Status Quo Effects in Fairness Games: Reciprocal Responses to Acts of Commission vs. Acts of Omission

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Abstract

Both the law and culture make a central distinction between acts of commission that overturn the status quo and acts of omission that uphold it. In everyday life, acts of commission often elicit stronger reciprocal responses than do acts of omission. In this paper we compare reciprocal responses to both types of acts and ask whether behavior of subjects is consistent with existing theory. We present three experiments that differ in the manner in which the endowments that characterize the status quo are induced. We find acts of commission generate stronger reciprocal reactions than acts of omission, as predicted by some theories but not others.

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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? In this paper we compare reciprocal responses to choice of acts of commission that actively impose harm or kindness and choice of acts of omission, which represent failures to prevent harm or to act kindly. We provide direct evidence that acts of commission yield stronger reciprocal responses than do acts of omission.[1] These distinctions are central to understanding of reciprocal preferences.

There are many examples where acts of commission trigger different reciprocal responses than acts of omission. 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 an unusually small tip for choosing not to fulfill an extraordinary request. A mobster may retaliate with a bloody vengeance because someone intentionally hurt his family member but might not hurt a bystander who chose not try to prevent the harm. Legal consequences may vary from probation to capital punishment to damages in millions of dollars depending on the level of intent inferred from acts of commission or omission.

The difference between acts of commission and acts of omission has important implications for legal decisions because they are often used to infer defendants' intentions. In criminal law, *actus reus* (the act of committing a crime) and *mens rea* (the state of mind) are crucial when deciding whether a person is guilty of a specific crime, some other crime, or no crime. The party responsible for the death of a human being can be convicted of criminally negligent homicide if the death was caused (beyond reasonable doubt) by a form of gross negligence. For example, gross negligence includes the failure to stop and render aid in a hit-and-run accident, which is an act of omission. A murder conviction, however, requires that the person had (beyond reasonable doubt) an intention to kill, which is inferred from acts of commission.

In tort law, compensatory damages are awarded for ordinary negligence due to the harmful consequences of an act of omission. However, in a particularly egregious case

[1] Our concept of an (act of) omission corresponds to common usage of language as reflected in Definition 1.1 of "omission" in Oxford Dictionaries (online) as: "The action of excluding or leaving out someone or something".

where the tort was reasonably foreseeable and, despite this, the harmful act was committed then punitive damages may be awarded.[2, 3]

The distinction between acts of commission and acts of omission has been explored in depth by philosophers whose main focus was on the morality of the actions. Some philosophers conclude that the distinction between the two types of acts is often morally irrelevant (Bennett 1966, 1981, 1983; Singer, 1979; Hare, 1981) while others argue for the relevance of the distinction (Kagan, 1988; Kamm, 1986; Steinbock, 1980).[4] Psychologists point out that some of the cases studied by philosophers often differ in other aspects than just acts of commission vs. omission and that philosophers themselves are often subject to psychological biases, and therefore it is reasonable to assume that there is no difference in morality between the two types of acts. Under this assumption they study causes of the *omission bias* (i.e., when subjects judge harmful commissions as worse than the corresponding omissions), such as loss aversion, exaggeration effect,

[2] "To support award of punitive damages, act which constitutes the cause of action must be activated by or accompanied with some evil intent, or must be the result of such gross negligence - such disregard of another's rights - as is deemed equivalent to such intent." (Newport v. USAA 11 P.3d 190 Okla., 2000, July 18, 2000). See also Feinberg (1984) on further discussion on how the law distinguishes between acts of commission and acts of omission.

[3] An interesting example of awarding punitive damages is tobacco litigation. In Florida, the information that the tobacco industry knew that cigarettes were harmful, nicotine was addictive, and there were risks from second-hand smoking, obtained in the mid-nineties by whistleblowers Merrell Williams and Jeffrey Wigand, was used for the first time in a jury trial. It was the first time that an individual won a lawsuit for lung cancer. In 2000, a Florida jury awarded the biggest punitive damages in US history at the time, $144.8 billion. This lawsuit explored the pattern of lies and bogus claims produced by tobacco companies while knowing that the use of their product was detrimental to consumers' health and could cause death. The jury foreman said: "This verdict wasn't about the state of the tobacco industry today. It was about 50 years of fraud, misrepresentation, and lying to the American public." (Tobacco News, www.tobacconews.org) According to the jury verdict, the amount of punitive damages was not as important as the strong message of the large judgment and that Big Tobacco must – and will – be held accountable (Schlueter, 2005, p. 573-577).

[4] A representative of this debate is the famous ethics thought experiment involving a trolley: "A trolley is running out of control down a track. In its path are five people who have been tied to the track by a mad philosopher. Fortunately, you could flip a switch, which will lead the trolley down a different track to safety. Unfortunately, there is a single person tied to that track. Should you flip the switch or do nothing?" (Foot, 1978). See also Thomson 1985; Unger, 1996; Kamm, 1989, Greene, 2007, Moll and de Oliveira-Souza, 2007.

overgeneralization, and commissions being linked to causality judgments.[5] The omission bias is closely related to the *bias toward the status quo*, a preference for the current state that Samuelson and Zeckhauser (1988) found in risky as well as in riskless choices and which has also been found in reactions to outcomes (Kahneman and Tversky, 1982; Viscusi, Magat, and Huber, 1987; Knetsch, Thaler, and Kahneman, 1988; Ritov and Baron, 1992; Baron and Ritov, 1994). All these studies focus on the decision-maker who chooses to maintain his position or selects an alternative. In a recent experiment employing a dictator game, Hayashi (2013) finds that dictators who can reallocate randomly assigned endowments tend to be less generous when they preserve the status quo (endowments) then when they overturn the status quo and reallocate. In contrast, our paper focuses 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. The status quo created by endowments at the beginning of a game allows us to make a distinction between acts of overturning and upholding those endowments, and enables us to examine its relevance for the strength of reciprocal responses. This places our study amongst the literature exploring the importance of reference dependence (Kahneman and Tversky, 1979; Kahneman, Knetsch, and Thaler, 1986; Kőszegi and Rabin, 2006) on fairness perceptions (e.g. Kahneman, Knetsch, and Thaler, 1986; Hart and Moore, 2008; Benjamin, 2005; Fehr, Hart, and Zehnder, 2011). Our main contribution to the literature is providing evidence about the impact of one type of reference point – the *status quo ante* a first mover's choice created by initial endowments – on reciprocal behavior in simple extensive form games.

