Sorting in Experiments*

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Abstract

Experiments provide a controlled environment where factors can be isolated and studied more easily than in the real world. But experiments are often challenged on the issue of applicability of results to the real world. A major feature of experiments is that they select subjects randomly, while markets do not draw individuals randomly. Markets allow people to sort to certain activities and away from others based on their preferences, beliefs and skills.

We design an experiment to demonstrate the importance of sorting in the context of social preferences. When individuals are constrained to play a dictator game, 74% of the subjects share. But when the same subjects are allowed to avoid the situation altogether, less than one third tend to share. This dramatic reversal of proportions demonstrates the importance of taking sorting into account when applying experimental results to the real world.

We also show that institutions designed to entice pro-social behavior may actually induce adverse selection. Increasing the total surplus available for sharing induces first those individuals who are least willing to share to sort back into the dictator game. Thus the impact of social preferences remain much lower when sorting is possible than in a mandatory dictator game, even if sharing is subsidized by higher payoffs.

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I. Introduction

Experiments are an important part of every science. Economics is no different. The ability to do experiments in a laboratory environment provides insights into behavior that cannot be studied easily in the real world. Control is the key. The experimental structure allows the scientist to answer the "what ifs" that cannot be answered in the complex, ever-changing, and simultaneous structure of the real world.

The controlled and artificial environment is also potentially a drawback. It is unclear how applicable the results of laboratory experiments are in the real world. Many criticisms of experiments have been successfully addressed in the past. Obvious examples are concerns that the stakes are too small, the experiences too unusual, or the setup too unrealistic to be applicable in the real economy.

The point of this paper is different. A key feature of experiments is the random nature of the experimental samples. The random assignment of subjects to experimental tasks is a strength of experiments, for example to evaluate treatment effects relative to a baseline setting. But it is a weakness in terms of real-world applicability. While, in the laboratory, subjects are locked into the experimental environment and forced to play the game presented to them, markets operate differently. In markets, individuals sort, based on their preferences, beliefs, and skills. The ability to sort implies that we cannot draw direct inferences about the relevance of behavioral response to experimental treatments for the real world.

For example, an experiment run on randomly selected individuals might reveal that a large portion of the subjects suffer from acrophobia. But sorting and voluntary selection ensure that those who build skyscrapers are unlikely to be among the sufferers. The wage premium paid in the market reflects the preferences of the marginal individual employed, not the average individual in the population.[1] If there are a sufficient number of non-acrophobic construction workers, there will be no wage premium at all for working at height. It is equally conceivable that sorting exacerbates the effect.

[1] The literature on equalizing wage differentials and hedonic prices, following Rosen (1974), elaborates on this insight..

Overconfidence, for example, may be a rare feature in the overall population. But those who sign up for a health club membership may be particularly prone to overestimating their future self-control, which would explain the low average rate of attendance of members who pay a high monthly fee.[2]

Both examples illustrate the power of sorting. Experiments that do not allow for sorting describe the preferences of the average individual and not the marginal player, whose behavior is relevant for determining prices and outcomes.[3] Whether the results of an experiment overstate or understate what is observed in the market depends on the relation of the marginal individual's preferences to those of the average individual. In this paper, we provide results from an experiment which show that the importance of a particular phenomenon, namely sharing, in the absence of sorting is reversed when sorting is permitted.

Although this criticism is fundamental, it is easily addressed. Experiments rarely give subjects the opportunity to opt out or to choose alternative tasks. If selection is an important force in economic decision-making, then the applicability of experiments without sorting to the real world is not straightforward and requires adjustment for endogenous selection. The analysis below provides an example of an adjustment of this sort and demonstrates the importance of making it.

A second point of our analysis is that the design of institutions and markets may exacerbate the discrepancy between the behavior of randomly drawn samples in experiments and self-selected samples in markets. Markets may actually select those individuals whose behavior is furthest from that of the average member of the population. And policy interventions or institutions targeting the average individual may affect the individual with the most perverse, or at least most extreme preferences.

To pin down ideas, consider the example of giving money to a beggar on a street corner. The first point of our analysis, applied to this context, is that experimental

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[2] See Della Vigna and Malmendier (2003), who show using real world data that subscribers to health clubs do not take into account their own behavior to minimize the costs of their subscriptions.

[3] Lazear (1990) argues that the difference between the classic Skinner experiments is that pigeons do not have the right to work for another experimenter if they do not like the environment. Workers, on the other hand, are not forced to remain with firms that do provide distasteful reinforcement schedules.

subjects who have just generously shared in a dictator game may not give any of their experimental earnings to the beggar outside the lab. Rather, they may cross the street to avoid encountering the beggar. Our second point is that those who do not look the other way or cross the street are most likely to be of two extreme types - the most and least compassionate. There are two reasons to give: Individuals may derive utility from giving to others. Or they do not derive utility from giving but they derive disutility from not giving when faced with the request to do so. The latter can be avoided by removing oneself from the situation. Those who enjoy giving pass by the beggar – and give. Those who do not enjoy giving and do not experience disutility from not giving in response to a request also pass by the beggar – and do not give. The middle group, who do not want to give, but dislike not giving when faced with the potential beneficiary, cross the street. Thus, the beggar sees the most compassionate people and as well as the least caring.[4] In this case, the market selects in the most extreme types, not the median individual.

Suppose now that policy-makers would like to induce more giving and set incentives to pass by the beggar. They may count on the median person to dislike not giving to a person confronted with. Sorting, however, will diminish giving. People who like sharing are giving already. Among the people who are not giving already, the incentives will affect most strongly those who experience the least disutility from not giving. Those people, however, give less than the median person. As a result the policy intervention will be less effective than predicted on the basis of average behavior in the overall population.

In economic language, there is a price at which individuals are willing to confront the beggar. Individuals who do so at the lowest price are (1) those who derive the most pleasure from giving and (2) those who suffer the least from not giving money despite being confronted with a request. Those who will not give, but who dislike intensely walking past the beggar and not giving, are most likely to cross the street. To get them to walk by the beggar, the highest payment is required.

[4] Those who like to give may care about the utility of others, enjoy the praise that giving brings, or for other reasons. Such considerations, although interesting, are not central to our argument.

Sharing and Sorting. To make concrete the importance of selection and heterogeneity, we employ a modified dictator game. In typical dictator games, one of two anonymously matched subjects decides how much of a given surplus should be sent to the other person and how much to keep. The standard result is that a significant proportion of subjects give some positive amount to an anonymous responder, even when their action is not observable by anyone, including the experimenter (see Camerer, 2003; Hoffman, et al., 1994). For example, in a simple binary choice experiment similar to the standard dictator game in which subjects choose between a ($10, $10) and a ($18, $2) allocation, 76 percent of subjects choose the "fair" option (Kahneman, Knetsch and Thaler, 1996).

Sharing behavior has been largely interpreted as reflecting a taste for fairness among a significant number of, if not a majority of, economic decision-makers. Several models attempt to explain this behavior as reflecting a stable preference for equitable outcomes or altruism (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). We point out that truly other-regarding preferences are not the only reason to share. Alternatively, individuals may simply feel compelled to give upon request but would prefer to avoid the sharing situation in the first place. A taste to avoid giving may reflect shame at not giving or other forces. We will provide evidence that sheds some light on these distinctions.

