6-10-2009

Sorting and Social Preferences

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Published In
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June 10, 2009

Abstract

Laboratory experiments on social preferences typically do not allow for sorting, while many field settings do. We demonstrate a strong effect of sorting when introduced in the laboratory. Across four experiments, most individuals who share with others do so reluctantly, preferring to avoid the opportunity to share. The attractiveness of the sorting option strongly affects the composition of self-selected participants. Subsidized entry into the sharing environment attracts those who share the least. Costly entry attracts few participants, but those share large amounts. Finally, we find that a similar proportion of sharers opt out even after inducing positive reciprocity.

* We thank Jordi Brandts, Colin Camerer, Stefano DellaVigna, Hank Farber, Ernst Fehr, Glenn Harrison, Larry Katz, Botond Köszegi, Klaus Schmidt, Georg Weizsäcker, as well as participants at several seminars and conferences for helpful comments. We also thank Jason Dana, Scott Rick, Aniol Llorente and David Rodriguez for help in conducting the experiments and the Pittsburgh Experimental Economics Laboratory (PEEL), the Laboratori d'Economia Experimental (LEEX), and the Berkeley Experimental Social Science Laboratory (XLab) for access to their resources.

I. Introduction

Experiments are an important part of every science, including economics. The controlled laboratory environment provides insights that cannot be gained easily in the field. It allows scientists to answer the "what ifs" that are hard to address in the complex and dynamic world outside the laboratory. For example, a large literature on dictator games demonstrates considerable sharing by individuals who unilaterally allocate wealth between to another recipient. Laboratory experiments have been critical in allowing researchers to cleanly study factors such as anonymity and social distance (Hoffman et al 1994; Bohnet and Frey 1999; see Camerer 2003 for a review).

Such control comes at a cost. Experiments simplify many features of naturally-occurring environments. One feature is the opportunity to sort. Most experiments select a random sample of subjects and lock them into a specific experimental environment. For example, in the typical dictator game, subjects are recruited with little information about the experiment and randomly assigned to play the role of dictator or receiver. The exogenous assignment allows the researcher to obtain a representative sample and to evaluate how the willingness to share is distributed across the population. Non-laboratory environments, however, operate differently. In the field, individuals sort into and out of environments based on their preferences, beliefs, or skills. Thus, individuals who participate in a market are unlikely to be a random sample of the population.[1]

In this paper, we explore the importance of sorting in the context of social preferences. We show that sorting strongly affects the sample composition of social-preference types and, as a result, sharing outcomes. Similarly to Fehr and Schmidt (1999), we show that the heterogeneity

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[1] Critics have questioned the sample selection among college students and, hence, the applicability of experimental results to "real people" performing "real tasks" (cf. Harrison and List, 2004). Many such criticisms have been successfully addressed, for example by replicating experiments with higher stakes (Hoffman, McCabe and Smith, 1996; Cameron, 1999; Camerer and Hogarth, 1999; Fehr, Fischbacher and Tougareva, 2002) or with professionals (see the overview in Harrison and List (2004), Section 4). The point of our paper is different. Rather than arguing that the samples are too narrow to reflect the overall population, we ask whether their selection is too broad to make inferences about the field. In addition, we demonstrate the potential of experiments to analyze sorting directly.

of social preferences interacts in important ways with the economic environment. However, while Fehr and Schmidt (1999) focus on the influence of one social-preference type on the behavior of other players – e.g. a selfish player inducing inequity-averse players to act selfishly – we show that sorting changes the sample composition, as a result, average sharing behavior. In a sequence of four experiments, we illustrate that the effect of sorting on sharing behavior is large and that sorting interacts with individual preferences to produce samples that look very different from an unbiased sampling of the population. For example, in the field, environments with the greatest opportunities for charitable or kind behavior – such as areas affected by disaster or famine – attract very different people than one would find in the population as a whole.