The above examples involving a waiter or a mobster offer straightforward illustration of the relationship between reciprocity and the status quo. As is often the case with examples from everyday life, there are numerous features of the examples that vary systematically between the scenarios, which prevents their clean interpretation. For example some everyday life examples suffer from the fact that acts of commission differ from acts of omission in some other aspect(s) of behavior such as the amount of effort

[5] For a further discussion see Spranca, Minsk, and Baron (1991) who also present an interesting psychology experiment showing that subjects often rate harmful omissions as less bad than harmful commissions. Subjects' ratings are associated with judgments that omissions do not cause outcomes.

necessary to take an action. Such confounds can cloud the intuition and make it hard to unambiguously attribute the causality solely to the difference between commission and omission. A controlled laboratory environment, however, makes feasible a clean manipulation of the status quo while keeping everything else constant. This enables us to identify the relationship between reciprocity and the status quo.

The central question of our study can be stated as: Do acts that overturn initial endowments generate stronger reciprocal responses than acts which uphold them? Consider the following two stylized thought experiments.

Scenario 1: Your initial wealth is $100K and John's initial wealth is $100K.

A. Suppose John had an opportunity to give you $10K but chose to give you nothing. Would you want to punish him?
B. Now suppose John does give you $10K. Would you want to reward him?

Scenario 2: Your initial wealth is $110K and John's initial wealth is $90K.

C. Suppose John had an opportunity to take $10K from you but chose to take nothing. Would you want to reward him?
D. Now suppose that John does take $10K from you. Would you want to punish him?

The two scenarios highlight the relationship between reciprocity and status quo. In Scenario 1, the status quo is that you did *not* own the $10K and John: (i) did *not* give it to you; or (ii) did give it to you. In Scenario 2, the status quo is that you did own the $10K and John: (i) did *not* take it from you; or (ii) did take it from you.

The importance of status quo and acts of commission or omission are particularly compelling when comparing scenario 1.A with 2.D and 1.B with 2.C. In both scenarios 1.A and 2.D, your final payoff is $100K and John's final payoff is also $100K. But in scenario 2.D John actively takes $10K from you while in scenario 1.A he chooses to give you nothing. In both scenarios 1.B and 2.C your final payoff is $110K and John's is

$90K. But in scenario 1.B John actively gives you $10K while in scenario 2.C he forgoes the opportunity to take $10K from you.

Little empirical work has focused on the effects of acts of commission vs. acts of omission defined relative to the status quo. In this paper we report direct evidence on this topic. We present three experiments that differ in the manner in which the endowments that characterize the status quo are induced. This allows us to perform a check of robustness of the behavioral patterns in the data.

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 first mover acts of commission that alter the initial endowments and acts of omission that do not change them. Importantly, we keep the terminal payoffs in both games the same, which gives us a clean test of the empirical significance of opportunities and payoffs that result from acts of commission that change the endowments versus acts of omission that do not change them.

2. Experimental Design

We first explain the abstract form of the game and, subsequently, explain the alternative economic implementations of the game. We use game trees to represent the games herein, however it is important for understanding the way the experiments were run to know that *subjects were not shown game trees*. Subject instructions and response forms that show exactly how the games were presented to the subjects are available at http://excen.gsu.edu/jccox/docs/Status-Quo-Effects-in-Fairness-Games-Instructions-and-Decision-Forms.pdf .

2.1 Abstract Game Tree

All of our experimental treatments involve the game that can be represented by the tree diagram in Figure 1. 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.

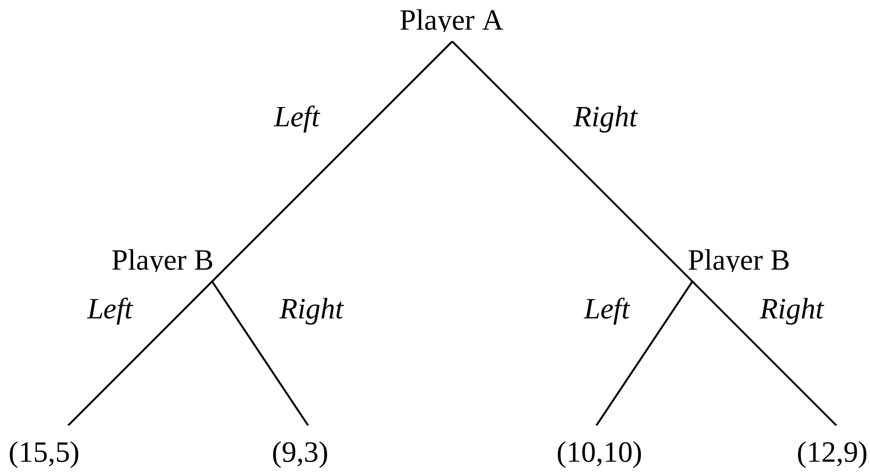

Figure 1. Abstract Game Tree

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 consequentialist models of preferences. Alternatively, Player B may have reciprocal preferences that cause her to base her 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.