The Theory. We present a theoretical framework that demonstrates our two main points: First, sorting is an important force in determining the importance of sharing. The amount of sharing may decrease significantly if individuals have the option to sort out of the sharing opportunity. Second, making the sharing option more attractive may first and foremost attract those individuals who are least willing to share.

Our theoretical framework allows for individuals to have other-regarding preferences. That is, individuals may derive utility not only from their own payoff but also from the payoff others receive. However, we also allow the utility to depend on the environment. In particular, the utility an individual derives from the payoff of others' may higher when the individual has the opportunity to share the own payoff. As a result, some individuals may seek out opportunities to share while others prefer to avoid them. A taste for giving might reflect altruism or the desire to receive praise. A taste to avoid

giving may reflect shame at not giving or guilt or other forces. [BRING BACK TRHEE TYPES?]

We show that sorting induces sharers to opt out and reduces the probability and the total amount of sharing. Moreover, increasing the total surplus available for sharing while holding the outside option constant induces first those individuals who are least willing to share to sort back into the dictator game.

The intuition is that people who are not motivated by true other-regarding preferences, but instead by shame or guilt may want to avoid settings in which they have the option to share. On the other hand, people who experience no such shame or guilt but who behave self-interestedly have a higher willingness to play the dictator game, especially when a premium is offered for playing it.[5] Among people who are motivated by shame or guilt, a measure of the amount of shame or guilt that they feel is the amount that they give to others when constrained to play the dictator game. Those who give more in dictator games because they dislike not giving should be more willing to avoid putting themselves in such situations – possibly even paying a cost to do so – than people who give little. Environments that put individuals in situations where sharing is an option tend to attract people who are least willing to share in addition to those who enjoy sharing.

The Experiment. We test these hypotheses in an experiment in which subjects can sort between environments that do and environments that do not allow sharing. The experiment has three stages. In the first stage, individuals play a dictator game, which we use to determine their propensity for sharing. In the second stage, they are offered an alternative between playing the dictator game and "opting out." If they opt out they simply receive a fixed payment and play no game. In that case, the (potential) receiver never finds out that a dictator game could have been played. The decision to switch to the new environment distinguishes agents who were initially sharing because of shame or guilt from those who are truly altruistic or fair. In the third stage, the total surplus of the

[5]The distinction between shame and guilt has to do with observability. Shame works only when others see the action taken. Guilt operates even in the absence of observation by others. See Kandel and Lazear (1992) for a theoretical treatment of this subject.

dictator game increases while the fixed amount in the alternative environment remains constant.

There are two main results. First, sorting significantly reduces the probability of sharing. When subjects are locked into the experimental context and forced to choose between sharing or not, 74% share. But when subjects are given the choice to avoid the situation altogether, which implies no sharing, only 30% of the subjects share. In other words, without choice most share; with choice most do not. The average percentage shared decreases from 27% to 12%. Overall, 42% of the subjects feel compelled to share if they cannot avoid the situation, as identified by sharing in the first round and then opting out. Only 25% have purely self-regarding preferences (as defined by not sharing in the first round), and 33% have other-regarding preferences (as defined by sharing in the first round and in the second round).

Second, allowing choice selects a particular type of player. Those most likely to put themselves in the sharing context come from two extremes. They are the most likely to share and also the most unlikely to share. Among those who share because they dislike not sharing (as opposed to liking sharing), those most likely to participate are the most immune to the social pressure of not sharing. Those who feel more inclined to share because they dislike being viewed as unfair, shun the sharing environment altogether and it requires a high premium to induce them to return to the sharing context. In fact, as predicted by our model we find that, among individuals who switched in the second round, those who shared most initially will switch back to the dictator game last (i.e., only when the compensation in the dictator game is large). In other words, if shame or guilt are important factors in fair behavior, markets that allow sharing will first and foremost attract people who are less willing to share.

Thus, choice induces the "best" and "worst" to play and, in the range inbetween, it is hardest to get high sharers to play. Results from the experiment that randomly chooses subjects and locks them into the sharing context camouflage the sorting phenomenon.

Our paper builds on a considerable body of work on dictator, ultimatum, and trust games (see Camerer, 2003, Chapter 2) revealing that altruistic and fairness-minded

behavior is largely robust to several experimental treatments (such as monetary stakes, anonymity, etc.). There is also a small experimental literature suggesting that the dictator game findings may not be driven by mere fairness considerations (Dana, Weber, and Kuang 2003; Oberholzer-Gee and Eichenberger 2004). Further, a number of experimental papers have been concerned with selection in other experimental contexts such as the prisoner's dilemma (Bohnet and Kübler 2004), the choice of reward and punishment (Sutter, Kocher, and Haigner 2003), and incentive contracts (Eriksson and Villeval, 2004).

The paper proceeds as follows. In the next Section, we describe our model of sharing decisions for different types other-regarding and context-dependent preferences. In Section 3, we present the experimental design. Section 4 presents the results and data analysis. Section 5 concludes.

II. Model

We employ a model that allows for three types of sharing preferences self-regarding, other-regarding and "shame" or "guilt" driven. The model generates testable predictions about the relationship between the decision to share and sorting.

The agent is endowed with an amount *w*, which she has to divide between herself and another agent, similarly to the decision in a dictator game. Denote the amount of *w* that the agent allocates to herself as *x* and the amount that she allocates to the other agent as *y*. Thus

$$x + y = w .$$

We assume the following Cobb-Douglas utility specification:

$$\begin{aligned} U(x,y) &= x^{\alpha} y^{1-\alpha} \\ &= x\left(\frac{y}{x}\right)^{1-\alpha} \end{aligned}$$

Agents with purely self-regarding preferences are only concerned about their own payoff, i. e. $\alpha = 1$ and $U(x,y) = x$. Agents with other-regarding preferences also care about the

payoff for the other person, i. e. $\alpha < 1$. We assume $\alpha \geq \frac{1}{2}$, i.e., the agent puts at least as much weight on her own consumption than on the other agent's consumption. Or, as revealed by re-writing the utility function in the form that includes the ratio of y to x, an agent with other-regarding preferences does not only care about her own level of consumption, x, but also about the ratio of y to x. Finally, agents who feel "shame" or "guilt" do not truly care about the other agent's consumption. Rather than sharing, she would altogether prefer to avoid the environment that permits sharing. In a non-sharing environment, in which she is not directly confronted with the option to share, this agent's utility can then be described in the classical framework, i. e. $\alpha = 1$ and $U(x, y) = x$.[6]

In an environment with the salient option to share, both agents with self-regarding preferences and shame-driven agents maximize the utility function $U(x, y) = x^{\alpha} y^{1-\alpha}$ and choose to allocate

$$x^* = \alpha w$$
$$y^* = (1-\alpha) w$$

The higher α, the more will the agent allocate to herself. In fact, $\partial x^* / \partial \alpha = w$ and $\partial y^* / \partial \alpha = -w$. Agents with purely self-regarding preferences have $\alpha = 1$ and set $x^* = w$ and $y^* = 0$. The parameter α determines how much an individual shares. For $\alpha = 1$, the individual is completely unfair, whereas were $\alpha = ½$, the individual might be thought of as completely fair, splitting the amount equally. This is consistent with other-regarding preferences and with the shame interpretation of the model.[7]

We now introduce sorting and the option to obtain w without facing the decision to share. Faced with this additional option, agents with purely self-regarding preferences

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[6] More generally, α may depend on the "visibility" of the sharing option, either to the agent herself or to the "society." For an agent acting on "shame," the more visible sharing is, the lower the α. In the empirical analysis, the level of α is not arbitrary because we compare the non-sharing and the sharing environment.