We begin by distinguishing three types of social preferences, based on whether an individual shares in the sharing environment (dictator game) and whether that individual sorts into the environment when it is costless to do so. "Willing sharers" share a positive amount and seek out the opportunity to do so. "Reluctant sharers" share if in the sharing environment, but prefer to avoid the environment altogether. "Non-sharers" simply never share. We generate predictions about how the three types are differentially attracted into environments as the price or cost for entry varies, and how this influences the resulting sample composition.

We then present four experiments that use variants of the dictator game, including a double-dictator game. In each experiment, we measure how much of an endowment (w) participants voluntarily share with another subject. We allow participants to sort out of the game, thus receiving a fixed payment (w') and leaving the potential recipient uninformed about the game. This design mimics situations in which a potential giver chooses whether to enter an environment in which sharing is possible, and the potential recipient becomes aware of the possible interaction only if entry occurs. We manipulate the cost of entering the sharing environment by varying the endowment in the dictator game (w) relative to the outside option (w').

In Experiments 1 (between-subjects) and 2 (within-subjects), we compare behavior under baseline (no sorting) and sorting treatments where entry is costless ($w = w'$). We find that aggregate sharing is considerably lower under sorting. We also find that the relative frequencies of the two kinds of sharers are fairly consistent across three geographic locations (Pittsburgh, Barcelona, Berkeley) and within each demographic subsample in our data. In all cases, a majority of sharing in the baseline dictator game appears to result from those who share reluctantly, i.e., sort out when it is possible. Thus, the presence of a sorting option greatly changes the amount of overall sharing relative to the standard dictator game without sorting.

Experiment 2 also introduces a cost of sorting by making the dictator game financially more attractive relative to the outside option ($w > w'$). The subsidy leads to greater entry into the sharing environment and more sharing on aggregate, but it disproportionately attracts those who share the least. As a result, average sharing among those who enter *decreases* for low subsidy levels, relative to no subsidy. Thus, subsidies intended to induce individuals to share may have the perverse effect of foremost attracting those who share the least.

Conversely, Experiment 3 makes entry into the sharing environment costly relative to the outside option ($w < w'$). Here, we find that those who pay the cost to enter the sharing environment are those who share the most. While aggregate sharing is low, since few subjects enter, average sharing among those who enter is substantially higher than in the standard dictator game. Thus, a cost for entering a sharing environment may attract primarily those who share the most.

Finally, in Experiment 4 we explore whether a strengthened motivation for sharing help to overcome the sorting effect. Several researchers have noted that reciprocity is a strong motive for generous behavior relative to other motives such as altruism (Fehr and Gächter 2000; Cox, Friedman and Gjerstad 2007). Is it strong enough to prevent individuals from opting out? For example, if small gifts such as address stickers increase the willingness of donors to give to a charity, does this increase in giving persist if the donor has the opportunity to avoid the request for giving? To test this question, we employ a double-dictator game, in which the (ultimate) recipient first decides about sharing $2 with the (ultimate) dictator.[2] We find that positive reciprocity, induced by receiving $1 out of $2 from the recipient, increases sharing, even when sorting is possible. That is, comparing double- and single-dictator games, the increase in giving after a small gift is robust to sorting. However, comparing double-dictator games with and without sorting, we also continue to find a decrease due to sorting, very similar in size to the single-dictator game (Experiment 1). Thus, positive reciprocity does not eliminate reluctance to share. Finally, negative reciprocity (induced by receiving $0 out of $2) virtually eliminates giving in the setting with sorting and is, in fact, strong enough to induce people *not* to sort out and to then share zero.

Overall, our paper makes two main contributions to the literature on social preferences. First, we show that sorting significantly reduces sharing, and that this result is robust across several experimental treatments, populations, and incentives. Even the inducement of reciprocity does not diminish the effect of sorting. One conclusion from our findings is that, in generalizing from experiments on social preferences to the field, one should account for the possible effects of individuals sorting between environments that allow different kinds of social acts.[3]

Our second main contribution is to relate sharing outcomes to the endogenous sample composition when sorting is possible. We show that the effects of sorting do not act uniformly across individuals. The deviation of the self-selected sample from the overall population depends systematically on the relative attractiveness of sharing environment and the outside option. Vary-

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[2] Differently from Ben-Nair et al. (2004), we use a mini-dictator game (over $2) in the first stage in order to distinguish reciprocity from distributional preferences (inequity aversion).