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 like that between the above Scenario 1.A (where John had an opportunity to give you $10K but did not) and Scenario 2.D (where John had an opportunity to take $10K from you and did so) because they lead to the same (ordered pair of) payoffs. The above approach also could not elicit the behavioral relevance of the difference between Scenario 1.B (where John has the opportunity to give you $10K and does so) and Scenario 2.A (where John has the opportunity to take $10K from you but does not do so) because they lead to the same payoffs. In order to study the behavioral significance of such distinctions we embed the game form in Figure 1 in two alternative economic contexts that differ in the assignment of endowments *ex ante* Player A's opportunity to act.

2.2 Endowments and Acts of Commission vs. Acts of Omission

Figures 2.a and 2.b 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 2.a, 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 2.a. 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 2.a.

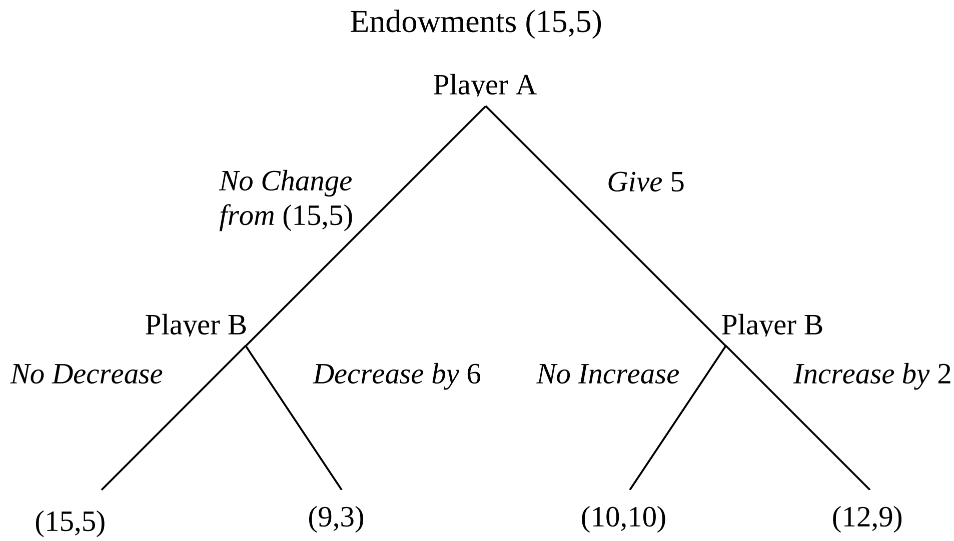

Figure 2.a. Give or Pass Game $T_{15,5}$

In the Take or Pass Game (treatment $T_{10,10}$), shown in Figure 2.b, 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 2.b. 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 2.b.

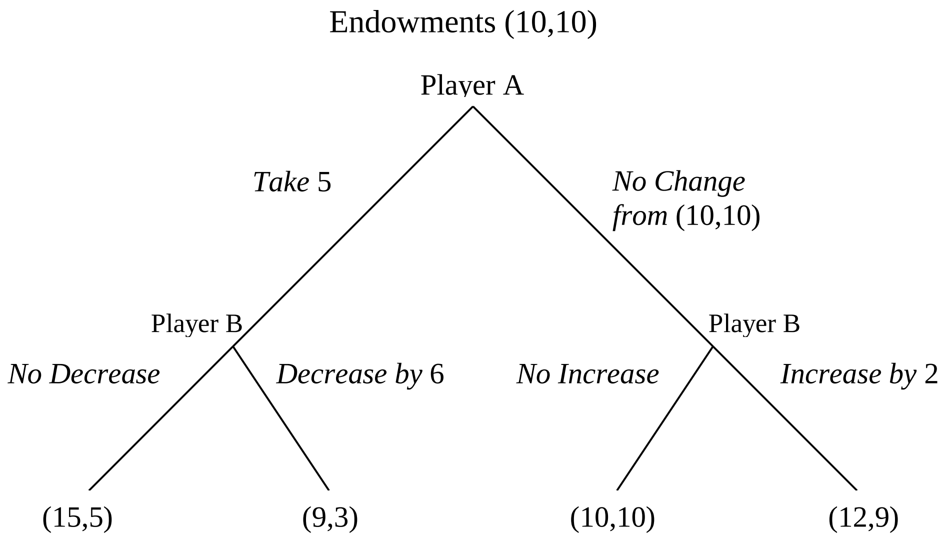

Figure 2b. Take or Pass Game $T_{10,10}$

3. Implications of Alternative Theoretical Models for Play in the Two Treatments

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.

3.1. The Two Treatments Are Equivalent for Purely Consequentialist Models

If all agents have self-regarding (or *homo economicus*) preferences and believe that all others have such preferences and that they are rational then the unique subgame perfect equilibrium payoff is (15,5) in both the give or pass game and the take or pass game.

Models of social preferences such as Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Cox and Sadiraj (2007) do *not* imply that all play will end at the (15,5) node in the two treatments because they model other-regarding or social preferences which are not necessarily the same as self-regarding preferences over ordered pairs of money payoffs. Furthermore, the different social preferences 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 $T_{10,10}$. If both Player A and Player B have such social preferences, and believe that the other player has these preferences and is rational, then both Player A and Player B will make the same choices in treatment $T_{15,5}$ as in treatment $T_{10,10}$. These types of models would have to be extended to incorporate endowment reference points in order to get predictions of differences in play across our two treatments; in their original forms they have a clear implication that play will be the same in the Give or Pass Game as in the Take or Pass Game. Most importantly, any such extensions would need to be done before data from the experiment reported herein was observed in order to avoid simply performing *ex post* exercises in "theoretical curve-fitting".[6]

In the forms reported by their authors, models of social preferences developed by Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Cox and Sadiraj (2007) have a testable implication for our experiment: *The distribution of play across the four terminal nodes is the same in treatments* $T_{15,5}$ *and* $T_{10,10}$.