[7] As in most experiments, the amount to be divided is treated as the total relevant amount. Deeper questions involve subjects' assumptions about other subjects' wealth levels. For example, even an altruist might keep all for herself if she believed that the other subject was wealthier than she.

are indifferent between sorting out on the one hand and continuing to play the dictator game and allocating the full surplus to themselves on the other hand:

$$U_{DG}(x^*, w - x^*) = U_{Out}(w, 0)$$

If agents have other-regarding preferences, however, the utility attained at the optimum under sharing, $U(x^*, y^*)$, is higher than the utility attained in an environment that does not allow for sharing:

$$U_{DG}(x^*, w - x^*) > U_{Out}(w, 0)$$

For an agent who shares because of shame, guilt, or social pressure, the utility attained at the optimum, $U(x^*, y^*)$ is instead smaller than the utility in a non-sharing environment since, with $\alpha \in [0.5;1)$

$$\begin{aligned} U_{DG}(x^*, y^*) &= (\alpha w)^{\alpha}(1-\alpha)^{1-\alpha} w^{1-\alpha} = \alpha^{\alpha}(1-\alpha)^{1-\varepsilon} w \\ &< U_{Out}(w, 0) = w \end{aligned}$$

Thus, if the agent acting on guilt or shame can choose between the environment with and the environment without sharing, she will choose the one without, whereas the altruistic agent would not want to miss the opportunity to give to the other agent.

We can summarize the empirical implications of the results in two predictions, which contrast the effects of sorting in a world without and with shame-driven agents. As a null hypothesis, assume that sharing is only driven by true other-regarding preferences and shame or guilt does not play a role. In this world the following two predictions hold:

Prediction 1. *Allowing to sort out of the dictator game and reap the full surplus w induces only those agents (if any) to sort out who do not share in the dictator game without sorting.*

Prediction 2. *Allowing to sort out of the dictator game and reap the full surplus w affects neither the amount nor the frequency of sharing.*

Alternatively, consider a world where sharing is also driven by shame or guilt. In this world, the following two alternative predictions hold:

Prediction 1'. *Allowing to sort out of the dictator game and reap the full surplus w induces some agents to sort out who share in the dictator game without sorting.*

Prediction 2'. *Allowing to sort out of the dictator game and reap the full surplus w reduces both the amount and the frequency of sharing.*

Suppose now that in order to induce more agents to play the dictator game and to share, we increase the total surplus available in the environment with sharing. It is easy to see all agents with purely self-regarding preferences switch immediately back to the dictator game (if they ever opted out). For shame-driven agents, the sorting decision depends on the size of the increased surplus. The threshold surplus $\hat{w}$, at which a shame-driven agent is willing to switch (back), i. e. at which she is indifferent between a straight transfer of w and a surplus of $\hat{w}$ with sharing, is defined by the equation

$$w = (x^*(\hat{w}))^{\alpha}(\hat{w} - x^*(\hat{w}))^{1-\alpha}$$
$$\Leftrightarrow w = \alpha^{\alpha}(1-\alpha)^{1-\alpha}\hat{w}$$

To show that $\hat{w}$ is increasing in the amount of initial sharing, y^*, we use that y^* is strictly decreasing in α. Hence, it suffices to show that $\hat{w}$ is decreasing in α. Applying the implicit function theorem, we find

$$\frac{d\hat{w}}{d\alpha} = -\ln\left(\frac{\alpha}{1-\alpha}\right)\hat{w},$$

which is negative for $\alpha \in [0.5;1)$.

We can summarize the result in the following prediction. Again, consider first a hypothesis without shame (or guilt or social pressure).

Prediction 3. *Increasing the total surplus available in the dictator game while holding the outside option constant affects neither the frequency of sharing nor the portion shared.*

Alternatively, consider again a world where sharing is also driven by shame or guilt.

Prediction 3'. *Increasing the total surplus available in the dictator game while holding the outside option constant induces agents who initially shared but then opted out to*

switch back. The premium required to induce such agents to sort back is increasing in the amount they shared in the initial sharing environment.

To illustrate Prediction 3' in an example, shame-driven individuals who have $\alpha = 1$ are indifferent between being the dictator over \$10 and receiving a straight transfer of \$10, where as shame-driven individuals whose α is equal to ½ would require \$20 in the dictator game to be indifferent between that and a game that provides them a straight \$10. Because they wish to avoid the shame associated with keeping money to themselves, low α individuals give much to the other individual in round 1 and require a high premium in round 2 to choose a dictator game over a game that simply gives them a straight \$10.

Although this utility function is a special case, it provides a nice framework for analyzing outcomes. It allows us to consider both variations in α and to contrast agents who are affected by the "visibility" of their choices and those who are not. The prediction that the portion shared by agents with pure self-regarding or with other-regarding preferences is constant is, of course, specific to the Cobb-Douglas specification. As we will see in the data, though, it captures the actual choices of these subsets of agents quite well.

III. Experimental Design

Our experiment consists of three parts. In all parts, subjects have the opportunity to play a simple dictator game in which they decide upon an allocation of some amount w between themselves (x) and another participant (y). In parts 2 and 3, we introduce the possibility of sorting out of the game ("passing"). In part 2, a potential dictator who sorts out receives a fixed sum (\$10) and the potential recipient never finds out about the game. In part 3, the amount w available in the dictator game rises while the sum dictators receive after opting out remains constant.

All three parts of our experiment are conducted in two different forms. Those two treatments vary in the extent to which dictators are anonymous. In the Anonymity treatment, the identities of dictators who chose to play the game are kept from recipients,

meaning that recipients find out how much they receive, but not who sent it. In the No-Anonymity treatment, the identities of dictators who choose to play the game are revealed to the recipients at the end of the experiment, meaning that recipients find out both how much they receive and who sent it. We conducted these two treatments because a) the Anonymity treatment more closely corresponds to how the dictator game is usually implemented in economics experiments (see Camerer, 2003), and b) the No-Anonymity treatment closely corresponds to how many sharing decisions are made outside the laboratory. Using these two treatments also allows us to explore the robustness of our theory to variations in the anonymity of the potential dictator and also possible differences between guilt and shame, which we discussed previously, as motivations for sharing.

In both treatments, each session consisted of an even number of between 10 and 20 participants and lasted about 30 minutes. Upon arriving at the experiment, subjects were told that they would receive a $6 payment for their participation in the experiment and that, in addition, they might receive additional money during the experiment. Subjects were then randomly assigned participant numbers and were told that participants with numbers between 11 and 20 (or, in the case of smaller groups, the half with the highest numbers) should collect all their belongings and follow one of the experimenters to another area. While all of the subjects were still in the main room, the experimenter publicly announced that these participants would complete a series of questionnaires for about 20 to 25 minutes and that they would not receive additional money from the experimenter for doing so.