[3] Related studies have addressed the role of sorting in other contexts such as prisoner's dilemma and public goods games (Bohnet and Kübler 2005; Ahn, Isaac and Salmon 2008), the choice of reward and punishment (Sutter, Kocher, and Haigner 2006; Botelho, Harrison, Pinto, and Rutström, 2005), incentive contracts (Eriksson and Villeval, 2004; Dohmen and Falk, 2006), auctions (Palfrey and Pevnitskaya, 2008), risky choices (Harrison, Lau and Rutström, forthcoming), partner selection in trust and dictator games (Slonim and Garbarino 2008), and endogenous entry in market games (Camerer and Lovallo, 1999).

ing the prices of sorting we can predict who will enter and who will not enter a sharing environment. A large set of observables, such as gender, race, or donations to charity, instead, fail to predict sharing and sorting.

In addition, sorting helps to identify social preferences. An individual may appear to be a willing sharer when looking at an environment where opting out is difficult, but turn out to avoid sharing if possible. While our paper does not aim at and cannot pin down the exact preferences underlying "reluctant sharing," such as self-signaling, shame, or guilt,[4] it reveals that looking at behavior across environments with and without sorting helps to identify individual motives.

Our first finding and the design of our first experiment closely relates to the results of Dana, Cain, and Dawes (2006) who demonstrate that roughly one third of individuals prefer to receive \$9 instead of playing a dictator game over \$10 with an anonymous recipient. Similarly, Broberg, Ellingsen & Johannesson (2007) elicit reservation prices for exiting the dictator game and find that roughly two-thirds of subjects are willing to accept less than 100 percent of the dictator endowment in order to opt out. However, while Dana et al. and Broberg et al. are mainly interested in the motivation for giving and the question of whether some subjects are willing to pay to avoid the dictator game, we investigate the effect of sorting, using a variety of settings and ask which social-preference types take advantage of the sorting option. Thus, while previous experimental evidence demonstrates that at least some sharing is "reluctant" or "involuntary,"[5] we go further in demonstrating how sorting–and the price or cost of sorting–interact with such preferences to yield sharing outcomes that look different than the standard dictator game result.[6]

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[4] This is the focus of Dana et al. (2006) and (2007), Benabou and Tirole (2006), Tadelis (2008), Battigalli and Dufwenberg (2007), among others.

[5] Our results also relate to the findings of Dana, Weber and Kuang (2007) that many people are willing to forego costless information about the recipient's payoffs in a binary dictator game (see also Feiler 2007; Grossman 2009).

[6] Broberg et al.'s (2007) data allows exploring some of the hypotheses we consider here, which they do not do in their paper. (They only report a statistically insignificant correlation between reservation prices and sharing, using their entire sample.) In Online Appendix 2, we examine their data in light of our specific predictions.

II. Predictions: Sorting and sharing under heterogeneous social preferences

Consider an agent who is endowed with an amount *w*. In a sharing environment such as the classic dictator game, the agent can divide *w* into a payoff for herself (*x*) and a payoff for another agent (*y*). In an environment with no sharing opportunity, a possibly different amount *w'* is exogenously allocated to the agent, and the other agent receives nothing (*y=0*). We allow the agent's utility to depend on the payoffs *x* and *y* as well as on the environment, *D*, *U* = *U*(*D*, *x*, *y*), where *D* equals 1 if the environment allows sharing and 0 otherwise.[7] In this framework, a sorting option means that the agent can choose between an environment with and without a sharing opportunity. Note that an agent who chooses to be in an environment obtains the same utility as an agent exogenously assigned to such an environment, holding payoffs constant.[8]

We distinguish between three types of social preferences, based on the observed behavior in environments with and without sorting. First, some individuals share a positive amount if in the sharing environment, $\arg\max_{x\in[0,w]} U(1,x,w-x) < w$, and they prefer to be in such an environment when *w* = *w'*, i.e., $\max_{x\in[0,w]} U(1,x,w-x) > U(0,w,0)$. This type, which we term "willing sharer," derives utility from sharing and enters (and shares in) a dictator game whenever the cost is less than or equal to zero. Such social preferences capture a range of sharing motives, including pure and impure altruism and inequity aversion (Andreoni 1990; Fehr and Schmidt 1999).