3.2. The Two Treatments are Not Equivalent for Models of Reciprocal Preferences

As with other (unconditional) social preferences models, the model in the text of Charness and Rabin (2002) implies that play in our two treatments will be the same. In contrast, an interpretation of the reciprocal preferences model in their appendix can lead to a different prediction, as follows. If we assume there is "social consensus" that choice

[6] It has been argued that cumulative prospect theory (with loss aversion) implies that the two games are not isomorphic. This argument is critically examined in the appendix to our paper; it is clearly not true for the version of cumulative prospect theory developed by Tversky and Kahneman (1992).

of Take 5 in the $T_{10,10}$ game is considered to be "misbehavior" while choice of No Change in the $T_{15,5}$ game is not, then Player B may place a lower weight on Player A's payoff on the left branch of $T_{10,10}$ than on the left branch of $T_{15,5}$. This would imply that a higher proportion of Players B would choose (9,3) in treatment $T_{10,10}$ than in $T_{15,5}$. Similarly, if we assume that social consensus is that choice of Give 5 in the $T_{15,5}$ game is considered to be good behavior while choice of No Change in the $T_{10,10}$ game is not, then Player B may place a higher weight on Player A's payoff on the right branch of $T_{15,5}$ that on the right branch of $T_{10,10}$. This would imply that a higher proportion of Players B would choose (12,9) in treatment $T_{15,5}$ than in treatment $T_{10,10}$. Different assumptions about social consensus could have different implications for play in our games; therefore an a priori criterion for specifying the social consensus is needed in order to make the model testable with our data.

The implications of psychological game theoretic models for our experiment are not clear. The Dufwenberg and Kirchsteiger (2004) and Falk and Fischbacher (2006) models can have multiple equilibria. Adding an equilibrium selection criterion to either model will not discriminate between predicted equilibria for our $T_{15,5}$ and $T_{10,10}$ games. Rather, the first-mover's perception of what is "kind" would have to be made dependent on the (status-quo) endowment of the game, which would be an extension of the models that could produce different behavioral predictions for our two games.

No special assumptions or extensions are needed to derive clear predictions of revealed altruism theory (Cox, Friedman, and Sadiraj, 2008) for choices by Players B in the $T_{15,5}$ and $T_{10,10}$ treatments. The other-regarding preference ordering and Axiom R of that theory predict that Player B will be more altruistic if Player A moves Right rather than Left in either treatment because the feasible set {(10,10), (12,9)} is more generous (to Player B) than the feasible set {(15,5), (9,3)}. This leads to the prediction that a higher proportion of Players B will move Right in the subgame corresponding to a choice of Right by Players A than will move Right in the other subgame in both the $T_{15,5}$ and $T_{10,10}$ treatments.

Revealed altruism theory makes additional predictions, as follows. Although the collection of opportunity sets that Player A can offer Player B are identical in treatments $T_{15,5}$ and $T_{10,10}$, the status quo set that corresponds to the endowments is different. The more generous opportunity set in treatment $T_{15,5}$ is selected by an act of commission by Player A (giving $5 to Player B). The more generous opportunity set in treatment $T_{10,10}$ is selected by an act of omission by Player A (making no change). Similarly, the less generous opportunity set in the $T_{10,10}$ treatment is selected by an act of commission while the less generous opportunity set in the $T_{15,5}$ treatment is selected by an act of omission.[7] Axiom S is the element of revealed altruism theory that implies that games $T_{15,5}$ and $T_{10,10}$ are not isomorphic. This axiom distinguishes between acts of commission, which overturn the status quo, and acts of omission which uphold the status quo. 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. The theory predicts that a Player B will respond more altruistically towards a Player A who overturns the status quo in treatment $T_{15,5}$ by choosing Give 5 than to a Player A in treatment $T_{10,10}$ who chooses No Change from (10,10), even though these actions provide Player B with the same opportunity set. Similarly, a Player B will respond less altruistically to a Player A who overturns the status quo in treatment $T_{10,10}$ by choosing Take 5 than to a Player A who chooses No Change from (15,5) in treatment $T_{15,5}$ even though these actions provide Player B with the same opportunity set.

In summary, revealed altruism theory has a clear testable implication for our experiment: (*) *The frequency of observation of nodes with payoffs (15,5) and (12,9) is greater in treatment* $T_{15,5}$ *than in treatment* $T_{10,10}$.

It is important to note that the preceding testable implication of revealed altruism theory contained in statement (*) is a property of the theoretical model, as published several years before the experiment reported herein was run. If the data are inconsistent

[7] A design in which players choose between acts of commission and omission eliminates the differences (e.g. the amount of effort) between these two types of acts that are sometimes present in field examples like the ones discussed in the introduction.

with statement (*) then this model is contradicted by the data. This is methodologically quite different than an exercise in "theoretical curve-fitting" motivated by data already observed.[8]

4. 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 laborious task on the 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 is set by the initial endowments that will subsequently be changed or preserved by Player A via feasible

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[8] Having observed the data, a discussant has argued that a hybrid model containing loss aversion and inequality aversion can fit ("explain") the data. We report such an exercise in the appendix to our paper. It is important to keep in mind the essential distinction between (a) having a testable model that makes predictions that can subsequently be found to be consistent or inconsistent with data vs. (b) first observing the data and subsequently fitting a model to the data.

actions. Feature (iii), however, deserves a few more comments. In Experiments 2 and 3 we opted to have the subjects earn their endowments in order to induce property right entitlements that better justify the labeling of actions (as "give" or "take" and "decrease" or "increase"). 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." The so-called "house money effect" has been documented to encourage risk taking (Battalio, Kagel, and Jiranyakul, 1990; Thaler, 1990; Thaler and Johnson, 1990; Arkes, Joyner, Pezzo, Nash, Siegel-Jacobs, and Stone, 1994; Keasey and Moon, 1996; Cárdenas, De Roux, Jaramillo, and Martinez, forthcoming). Clark (2002) finds no effect of house money in the voluntary contributions mechanism public goods game using unconditional nonparametric methods. Harrison (2007), however, reports that the same data display a significant effect when analyzing responses at the individual level and accounting for the error structure of the panel data.