Once participants with the higher numbers were outside the main room, the remaining participants received a set of sheets that contained a series of questionnaires. On the front page, subjects were asked to proceed through the questionnaires at their own pace. Once they finished, they were told to wait quietly for additional instructions.

They were then told that they would make a series of decisions (5 decisions in the Anonymity treatment, 6 decisions in the No-Anonymity treatment), and that at the end of the experiment one of these decisions would be randomly selected by drawing a number out of a bag. This decision would be the only one that counted and would determine

payoffs. Subjects were told that they would make each decision sequentially and that they would receive new instructions and materials for each decision.

Decision 1

In both treatments, Decision 1 consisted of a dictator game without a sorting option. That is, each subject played a $10 dictator game in which he or she was matched with one of the participants outside the room. Subjects were told that if Decision 1 was selected to count at the end of the experiment, then the participants outside the room would be brought back in to the room. The experimenter would describe the dictator game publicly to these participants, and then each of the recipients would find out how much money he or she had been given. The only difference between treatments was in whether the recipient would also find out the identity of the dictator with whom he or she had been matched.

Subjects received an instruction sheet (describing the decision) and an envelope. They were told not to open the envelope until after the instructions were read and questions were answered. Inside the envelope they would find a sheet with a number corresponding to one of the participants who had left the room.[8] They would be matched with this participant for this decision. On the sheet, dictators would write their own participant number and indicate a division of 40 tokens (each worth 25 cents), specifying how much to keep and how much to give to the other subject.[9]

In the Anonymity treatment, if Decision 1 was selected to count, then the recipients would find out only the amount and the participant number of the subject who sent that amount. This was done by having the experimenter show each recipient the sheet filled out by the dictator. In the No Anonymity treatment, the recipients would in addition find out the identity of the dictator with whom they had been matched. This was done by having the dictators themselves hand the sheets to the recipients with whom they had been matched.

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[8] Random matching in each period was implemented by shuffling the envelopes, distributing them in a different order to dictators, and allowing them to select from the stack that remained.

[9] Subjects were told that if the numbers did not add up to the allocation, then the amount to the recipient would determine the allocation (the dictator would receive the remaining amount). This did not occur.

After receiving the instructions and being given the opportunity to ask questions, dictators were told to open their envelopes for Decision 1, write their participant numbers, and indicate their chosen division. The experimenter then collected the envelopes and placed them aside.

Decision 2

In Decision 2, participants 1-10 had the opportunity to play exactly the same dictator game as in Decision 1, although with a potentially new randomly selected participant. If they chose to play the dictator game, and if Decision 2 was selected to count, then the participant with whom they were matched would be brought back into the room and informed of the game in the same way as would have occurred in Decision 1. If they chose not to play the game, then dictators would receive a payment of $10 without having to make a choice. In this case, the potential recipient outside the room would be told nothing about the dictator game.

Subjects received an instruction sheet and two envelopes, one labeled "Play" and another labeled "Pass." Subjects were instructed not to open either envelope until after the instructions had been read aloud.

The instructions informed subjects that they would have the opportunity to play exactly the same game as in Decision 1, but that they could also choose not to play the game, by "passing." If he or she chose to play the game, the subject would open the envelope marked "Play," see the participant number of the person outside the room with whom he or she was matched, and specify a division of the 40 tokens. If the subject chose to not play the game, then he or she would open the envelope marked "Pass" (which did not contain a participant number) and mark an "X" on the sheet inside.[10] After making either choice, subjects returned the envelopes to the experimenter.

Remaining decisions

[10] This was done to ensure that people playing and passing wrote roughly the same amount on the sheets. In this way, looking around to see how much people were writing would not reveal what others were doing. In addition, the experimenter collected the envelopes with the labeled side facing down, so that subjects could not observe what others had done by which envelope they handed to the experimenter first.

The session then proceeded in sequence through the remaining three (Anonymity) or four (No Anonymity) decisions. Each of these decisions proceeded almost identically to Decision 2, with the exception that the amount of money (tokens) to allocate in the dictator game increased. Table 1 presents the amount that the dictator could receive to allocate – if he or she chose to play the dictator game – for each decision.[11] As in Decision 2, the subject could opt to play the dictator game by opening the envelope labeled "Play." In this case, if the decision was selected to count, then the subject with whom the dictator was matched would be brought back into the room at the end of the experiment and would receive a description of the game, and would find out how much he or she had received. In the No Anonymity treatment, the recipient would also find out the identity of the dictator. On the other hand, if the potential dictator chose to "Pass" (by opening the corresponding envelope) then, if the decision was selected to count, the potential recipient was excused from the experiment and not told anything about the game.

All payment and sorting features are summarized in Table 1. As also reported in Table 1, we conducted six sessions in each treatment, with a total of 94 dictators (46 in the Anonymity treatment, 48 in the No Anonymity treatment).[12] The large majority of subjects were undergraduate students of the University of Pittsburgh. 54% of the dictators were female. Including subjects in the role of potential recipient, we used 188 total subjects.

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[11] There are two reasons why the parameters (number of decisions, allocation) differ between the two treatments. First, we initially conducted Anonymity sessions with the same payoffs and structure as in the No Anonymity sessions. We found that the steeper payoffs (relative to those for Anonymity in the table above), meant that a majority of dictators opted out of the game in Decision 2 ($10 allocation), but almost all of them opted to play the game by Decision 4 ($13 allocation). Since part of our goal was to obtain variance in "re-entry" to the game, we modified the payoffs. Second, we also decreased the number of rounds to allow the experiment to run more quickly.

[12] One subject was accidentally allowed to participate twice (both times in the role of dictator). We omitted this subjects' second participation from the data analysis. Since subjects' choices were never revealed to anyone else until the end of the experiment, it seems very unlikely that this subject might have contaminated the choices of other dictators in the same (second) session.

IV. Empirical Analysis

A detailed documentation of the behavior of (potential) dictators in all rounds and both treatments is in Appendix-Tables 1 and 2. In this section, we will first give a broad overview over the empirical findings and then specifically address Predictions 1 to 3 (versus Predictions 1' to 3').

The main features are summarized in Figures 1 (No Anonymity) and 2 (Anonymity). Each figure presents the average total amount that potential dictators shared in each decision (bars and left axis), the total percentage of potential dictators who chose to play the game (dashed line and right axis), and the percentage of the pie shared by those who chose to play (solid line and right axis). Note that in the first decision, all potential dictators were required to play the game.