The second type shares a positive amount, $\arg\max_{x\in[0,w]} U(1,x,w-x) < w$, but prefers not to have the option to share when there is no monetary reward: $\max_{x\in[0,w]} U(1,x,w-x) < U(0,w,0)$. We refer to this type as "reluctant sharer." This type may share out of shame or guilt at not shar-

[7] By including only "own payoff" and "others' payoff", we implicitly assume narrow framing. That is, the agent does not consider payoffs or wealth beyond payoffs from the current decision.
[8] Alternatively, agents may obtain lower utility when choosing to avoid the sharing opportunity than when being exogenously assigned to an environment without sorting. For example, they may incur disutility from (self-)-signaling that they prefer not to face the request to share. The existence of such an effect would lead us to *under*estimate the extent of reluctant sharing.

ing or due to (self-)signaling reasons. Another possible reason is "emotional altruism:" a reluctant sharer does not want to hurt the other person's feelings by saying no to a sharing request, but if the other person does not even know about the sharing opportunity, the reluctant sharer happily keeps the full amount for herself. We do not distinguish between these explanations. Our goal is simply to detect the reluctance to share, using the sorting option, and to assess the magnitude and its responsiveness to the price of sorting. Note that reluctant sharers not only prefer to opt out whenever the cost is zero ($w = w'$), but may even be willing to incur a positive cost ($w < w'$).

We will emphasize the simplified case that reluctant sharers derive utility solely from their own payoff. If given the choice between an endowment of w with sharing and w' without sharing, a reluctant sharer chooses the sharing environment if the portion she shares (a) is small enough such that $(1-a)w > w'$. This implies that the subsidy that a reluctant sharer requires to enter the sharing environment ($w - w'$) increases as the fraction she shares a increases.[9]

The third types does not share, even if the environment allows for sharing: $\arg\max_{x\in[0,w]} U(1,x,w-x) = w$. We will call this type "non-sharer." Non-sharers could be standard economic agents, who derive utility from their own payoff and are not affected by the presence of a sharing opportunity, in which case $\max_{x\in[0,w]} U(1,x,w-x) = U(1,w,0) = U(0,w,0)$. Since standard economic agents are indifferent between environments with and without sorting, holding the endowment constant, we have no prediction about their sorting behavior when $w = w'$. We predict that they choose the sharing environment (and share nothing) when it yields a greater payoffs ($w > w'$). [10]

Based on the above three kinds of individuals, we can generate simple predictions regard-

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[9] In Online Appendix 1, we show that also under a less restrictive model of preferences (i.e., a Cobb-Douglas model that allows for utility from sharing also for reluctant sharers), those reluctant sharers who are willing to share less are attracted at lower subsidies than those who share more.

[10] Alternatively, even non-sharers might prefer not to be asked to share, $U(1,w,0) < U(0,w,0)$`. Such non-sharers, which might be dubbed "reluctant non-sharer," sort out if it is possible to do so at zero cost. For simplicity, we will neglect the latter type. As with the simplification discussed in fn.8, this neglect induces an *under*estimation of reluctant (non-)sharers and makes our estimates of the relevance of sorting more conservative.

ing sharing and sorting behavior. The first prediction deals with the impact of reluctant sharers in environments where opting out is possible at no cost:

Prediction 1: *The introduction of a sorting option in sharing environment with equal endowment (w = w') reduces aggregate sharing.*

The reduction in sharing when a costless sorting option is introduced (*w* = *w'*) identifies exactly the fraction of reluctant sharers as defined above.