Several previous studies have found a notable effect of earned (rather than randomly assigned) endowments on subsequent behavior (e.g., Hoffman, McCabe, Shachat, and Smith, 1994; Rutström and Williams, 2000; Cherry, Frykblom, and Shogren, 2002; Gächter and Riedl, 2005, Oxoby and Spraggon, 2008). Cox and Hall (2010) tested robustness of the Cox, Ostrom, and Walker et al. (2009) empirical observation that the behavior of second movers does not differ between common-property and private-property trust games that include a rich strategy space for both players. Cox and Hall had their subjects earn their endowments in a real effort task prior to playing a common-property or private-property trust game and found the behavior of their second movers to be different with earned than with unearned endowments.

We conducted four one-day sessions in Experiment 1, six two-day sessions in Experiment 2 and five two-day sessions in Experiment 3. 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 anonymous (or "double blind").

4.1 Experiment 1: Randomly Assigned Endowments

In Experiment 1 the 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 $T_{10,10}$. The treatments were implemented in a between-subjects design. All sessions were run manually using the strategy method (Selten, 1967; Brandts and Charness, 2011).

In treatment RANDOM $T_{15,5}$ Player A started with \$15 and Player B with \$5. The available choices were described to subjects as follows: Player A had to choose whether to give \$5 to an anonymously paired Player B or to make no change in payoffs. If Player A decided to give \$5, Player B could either make no further change in payoffs or decrease his own payoff by \$1 in order to increase Player A's payoff by \$2. If Player A decided to make no change in endowments, Player B could either make no further change in payoffs or decrease her own payoff by \$2 in order to decrease Player A's payoff by \$6.

In treatment RANDOM $T_{10,10}$ Player A had to choose whether to take \$5 from an anonymously paired Player B or to make no change in endowments. If Player A decided to make no change in endowments, Player B could either make no further change in endowments or decrease his own payoff by \$1 in order to increase Player A's payoff by \$2. If Player A decided to take \$5, Player B could either make no further change in endowments or decrease her own payoff by \$2 in order to decrease Player A's payoff by \$6.

4.2 Experiment 1 Results

We first describe the data and then compare subjects' behavior in three ways: (i) for the whole game trees; (ii) for corresponding subgames; and (iii) for corresponding subgames after eliminating subjects who have not revealed reciprocal preferences.

Sixty-six subjects (or thirty-three pairs) participated in treatment RANDOM $T_{15,5}$ and sixty-eight subjects (or thirty-four pairs) in treatment RANDOM $T_{10,10}$. In treatment RANDOM $T_{15,5}$, twelve (=36.4%) A Players chose to No Change from (15,5) while twenty-one A Players chose to Give 5 to their counterpart Player B. In treatment RANDOM $T_{10,10}$ twenty-six (=76.5%) chose to Take 5 while only eight chose to No Change from (10,10).[9] This difference in A Players' behavior is statistically significant (p=0.001, Fisher's exact two-sided test). [10] The main contribution of the paper is testing the implications of reciprocal preference theory which makes direct predictions for the behavior of Players B. We defer further discussion of A Player's choices until subsection 4.7.

B Players' choices were elicited by the strategy method. Each player B thus made two choices, one for each of the two subgames. Data for All B Players are reported in the upper panel of Table 1.

The data are consistent with the testable implication of reciprocal preference theory; our next question is whether the observed difference in play between the two games is statistically significant. First we test the implication of consequentialist social preference theories that behavior of B Players does not differ between the two treatments. 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. However, 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.

[9] Player A's behavior is summarized in Table 4 in Section 4.

[10] All subsequent p-values in this paper refer to Fisher's exact test. Throughout the paper we report one-sided test in all cases when we have clear theoretical predictions and when the nature of the data allows us to do so (i.e., for behavior of type B players with binary categories); otherwise, we report two-sided tests.

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. This test, which is naturally two-sided, rejects the hypothesis that strategies are the same in the two treatments in favor of the alternative that they are different (p=0.004).

Table 1: Player B Behavior in Experiment 1

	No Decrease	Decrease by 6	No Increase	Increase by 2
All B Players				
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	
Reciprocal B Players				
RANDOM $T_{15,5}$	10/17 (58.8%)	7/17 (41.2%)	5/17 (29.4%)	12/17 (70.6%)
RANDOM $T_{10,10}$	1/15 (6.7%)	14/15 (93.3%)	13/15 (86.7%)	2/15 (13.3%)
Fisher's Test for Subgames	0.002		0.001	

[a] two-sided test.

We next test the implications of reciprocal preference theory for play in each individual subgame. In particular, for the subgame on the left side of the game tree reciprocal preference theory implies that the frequency of "Decrease by 6" will be higher in treatment RANDOM $T_{10,10}$ than in RANDOM $T_{15,5}$. To test this (directional) prediction, we use the one-sided Fisher's exact test which detects a statistically significant difference between frequencies with which the Decrease by 6 choice was selected in the two treatments (p=0.067). For the subgame on the right side, reciprocal preference theory implies that the frequency of Increase by 2 is higher in treatment RANDOM $T_{15,5}$ than

RANDOM $T_{10,10}$. The one-sided Fisher's exact test detects a statistically significant difference (p=0.002).

A final test focuses on individuals who revealed strictly reciprocal preferences by making at least one decision to punish or reward another participant at a monetary cost to themselves.[11] In other words, we exclude B Players who chose No change in both subgames from these tests. These test results are significant in each of the individual subgames (p=0.002 and 0.001, respectively for the left and right subgames).