Decision 1 differed between the treatments only in the anonymity of the dictator. If the decision was selected to count, then in the Anonymity sessions the recipient would find out only the participant number of the dictator, while in the No Anonymity sessions the recipient would find out the dictator's identity. Comparing Decision 1 in the two treatments we find that, as expected, the lack of anonymity produced slightly more sharing when there was no anonymity (the average allocation was \$2.42 in Anonymity and \$2.92 in No Anonymity). However, this difference is not statistically significant.[13]

Following Decision 1, the general pattern is similar between the two treatments. In Decision 2, over half of the participants opt out of playing the game. Among those who opt to play the game, the average amount of sharing is slightly higher than in Decision 1 (see the solid line in each graph). However, the total amount shared per potential dictator (indicated by the bars) decreases substantially (to \$1.22 in Anonymity and \$1.17 in No Anonymity). That is, even though the level of sharing among those choosing to play the game is slightly higher in Decision 2, the effective amount shared by all potential dictators (taking into account those who opt out), decreases by roughly half in the Anonymity treatment and by slightly more in the No Anonymity treatment.

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[13] The significance fails to reach 10% either using a t-test ($t_{92} = 1.17$) or a Kolmogorov-Smirnov test ($D_{46,48} = 0.19$).

General trends in subsequent decisions are also similar between the two treatments. In both treatments, subsequent decisions increased the amount to be allocated in the dictator game. This produces increased entry into the dictator game (indicated by the dotted line) and increased effective sharing (indicated by the bars).

Increasing the amount to be allocated in the dictator game produces an increase in the number of subjects choosing to play the game and also increases the effective allocation amount. However, in both treatments, the percentage of subjects who choose to play the dictator game remains well below 100 percent, even for dictator games with greater total value than the outside option of $10. That is, a significant proportion of potential dictators chooses to "pass" on the game, even when playing the game presents potential allocations that strictly Pareto-dominate the outside option. In the No Anonymity treatment, the number of potential dictators choosing to play the game only reaches 100 percent when the value to allocate reaches $20 (though it is close, at 90 percent, when the value is $16). In the Anonymity treatment, the proportion of subjects choosing "play" never rises above 76 percent.

The effective allocation amount per subject also fails to reach the level in Decision 1, even when the value of the dictator game is greater than the $10 value in the first decision. For instance, in Decision 4 of the No Anonymity treatment, in which the dictator can allocate $13, the effective average allocation is $2.07, which is well below the amount allocated in Decision 1 ($2.92).[14] In the Anonymity treatment, the effective allocation amount in Decision 5, where the dictator game is worth $12, is $1.52, which is also below the average allocation in Decision 1 ($2.42).[15]

The above increases are larger in the No Anonymity sessions, which is likely due to the more rapid increases in the total value of the dictator game. For instance, by Decision 5, the entry rate is 90 percent in the No Anonymity sessions and the average allocation per potential dictator is $3.21. In the Anonymity sessions, however, the corresponding figures for Decision 5 are 76 percent and $1.52. However, this comparison neglects the fact that the potential allocation amount in the No Anonymity

[14] This difference is significant at $p < 0.01$ in a paired t-test ($t_{47} = 2.73$).

[15] This difference is significant at $p < 0.01$ in a paired t-test ($t_{45} = 2.73$).

treatment is \$16, while in the Anonymity treatment it is \$12. A better comparison, then, consists of comparing decisions with comparable allocation amounts. For instance, Decision 3 in the No Anonymity treatment and Decision 4 in the Anonymity treatment both offer the opportunity to play an \$11 dictator game. For these decisions, the figures are 58 percent and \$1.51 for No Anonymity and 74 percent and \$1.42 for Anonymity.[16]

We now analyze the experimental data to test the predictions developed in Section II. To address Prediction 1 versus 1', we first ask whether any subjects who share a positive amount in Round 1 choose the option to sort out in Round 2. Table 2 provides the answer. 58.51% of the dictators choose to opt out in round 2, and the vast majority (39 out of 55) shared positive amounts in Round 1. Overall, we find that 25% do not share initially as predicted by purely self-regarding preferences. 33% of subjects share initially and continue to play and share in the dictator game in round 2, as predicted by other-regarding preferences. The largest group of subjects, however, shares initially, in Round 1, but sorts out of the dictator game in Round 2 (44%). These three types of decision-makers closely resemble the three different types of agents considered in our model: agents with self-regarding preferences, agents with other regarding preferences, and agents who are affected by shame, guilt, or social pressure. These findings reject Prediction 1 in favor of Prediction 1'.

Interestingly, the subjects who exhibit self-regarding or other-regarding preferences do in fact behave quite consistently over time and share a roughly constant portion of the available surplus, matching rather closely the Cobb-Douglas specification; see Figures 3 and 4, in particular 3c and 4c.

Turning to Prediction 2, we ask whether and how the frequency and the amount of sharing are affected by the option to sort out, as introduced in Round 2. As shown in Figures 6a and 6b, allowing for sorting reduces the frequency of sharing from 74% to 30% and the average percentage shared from 27% to 12%. Table 3 presents the results in

[16] While the difference in entry rates is marginally significant ($\chi^2_{(1)} = 2.54$)), the difference in the effective amount shared by dictators is not ($t_{92} = .20$).

a regression framework and provides evidence that the decrease in significance is statistically and economically highly significant, after controlling for the degree of anonymity, gender, and after clustering by person (column 4). These findings reject Prediction 2 in favor of Prediction 2'. We find a large negative effect of sorting on the degree of sharing. As expected, the effects are driven by the Anonymity treatment. The option to sort out of the dictator game is more attractive if it allows dictators to avoid having to face the receiver.

Finally, we analyze endogenous sorting as total surplus available in the dictator game increases. As indicated in Figures 7a-7c, the percentage of total sharing increases over round 2 to 5, again mostly driven by the No-Anonymity sample. The level of sharing does not reach the initial level in Round 1 at any point, even though most dictators have sorted back into the dictator game in the last round. We then test specifically the relationship between the amount initially shared and subsequent sorting behavior for the subset of shame-driven people (i. e. those who initially shared and then opted out). As predicted by our model, we find a negative correlation. Those who shared the most require the highest amount to be induced to sort back into the dictator game.

All findings strongly support not only the existence of "shame" or "guilt" as an important explanation for sharing. Our findings also confirm our main hypotheses: Sorting significantly affects the extent of sharing and increased incentives not to opt out has most effect on those who are less willing to share.

V. Conclusions

The research presented in this paper aims at highlighting the effects of sorting in economic environments. Our work is motivated by the observation that in the real world people regularly sort in and out of different kinds of economic environments such as firms, markets, and institutions, but that in the laboratory these sorting decisions are largely ignored. Instead, subjects are typically placed in one particular kind of situation where they are forced to make a choice that they might avoid making outside the

laboratory. Our goal is to allow these kinds of sorting decisions by economic agents and explore their impact in both a theoretical model and in laboratory experiments. Our model, based on a different motivation for sharing than what is present in most current behavioral models, makes some counter-intuitive predictions about what effects sorting will have. Specifically, we predict that sorting will lead those who appear to be most fairness-minded in the forced-choice experiments to be more likely to avoid environments where they can act fairly. We conduct an experimental study that allows for sorting and find strong support for our hypothesis.

We plan to extend this work to explore the effects of sorting on other kinds of social preferences such as reciprocation and intrinsic motivation. While we expect that sorting might produce outcomes that are less consistent with these preferences, it is worth noting that we do not mean to imply that these preferences do not exist or matter. Instead, we argue that the possibility of sorting might mitigate their impact outside the laboratory.