The next two predictions deal with changes in the composition of self-selected individuals in the sharing environment as the cost to entering changes.

Prediction 2: *Subsidizing the sharing environment relative to the outside option (w > w') increases entry. Entry among those who opt out if sharing is not subsidized (w = w') increases with the size of the subsidy, but is decreasing in the portion agents share (a).*

Subsidizing entry into the sharing environment has no effect on the sorting decisions of willing sharers. They enter the game and share even when there is no subsidy. A positive subsidy attracts, however, non-sharers and reluctant sharers. The former are indifferent between entering and not entering when it is costless (no subsidy); but under positive subsidies, they enter and share nothing. So the first agents to respond to the subsidy are those who share nothing. Reluctant sharers also need to be compensated for entering. However, those who are first attracted by relatively low subsidy are those who share the least. This follows from the above assumption that reluctant sharers care only about their monetary payoff, but also holds under a less restrictive model of preferences (see Online Appendix 1).

As an illustration of Prediction 2, consider the famous "beggar on the street" example. Suppose individual A is a non-sharer, who is indifferent between encountering the beggar (and giving zero) and crossing the street if it is costless to do so. Individuals B and C are reluctant sharers who give money – B a small amount, C a large amount – but prefer to cross the street to avoid the encounter if it is costless. If there is a small cost to cross the street, such as an extra five seconds of walking, the non-sharer A will be unwilling to incur such a cost and choose to encounter the beggar and share nothing. Individuals B and C may still cross the street to avoid the beggar. Thus, a very low avoidance cost (or entry subsidy) only induces the non-sharer to enter. If there is a larger cost to crossing, such as an extra two minutes of waiting for a light, B is likely to stop crossing. The one most likely to wait for the light is individual C, who has the most to gain from avoiding the encounter. Thus, as the cost to avoiding the beggar increases, those reluctant sharers who share the least will be the first to re-enter the sharing environment.

Prediction 3 considers the opposite pricing differential. If sorting at no cost removes reluctant sharers and subsidizing entry attracts the most ungenerous reluctant sharers, what happens if there is a cost to entry?

Prediction 3: *Making the sharing environment costly relative to the outside option (w < w') decreases entry, but sharing conditional on entry is higher than in the environment without sorting.*

Costly sharing induces both the non-sharers and the reluctant sharers to sort out. Only a subset of willing sharers remains, namely those who care the most about the payoff of the other person. Hence, we expect the conditional average amount shared to be high. This prediction relies on (1) a high enough fraction of non-sharers dropping and (2) the subset of willing sharers who enter at a given cost *w' – w* to share more than reluctant sharers. A sufficient condition would be, for example, that the distribution of amount shared by willing sharers first order stochastically dominated the distribution for reluctant sharers, consistent with the idea that reluctant sharers want to appear as willing sharers, but find it costly to do so (cf. Benabou and Tirole, 2006).

Finally, we explore the effects of sorting when subjects have stronger motives for sharing. If an initial kind act yields greater sharing, consistent with previous research on reciprocity, will the introduction of sorting still affect reciprocal sharing? Given that reciprocity is often perceived to be a stronger motivation to share, we expect a higher fraction of those who have been treated kindly to enter the sharing environment than in a standard dictator game with sorting.

Prediction 4: *The proportion of sharers who enter the sharing environment is higher in a context involving positive reciprocity, than in sharing contexts without reciprocity.*

In other words, we expect to observe a higher fraction of willing sharers after inducing positive reciprocity. There are two possible channels: Receiving a gift may turn reluctant sharers into willing sharers. In that case, sorting becomes unimportant; all sharing is done willingly. Alternatively, receiving a gift turns non-sharers into willing sharers. In that case, the fraction of reluctant sharers who sort out would remain similar to that in a simple dictator game with sorting.

We now have a series of predictions that can be borne out or refuted by experimental evidence. The next several sections present experiments testing these predictions.