4.3 Experiment 2: Endowments Earned in a Tournament

Saliency of the status quo in an experiment depends on whether subjects respect the property rights induced through the initial endowments. In Experiment 1, entitlements to the initial endowments were created by a stylized experimental procedure – random assignment. In everyday life, however, entitlements are usually created in a more natural way, for example by exchanging one's skills, effort and time for a payment. In what follows we present two additional experiments that serve as robustness checks with respect to procedures by which entitlements are induced. Our designs mimic two common labor market compensation practices, tournaments and absolute (or fixed) performance targets. Our subjects earn initial endowments by their performance in a math quiz.

In Experiment 2 subjects compete in a tournament which places them in three different groups based on their performance in the quiz. Groups 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

[11] It should be noted that the whole set of reciprocal subjects might be larger than this group.

a \$10 certificate, and those in the bottom 25% received a \$5 certificate.[12] 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 $T_{10,10}$ implemented in a between-subjects design. Day 2 sessions used procedures identical to Experiment 1 with the only difference that the endowments were earned in 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.

4.4 Experiment 2 Results

Seventy subjects (or thirty-five pairs) participated in each of the two treatments in Experiment 2. In treatment TOURNAMENT $T_{15,5}$, twenty-three (=65.7%) A Players chose No Change from (15,5) while twelve A Players chose Give 5. In treatment TOURNAMENT $T_{10,10}$, twelve (=34.3%) chose Take 5 while twenty-three chose No Change from (10,10). This difference in A Players' behavior is statistically significant (p=0.016), suggesting that the status quo is an important consideration for the subjects. Data for all B Players are reported in the upper panel of Table 2.

We proceed to testing with data from Experiment 2. Fisher's test for strategies rejects the null hypothesis in favor of the alternative (p=0.061, two-sided). However, the one-sided Fisher's exact test does not detect a difference between frequencies with which the Decrease by 6 choice was selected in the two treatments (p=0.296). For the subgame

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[12] Note that the tournament procedure puts subjects to treatments based on their performance on the Day 1 task. This is, however, a natural consequence of assigning endowments in this manner and an important part of the robustness-check exercise. A reader might be curious about a possible link between reciprocal preferences and analytical skills. We are not aware of any such result published in the literature. Furthermore, our other two experiments (1 and 3) had random assignment to treatments and the results are very much in line with the results in the tournament experiment. See subsection 4.8 for a comparison of B Players' behavior across experiments.

on the right side, Fisher's test detects that the frequency of Increase by 2 is higher in TOURNAMENT $T_{15,5}$ than TOURNAMENT $T_{10,10}$ (p=0.01).

The lower panel Table 2 reports data for the subset of subjects who revealed reciprocal preferences by making at least one decision to punish or reward. After removing B Players who chose No change in both subgames, the test rejects the null on both sides of the game tree (p=0.039 and p=0.032, respectively, for the left and right subgames).

Table 2: B Players' Behavior in Experiment 2

	No Decrease	Decrease by 6	No Increase	Increase by 2
All B Players				
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	
Reciprocal B Players				
TOURNAMENT $T_{15,5}$	13/21 (61.9%)	8/21 (38.1%)	5/21 (23.8%)	16/21 (76.2%)
TOURNAMENT $T_{10,10}$	4/15 (26.7%)	11/15 (73.3%)	9/15 (60%)	6/15 (40%)
Fisher's Test for Subgames	0.039		0.032	

[a] two-sided test.

4.5 Experiment 3: Earned Endowments by Reaching a Target Output

Experiment 3 presents a second robustness check with respect to procedures by which entitlements were induced. Recall that in Experiment 2 subjects' performance in a tournament determined their initial endowment (and thus also the roles) in Day 2 part of the experiment. In Experiment 3 subjects performed the same earning task of solving GMAT problems, except that their assignment to roles was random. This was accomplished by the following procedure. On Day 1 of the experiment participants were asked to correctly answer 10, 20 or 30 problems, depending on a session they were recruited for. There was no penalty for providing an incorrect answer. 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.

4.6 Experiment 3 Results

Seventy-two subjects (or thirty-six pairs) participated in each of the two treatments in Experiment 3. In treatment TARGET $T_{15,5}$, twenty-six (=72.2%) A Players chose No Change from (15,5) while ten A Players chose Give 5. In treatment TARGET $T_{10,10}$, eighteen (=50%) chose Take 5 while the other eighteen chose Uphold (10,10). This difference in A Players' behavior between the two treatments is weakly significant (p=0.090). Data for All B Players in Experiment 3 are reported in the upper panel of Table 3.

As before, we test the null hypothesis that behavior of B Players does not differ between the two treatments using Fisher's exact test for strategies. With these data the pattern of behavior is in the same direction as with other data but the difference is not significant (p=0.211, two-sided). Next, we proceed with testing with data from the individual subgames. On the left hand side of the game tree we find that the frequency of Decrease by 6 is higher in TARGET than in TARGET (p=0.084). For the subgame on the right hand side, the result of Fisher's exact test reveals that this difference is insignificant in Experiment 3 (p=0.133).

As shown in the lower panel of Table 3, when performing the same tests on reciprocal B Players only, we find significant differences in behavior on both sides of the game tree (p=0.086 and p=0.042, respectively, for the left and right subgames).

Table 3: Player B Behavior in Experiment 3

	No Decrease	Decrease by 6	No Increase	Increase by 2
All B Players				
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	
Reciprocal B Players				
TARGET $T_{15,5}$	8/18 (44.4%)	10/18 (55.6%)	7/18 (38.9%)	11/18 (61.1%)
TARGET $T_{10,10}$	4/21 (19%)	17/21 (81%)	15/21 (71.4%)	6/21 (28.6%)
Fisher's Test for Subgames	0.086		0.042	

[a] two-sided test.