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Figure 1. No Anonimity (6 sessions; N = 48)

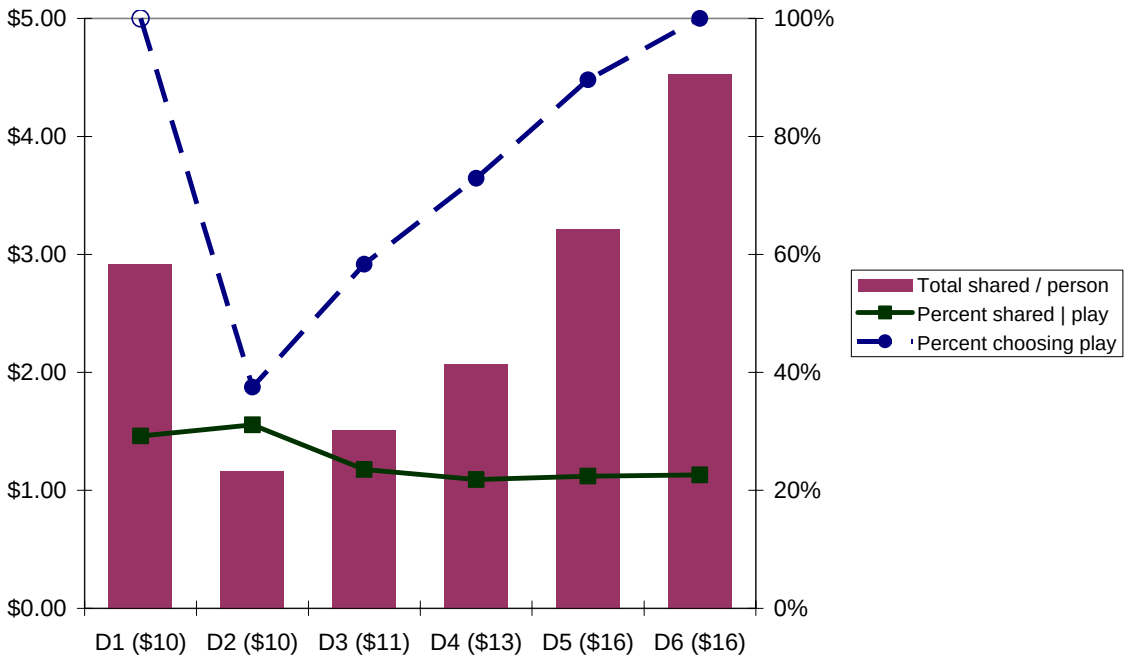


Figure 2. Anonimity (6 sessions; N = 46)

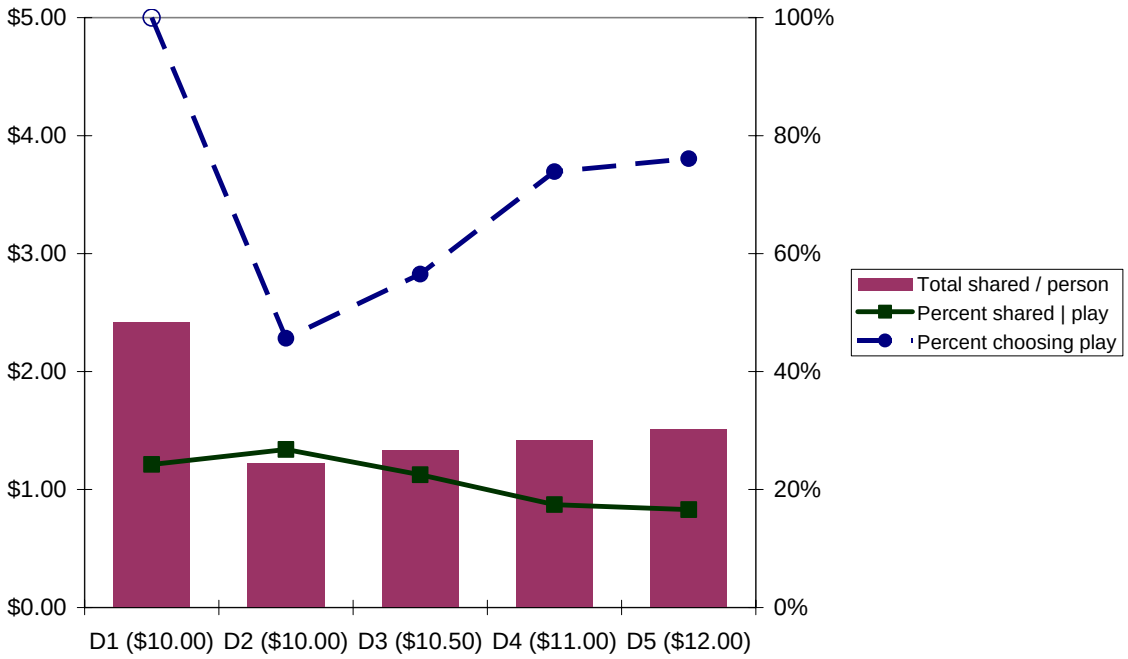
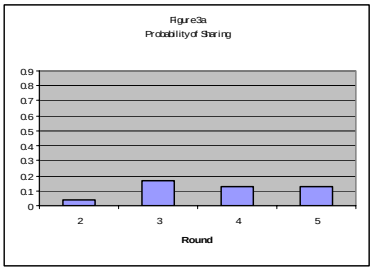
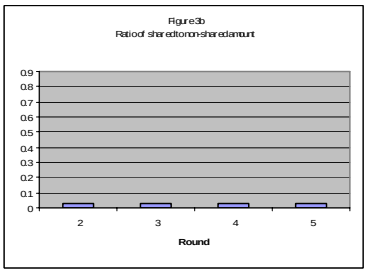


Figure 3. Sharing of Initial Non-Sharers





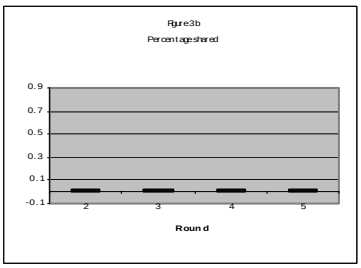
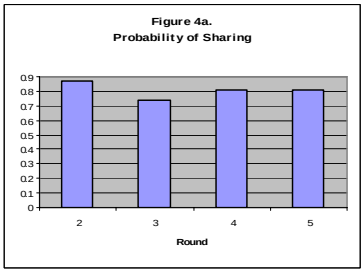
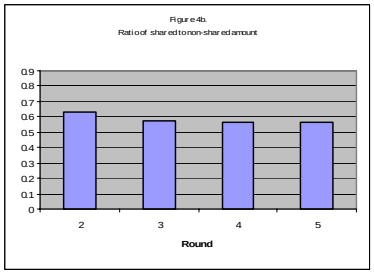


