass Game $T_{10,10}$ with Endowments (10, 10)

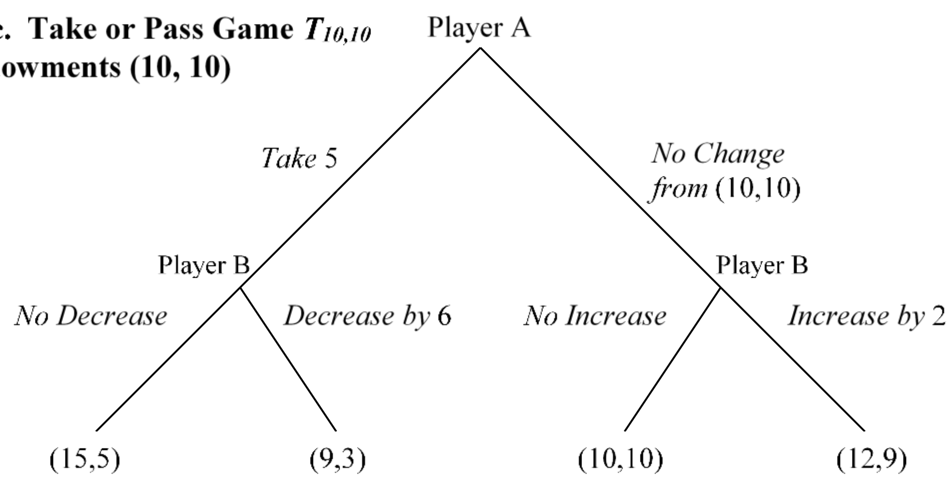

Table 1. Raw Data on B Players' Behavior Categorized According to Strategies

	Strategies			
Treatment	**ND-NI**	**ND-IB2**	**DB6-NI**	**DB6-IB2**
RANDOM $T_{15,5}$ n = 33	16	10	5	2
RANDOM $T_{10,10}$ n = 34	19	1	13	1
TOURNAMENT $T_{15,5}$ n =35	14	13	5	3
TOURNAMENT $T_{10,10}$ n = 35	20	4	9	2
TARGET $T_{15,5}$ n =35	17	8	7	3
TARGET $T_{10,10}$ n = 36	15	4	15	2
POOLED DATA $T_{15,5}$ n =103	47	31	17	8
POOLED DATA $T_{10,10}$ n = 105	54	9	37	5

ND = No Decrease; DB6 = Decrease by 6; NI = No Increase; IB2 = Increase by 2

Table 2. Tests for B Players' Behavior across the Three Experiments

Tests for $T_{15,5}$ Treatments	
RANDOM $T_{15,5}$ vs. TOURNAMENT $T_{15,5}$	0.897
RANDOM $T_{15,5}$ vs. TARGET $T_{15,5}$	0.882
TOURNAMENT $T_{15,5}$ vs. TARGET $T_{15,5}$	0.606
Tests for $T_{10,10}$ Treatments	
RANDOM $T_{10,10}$ vs. TOURNAMENT $T_{10,10}$	0.488
RANDOM $T_{10,10}$ vs. TARGET $T_{10,10}$	0.500
TOURNAMENT $T_{10,10}$ vs. TARGET $T_{10,10}$	0.520

All Fisher's tests reported in Table 6 are two-sided.

Table 3: Player B Behavior

Panel A: Experiment 1				
	No Decrease	Decrease by 6	No Increase	Increase by 2
RANDOM $T_{15,5}$	26/33 (78.8%)	7/33 (21.2%)	21/33 (63.6%)	12/33 (36.4%)
RANDOM $T_{10,10}$	20/34 (58.8%)	14/34 (41.2%)	32/34 (94.1%)	2/34 (5.9%)
Fisher's Test for Strategies	0.004[a]			
Fisher's Test for Subgames	0.067		0.002	
Panel B: Experiment 2				
TOURNAMENT $T_{15,5}$	27/35 (77.1%)	8/35 (22.9%)	19/35 (54.3%)	16/35 (45.7%)
TOURNAMENT $T_{10,10}$	24/35 (68.6%)	11/35 (31.4%)	29/35 (82.9%)	6/35 (17.1%)
Fisher's Test for Strategies	0.061[a]			
Fisher's Test for Subgames	0.296		0.01	
Panel C: Experiment 3				
TARGET $T_{15,5}$	25/35* (71.4%)	10/35* (28.6%)	25/36 (69.4%)	11/36 (30.6%)
TARGET $T_{10,10}$	19/36 (52.8%)	17/36 (47.2%)	30/36 (83.3%)	6/36 (16.7%)
Fisher's Test for Strategies	0.211[a]			
Fisher's Test for Subgames	0.084		0.133	
Panel D: Pooled Data on B Players' Behavior				
$T_{15,5}$	78/103* (75.7%)	25/103* (24.3%)	65/104 (62.5%)	39/104 (37.5%)
$T_{10,10}$	63/105 (60%)	42/105 (40%)	91/105 (86.7%)	14/105 (13.3%)
Fisher's Test for Strategies	0.000[a]			
Fisher's Test for Subgames	0.011		0.000	

[a] two-sided test.

* One Player B did not provide an answer on the left side of the game tree.

Table 4. Comparison of A Players' Behavior across the Three Experiments

	$T_{15,5}$		$T_{10,10}$	
	Give 5	No Change from (15,5)	No Change from (10,10)	Take 5
Experiment 1: RANDOM assignment	21/33 (63.6%)	12/33 (36.4%)	8/34 (23.5%)	26/34 (76.5%)
RANDOM $T_{15,5}$ vs. RANDOM $T_{10,10}$	0.001			
Experiment 2: TOURNAMENT	12/35 (34.3%)	23/35 (65.7%)	23/35 (65.7%)	12/35 (34.3%)
TOURNAMENT $T_{15,5}$ vs. TOURNAMENT $T_{10,10}$	0.016			
Experiment 3: TARGET	10/36 (27.7%)	26/36 (72.3 %)	18/36 (50%)	18/36 (50%)
TARGET $T_{15,5}$ vs. TARGET $T_{10,10}$	0.09			
Tests for $T_{15,5}$ Treatments (Give 5)				
RANDOM $T_{15,5}$ vs. TOURNAMENT $T_{15,5}$	0.028			
RANDOM $T_{15,5}$ vs. TARGET $T_{15,5}$	0.004			
TOURNAMENT $T_{15,5}$ vs. TARGET $T_{15,5}$	0.614			
Tests for $T_{10,10}$ Treatments (Take 5)				
RANDOM $T_{10,10}$ vs. TOURNAMENT $T_{10,10}$	0.001			
RANDOM $T_{10,10}$ vs. TARGET $T_{10,10}$	0.028			
TOURNAMENT $T_{10,10}$ vs. TARGET $T_{10,10}$	0.232			

All Fisher's tests reported in Table 4 are two-sided.

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