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

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

While the main focus of the current paper is on the reciprocal behavior of B Players, let us start by briefly discussing 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 Give 5 between the RANDOM $T_{15,5}$ treatment where the initial endowments were assigned randomly by the experimenters and treatments TOURNAMENT and TARGET where the endowments were earned (p=0.028 and p=0.004, respectively). The evidence that A

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.

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 giving behavior between TOURNAMENT and TARGET treatments (p=0.614).

Comparison of treatment RANDOM with TOURNAMENT and TARGET 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 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).

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

To assess the impact of earned endowments on Player B reciprocal responses, we compare their behavior in the respective treatments using data categorized by strategies, presented in Table 5.

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 6 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 output (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 either (p=0.606). A similar pattern emerges for the $T_{10,10}$ treatments where the respective p-values are equal to 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.

In light of the weak results in Experiment 3, however, it might seem a bit puzzling. When inspecting the data in Tables 2 and 3, one might notice that this could be driven by a marginally greater percentage of subjects who punished Player A (by

Table 5. 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

choosing Decrease by 6) for *not* giving them 5 in the TARGET $T_{15,5}$ treatment and a slightly lower percentage of subjects who rewarded (chose Increase by 2) Player A for giving 5 than in TOURNAMENT $T_{15,5}$. While this change in behavior was not sufficient to detect significant differences in play between the two treatments, it had implications when using data categorized according to strategies which could be due to entitlements. When designing the experiments we conjectured that property right entitlements depend on the following three factors: (1) opportunity cost of coming to the lab; (2) effort-based performance in the lab; and (3) time spent bonding with earnings. Based on the current data we speculate that in Experiment 3 the opportunity cost of coming to the lab might have dominated the other two factors since the $5 subjects, who had to come to the lab on two consecutive days (along with the $15 subjects), behaved as if they felt entitled to more than $5.[13] In Experiment 2 this effect appears to be muted by subjects' performance in a tournament that may legitimize the differences in payoffs.

Table 6. 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.

Finally, it is also possible that having to earn one's endowment increased the costs of reciprocity which in turn decreased the frequency of punishment and rewarding. However, this is not what we see in the data. Moreover, this conjecture rejected in a recent study by Danková and Servátka (2014), where in a two-player Taking Game the

[13] Recall that the experimental $ were exchanged into NZD using a 3:4 exchange rate and that subjects also received additional NZD 10 for showing up on both days.

extent and frequency of punishment increases when subjects use their earned endowments as opposed to when a windfall endowment is assigned to them by the experimenter.

4.9 Tests Using Pooled Data

Given that we do not find any differences in B Players' behavior across the three experiments, we pool all data together and perform tests for the overall effect. The Fisher's exact test for data categorized according to strategies rejects the null hypothesis that the distribution of play across the four terminal nodes is the same in treatments $T_{15,5}$ and $T_{10,10}$ with very high significance (p=0.000). The pooled data in the strategy form is presented in the bottom two rows of Table 5.

Table 7 presents pooled data on Player B's behavior according to the distribution of play. For the subgame on the left side, Fisher's test detects that the frequency of Decrease by 6 is higher in $T_{10,10}$ than in $T_{15,5}$ (p=0.011). For the subgame on the right side, Fisher's test detects that the frequency of Increase by 2 is higher in $T_{15,5}$ than $T_{10,10}$ (p=0.000). After removing self-regarding B Players who chose No change in both subgames (lower panel in Table 7), the test also rejects the null on both sides of the game tree (p=0.000 for both subgames).

5. 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.

Table 7. Pooled Data on B Players' Behavior

	No Decrease	Decrease by 6	No Increase	Increase by 2
All B Players				
$T_{15,5}$	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	
Reciprocal B Players				
TARGET $T_{15,5}$	31/56 (55.4%)	25/56 (44.6%)	17/56 (30.4%)	39/56 (69.4%)
TARGET $T_{10,10}$	9/51 (17.6%)	42/51 (82.4%)	37/51 (72.5%)	14/51 (27.5%)
Fisher's Test for Subgames	0.000		0.000	

[a] two-sided test.

* Recall that in Experiment 3 one Player B did not provide an answer on the left side of the game tree.

Because payoffs are the same at respective terminal nodes of the two treatments, (unconditional) distributional preference models predict that the second movers (Players B) using the strategy method will make choices with the same frequency distribution over the terminal nodes. That prediction is rejected by the data.

Reciprocal preference theories make different predictions for play in the two treatments. The interpretation of the appendix model in Charness and Rabin (2002) explained above predicts that the frequency of observation of nodes with payoffs (15,5) and (12,9) will be greater in treatment $T_{15,5}$ than in treatment $T_{10,10}$. The data are consistent with that interpretation of the model.

As explained above, revealed altruism theory (Cox, Friedman, and Sadiraj 2008) makes sharp predictions of differences in play across our two treatments; the experiment reported herein was designed as a test of those predictions. Reciprocal preferences incorporating Axiom R of that theory predict that Players B will be more altruistic in both treatments in the right-side subgames, than in the left-side subgames, because the right-side feasible sets are more generous to them. This leads to the first prediction of this model that is tested with our data: that a higher proportion of Players B will choose (12,9) than will choose (9,3) in both treatment $T_{15,5}$ and in treatment $T_{10,10}$. The data do not reject this prediction. This theory has additional predictions that follow from its Axiom S (together with Axiom R) that introduces dependence of reciprocal preferences on overturning or preserving a *status quo ante* feasible set that is more or less generous to oneself. The complete theory, with both axioms, predicts that the frequency of observation of nodes with payoffs (15,5) and (12,9) will be greater in treatment $T_{15,5}$ than in treatment $T_{10,10}$. The data do not reject this prediction.