Figure 4. Sharing of Initial and Subsequent Sharers





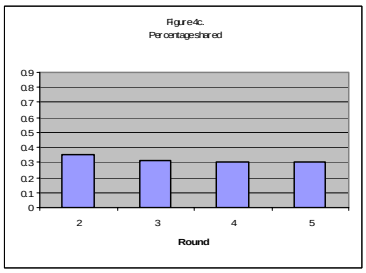
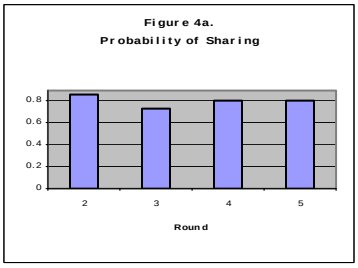
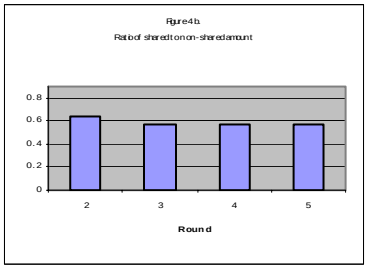


Figure 5. Sharing of Initial Sharers Who Opt Out





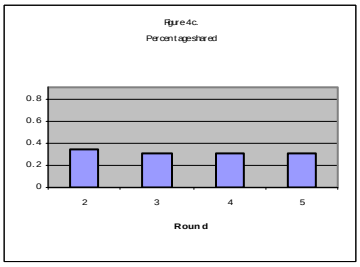
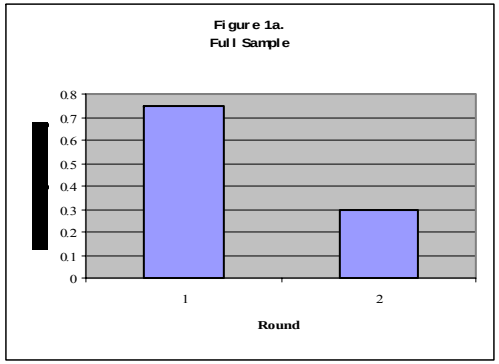
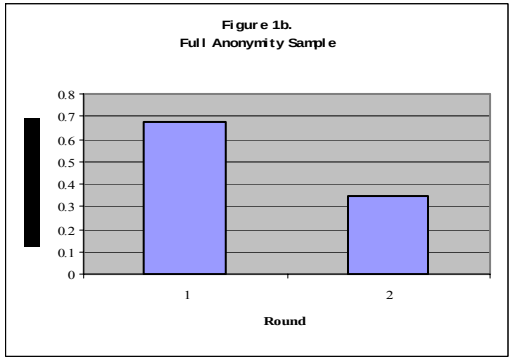


Figure 6a. Frequency of Sharing in Round 1 and Round 2





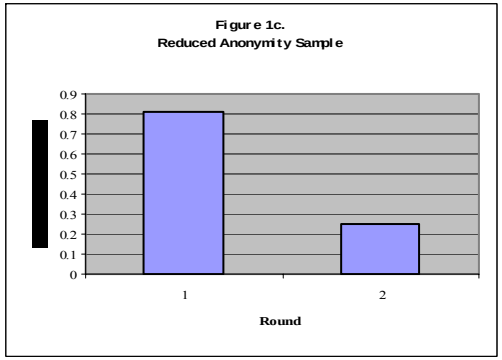
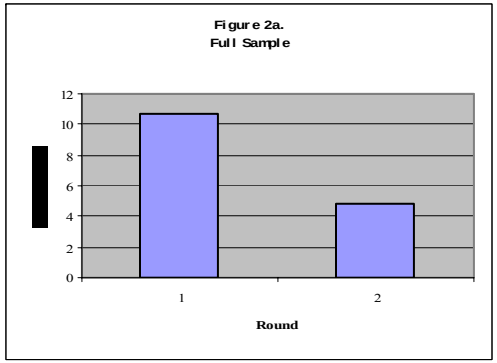
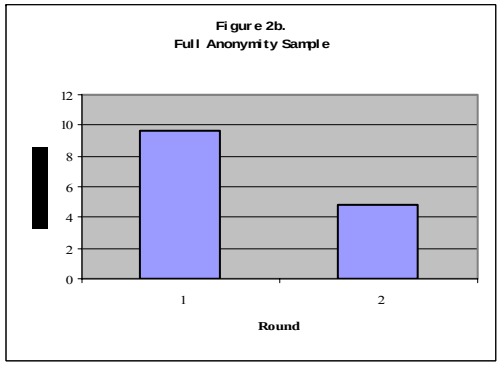


Figure 6b. Amount of Sharing in Round 1 and Round 2





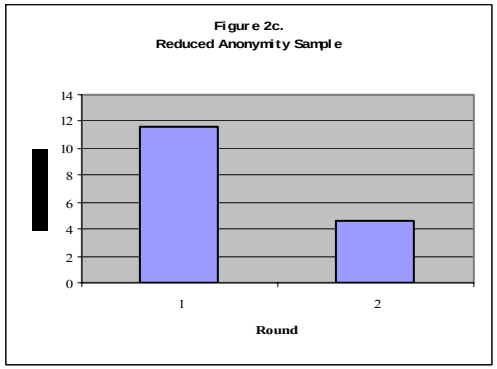
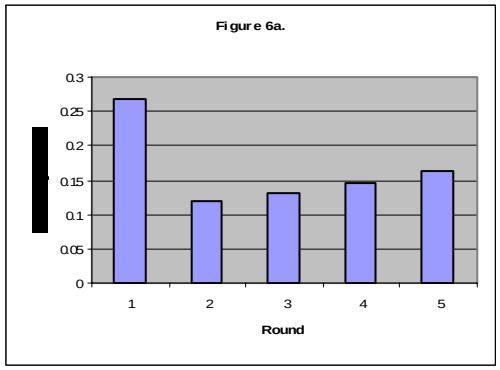
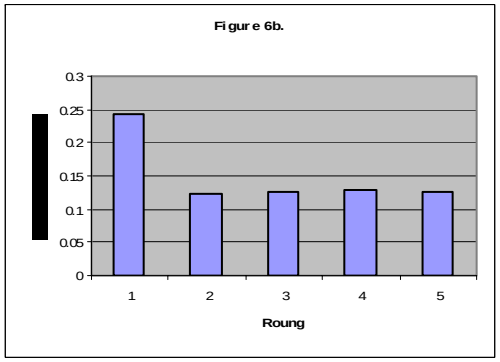


Figure 7. Total Sharing over all Rounds





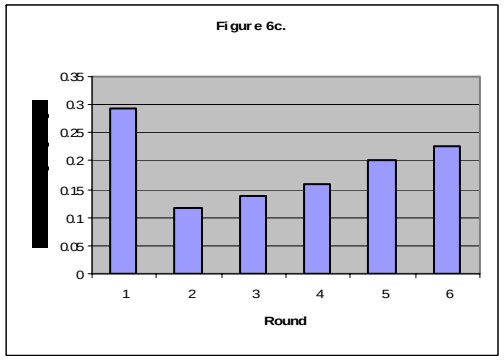


Table 1. Amount allocated in dictator game by decision and treatment

	Dictator allocation (Anonymity)	Dictator allocation (No Anonymity)	Sorting option ($10)
Decision 1	$10.00 (40 tokens)	$10.00 (40 tokens)	No
Decision 2	$10.00 (40 tokens)	$10.00 (40 tokens)	Yes
Decision 3	$10.50 (42 tokens)	$11.00 (44 tokens)	Yes
Decision 4	$11.00 (44 tokens)	$13.00 (52 tokens)	Yes
Decision 5	$12.00 (48 tokens)	$16.00 (64 tokens)	Yes
Decision 6		$20.00 (80 tokens)	Yes
Number of sessions	6	6	
Number of dictators	46	48	

Table 2. Sorting Decision in Round 2

Play	Frequency	Percent	Initial Non-Sharers	Initial Sharers
0	55	58.51	16	39
1	39	41.49	8	31
Total	94	100		

Table 3. Amount Shared in Rounds 1 and 2

The dependent variable is the amount shared. Choice is a binary variable and equal to 1 in Round 2. Anonym is a binary variable and equal to 1 in the Anonymity treatment. We allow for arbitrary within-person correlation in Column 4.