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 had insignificant effect on second movers' reciprocal responses. The behavior predicted by Axiom S was prevalent in Experiments 1 and 2, but the effect, although visible, was not significant in Experiment 3. However, since we do not observe any significant differences in behavior across the three experiments, we pool the data and find clear support for Axiom S in pooled data as well as in Experiment 1 and 2 data separately. Our results highlight the importance of the clear distinction between acts of commission and acts of omission (see also Blount, 1995; Charness, 2004).

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 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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Appendix: Discussion of Heuristic Applications of Prospect Theory

A.1. Application of Original Cumulative Prospect Theory

It has been argued that cumulative prospect theory (Tversky and Kahneman, -1992) implies that the $T_{15,5}$ and $T_{10,10}$ treatments are *not* isomorphic because of loss aversion relative to the endowments as reference points. Here is a critical examination of this type of heuristic application of prospect theory. Recall that prospect theory models self-regarding preferences on a lottery space. Suppose one views the second mover's payoff at a terminal node as a degenerate lottery. Also suppose that the second mover's payoff at any terminal node is coded as the difference between the money payoff at the node and his endowed payoff (a reference point). Then the value function v($\cdot$) gives utilities for the payoffs at the four terminal nodes in the $T_{15,5}$ treatment as (from left to right in Figure 1.a): v(5-5), v(3-5), v(10-5), and v(9-5). Similarly, the value function evaluates payoffs at the four terminal nodes in the $T_{10,10}$ treatment as (from left to right in Figure 1.b): v(5-10), v(3-10), v(10-10), and v(9-10). These values (or utilities) imply the same choices as does the "economic man" model of choice on a commodity space: choose (15,5) on the left branch and (10,10) on the right branch *in both games*. In this way, a discussant's suggested heuristic application of prospect theory actually implies that the $T_{15,5}$ and $T_{10,10}$ treatments are isomorphic, not the opposite.

A.2. Construction of a Hybrid Model of Loss Aversion and Inequality Aversion

It has also been argued that a hybrid model incorporating loss aversion and inequality aversion can "explain" our data. Here is an examination of this argument. We demonstrate that there are enough free parameters in this type of hybrid model to make it fit our data. On the other hand, slightly different parameters in the same model do not fit our data. Unless there are some agreed criteria for specifying parameters *before* data are observed, we are not sure what the contribution may be. But here is a discussion that explores the question. We report an example of parameters that fit our data and another example of parameters that do not fit our data.

To keep things simple suppose that only Player B is both loss-averse and inequity averse, i.e., suppose that for a given initial endowment e_B, the gain-loss utility function is $v_B(y;\ e_B) = y + \eta \max[y - e_B, 0] - \eta\lambda \max[e_B - y, 0]$. In addition suppose that B is inequity averse $u_B(x, y) = v_B(y) - \alpha \max[x - v_B(y), 0] - \beta \max[v_B(y) - x, 0]$, where x and y are monetary payments received by players A and B respectively. We follow the literature by assuming that $\eta > 0, \lambda > 1$ and $0 \leq \beta < 1 < \alpha$. With this structure the terminal payoffs of Player B in the Give or Pass Game (treatment $T_{15,5}$)become: $u_B(15,5) = 5 - 10\alpha$, $u_B(9,3) = 3 - 6\alpha - 2\eta\lambda(1 + \alpha)$, $u_B(10,10) = 10 + 5\eta - 5\eta\beta$, $u_B(12,9) = 9 + 4\eta - 3\beta - 4\eta\beta$ if $\eta \leq 3/4$ and $u_B(12,9) = 9 + 4\eta - 3\alpha + 4\eta\alpha$ if $\eta > 3/4$. In the Take or Pass Game (treatment $T_{10,10}$) the terminal payoffs are: $u_B(15,5) = 5 - 10\alpha - 5\eta\lambda(1 + \alpha)$, $u_B(9,3) = 3 - 6\alpha - 7\eta\lambda(1 + \alpha)$, $u_B(10,10) = 10$, and $u_B(12,9) = 9 + 3\alpha - \eta\lambda(1 + \alpha)$.

Our data suggests that Player B subjects tend to choose “No decrease” in the left subgame of $T_{15,5}$ and “Decrease by 6” in the same subgame of $T_{10,10}$. The choice is reversed in the right subgame, i.e., tend to subjects choose “Increase by 2” and “No increase.”. For the theory to match the behavior in the left subgame of $T_{15,5}$ it has to be that

$$\eta\lambda) \geq (2\alpha - 1)/(1 + \alpha)$$

and the reverse has to be true for the left subgame of $T_{10,10}$

$$\eta\lambda \leq (2\alpha - 1)/(1 + \alpha).$$

It follows that we obtain a restriction on parameters $\eta\lambda = (2\alpha - 1)/(1 + \alpha)$. This condition is useful for two reasons. First, it implies that in this parametric example the theory can predict the behavior only if the decision maker is exactly indifferent between the actions in the left subgame of both $T_{15,5}$ and $T_{10,10}$.

Second, we can use the condition to construct numerical examples. Consider the following parameters: $\eta = 1,\ \lambda = 2, \alpha = 2$ and $\beta = 8/5$. It can be easily verified that these parameters satisfy the condition above and hence can produce the desired predictions in the left subgame. Similar incentive conditions hold for the right subgame and it can be verified that the parameters satisfy these conditions too. However, if we were to slightly perturb the parameters, e.g., set $\beta = 1$ or $\eta = 1/2$, the incentive conditions would have been violated and the theory would no longer align with the behavior. It may be possible to enhance the robustness of the theory by making it richer, i.e., by supposing that Player B internalizes the loss aversion (and perhaps also inequity aversion) of Player A. Notice, however, that this would come at the cost of adding another two (or four) parameters to the utility function, giving it even more degrees of freedom and making it harder to reject.