	(1)	(2)	(3)	(4)
Choice	-5.936	-5.936	-5.936	-5.936
	(1.175)	(1.177)	(1.179)	(0.801)
Anonymity	-0.884		-0.874	-0.874
	(1.176)		(1.183)	(1.482)
Female		-0.192	-0.115	-0.115
		(1.181)	(1.187)	(1.501)
Constants	11.145	10.817	11.203	11.203
	(1.011)	(1.051)	(1.175)	(1.327)
Observations	188	188	188	188
R^2	0.12	0.12	0.12	0.12

Table 4. Relation between Initial Sharing and Sorting Over Time for Initial Sharers that Opt Out in Round 2

	(1)	(2)	(3)	(4)
Initial Share	-0.082	-0.095	-0.112	-0.112
	(0.029)	(0.032)	(0.035)	(0.035)
Amount		0.102	0.156	0.155
		(0.023)	(0.316)	(0.032)
Anonym			1.840	1.861
			(0.529)	(0.537)
Female				-0.112
				(0.459)
Constant	1.528	3.631	6.877	-6.779
	(0.421)	(1.157)	(1.669)	(1.711)
Observations	141	141	141	141
R^2	0.05	0.22	0.29	0.29

Appendix-Table 1. Actions of (Potential) Dictators in No-Anonymity treatment
Sample divided by behavior in first decision.

	Decis. 1 ($10)	*Decis. 2 ($10)*	*Decis. 3 ($11)*	*Decis. 4 ($13)*	*Decis. 5 ($16)*	*Decis. 6 ($20)*
All potential dictators (N=48)						
Pass		30 (63%)	20 (42%)	13 (27%)	5 (10%)	0 (0%)
Share 0%	9 (19%)	6 (13%)	10 (21%)	11 (23%)	13 (27%)	14 (29%)
Share (0%,50%)	22 (46%)	3 (6%)	10 (21%)	17 (35%)	21 (44%)	21 (44%)
Share ≥ 50%	17 (35%)	9 (19%)	8 (17%)	7 (15%)	9 (19%)	13 (27%)
Total shared / subj	$2.92	$1.17	$1.51	$2.07	$3.21	$4.53
Potential dictators who initially shared 0% (N=9)						
Pass	9 (100%)	6 (67%)	2 (22%)	0 (0%)	0 (0%)	0 (0%)
Share 0%	0 (0%)	3 (67%)	6 (67%)	9 (100%)	8 (89%)	8 (89%)
Share (0%,50%)	0 (0%)	0 (0%)	1 (11%)	0 (0%)	1 (11%)	1 (11%)
Share ≥ 50%	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Total shared / subj	$0.00	$0.00	$0.06	$0.00	$0.11	$0.17
Potential dictators who initially shared (0%,50%) (N=22)						
Pass	0	17 (77%)	12 (55%)	8 (36%)	4 (18%)	0 (0%)
Share 0%	0	2 (9%)	3 (14%)	1 (5%)	3 (14%)	3 (14%)
Share (0%,50%)	22 (100%)	2 (9%)	7 (32%)	13 (59%)	15 (68%)	17 (77%)
Share ≥ 50%	0	1 (5%)	0 (0%)	0 (0%)	0 (0%)	2 (9%)
Total shared / subj	$2.51	$0.55	$0.86	$1.63	$2.38	$3.94
Potential dictators who initially shared ≥ 50% (N=17)						
Pass		7 (41%)	6 (35%)	5 (29%)	1 (6%)	0 (0%)
Share 0%	0 (0%)	1 (6%)	1 (6%)	1 (6%)	2 (12%)	3 (18%)
Share (0%,50%)	0 (0%)	1 (6%)	2 (12%)	4 (24%)	5 (29%)	3 (18%)
Share ≥ 50%	17 (100%)	8 (47%)	8 (47%)	7 (41%)	9 (53%)	11 (65%)
Total shared / subj	$5.00	$2.59	$3.12	$3.74	$5.94	$7.59

Appendix-Table 2. Actions of (Potential) Dictators in Anonymity Treatment
Sample divided by behavior in first decision.

	Decis. 1 ($10)	**Decis. 2 ($10)**	**Decis. 3 ($10.50)**	**Decis. 4 ($11.00)**	**Decis. 5 ($12.00)**
All potential dictators (N=46)					
Pass		25 (54%)	20 (43%)	12 (26%)	11 (24%)
Share 0%	15 (33%)	5 (11%)	6 (13%)	11 (24%)	14 (30%)
Share (0%,50%)	16 (35%)	12 (26%)	14 (30%)	16 (35%)	16 (35%)
Share ≥ 50%	15 (33%)	4 (9%)	6 (13%)	7 (15%)	5 (11%)
Total shared / subj	$2.42	$1.22	$1.34	$1.42	$1.52
Potential dictators who initially shared 0% (N=15)					
Pass		10 (67%)	7 (47%)	4 (27%)	4 (27%)
Share 0%	15 (100%)	4 (27%)	5 (33%)	8 (53%)	9 (60%)
Share (0%,50%)	0 (0%)	1 (7%)	3 (20%)	3 (20%)	2 (13%)
Share ≥ 50%	0 (0%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Total shared / subj	$0.00	$0.27	$0.33	$0.40	$0.40
Potential dictators who initially shared (0%,50%) (N=16)					
Pass		8 (50%)	6 (38%)	3 (19%)	4 (25%)
Share 0%	0 (0%)	1 (6%)	1 (6%)	2 (13%)	2 (13%)
Share (0%,50%)	16 (100%)	7 (44%)	9 (56%)	11 (69%)	9 (56%)
Share ≥ 50%	0 (0%)	0 (0%)	0 (0%)	0 (0%)	1 (6%)
Total shared / subj	$2.25	$1.20	$1.09	$0.92	$1.52
Potential dictators who initially shared ≥ 50% (N=15)					
Pass		7 (47%)	7 (47%)	5 (33%)	3 (20%)
Share 0%	0 (0%)	0 (0%)	0 (0%)	1 (7%)	3 (20%)
Share (0%,50%)	0 (0%)	4 (27%)	2 (13%)	2 (13%)	5 (33%)
Share ≥ 50%	15 (100%)	4 (27%)	6 (40%)	7 (47%)	4 (27%)
Total shared / subj	$5.03	$2.20	$2.60	$2.97	$2.63