III. Experiment 1 – Costless Sorting

Our first experiment uses a between-subjects design to compare outcomes in treatments without and with sorting. In the treatment with sorting, subjects who opt out receive an amount equal to the full endowment in the dictator game ($w = w'$).

A. Experimental Design

Experiment 1 was conducted in two locations: Barcelona and Berkeley. Subjects were graduate and undergraduate students at the Universitat Pompeu Fabra (UPF), the Universitat

Autònoma de Barcelona (UAB),[12] and at UC Berkeley. We conducted 16 sessions – eight in each city. In total, 336 subjects participated (154 in Barcelona and 182 in Berkeley); 166 subjects (83 dictators) in the No-Sorting treatment and 170 subjects (85 dictators) in the Sorting treatment. Each session consisted of an even number of 10 to 36 participants and lasted 20 to 25 minutes.

Upon arriving at the experiment, subjects were told that they would receive a participation fee (€5 in Barcelona[13] and $5 in Berkeley) and that they might earn additional money. Subjects randomly drew participant numbers, which determined in which of two rooms they had to go. One half of the subjects (the recipients) were asked to complete a brief questionnaire, for which they would not receive any additional payment, and to then wait quietly. The other half of the subjects, the dictators, who were located in a separate room, received instructions, both in writing and aloud. These instructions varied by treatment.

No-Sorting Treatment. In dictator games without sorting, the dictators were told that they would divide €10 (Barcelona) or $10 (Berkeley) between themselves and a randomly and anonymously matched subject in the other room. At the end of the experiment, the experimenter would describe the game to the participants in the other room, and then show each of them how much money they received. Participants would then be paid, which concluded the experiment.

Dictators received an envelope with a sheet inside indicating the number of the paired recipient.[14] On the sheet, each dictator wrote his or her own participant number and indicated a division of the endowment (in ten-cent increments). The experimenter then collected the envelopes and asked the dictators to complete the same one-page questionnaire as the recipients.

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[12] Instructions and materials for both experiments are in Online Appendix 3. Instructions for the remaining experiments are similar, except for the specified treatment differences. The Barcelona sessions were conducted in Spanish (Castilian); the instructions are translated into English. The entire dataset is available from the authors.
[13] At the time the sessions were conducted €1 was worth between $1.27 and $1.29.
[14] Random matching was implemented by shuffling the envelopes and allowing the dictators to select from the stack that remained.
[16] This procedure ensured that subjects participating and not participating wrote roughly the same amount on the sheets, thus preserving anonymity.

Sorting Treatment. In the dictator games with sorting, dictators received the same instructions, but – in addition – were told they could decide whether or not to "participate." If they chose to participate, they would share the endowment and the matched recipient would be informed of the game and the divided amount. If they chose not to participate, they would receive a payment ($10/€10) without having the option to distribute the money. In that case, the potential recipient would be paid the ($5/€5) participation fee, and told nothing about the dictator game.

In this treatment, dictators received *two* envelopes, one labeled "participate" ("participar") and another labeled "don't participate" ("no participar"). Subjects who chose to play the game opened the envelope marked "participate," saw the participant number of the paired recipient, recorded their own number, and specified a division of the ($10/€10) endowment. Subjects who chose not to play the game, opened the envelope marked "don't participate" (which did not contain an even-numbered participant number) and wrote only their participant number on the sheet inside.[16] After making either choice, subjects returned the envelopes to the experimenter. The experimenter then separated receivers matched with dictators who chose to play the game and those who did not. For the recipients matched with a non-participating dictator, the experiment was concluded. The recipients paired with a participating dictator received a description of the dictator game, saw the sheet informing them of how much they had been anonymously given.

The questionnaire, administered in both treatments, asked for detailed demographics (age, gender, race or ethnic group, education), including subjects' family background (number of siblings, language spoken at home, years of residence in Berkeley/Barcelona, social class). We also asked about social preferences (donations to charity during the past year) and risk preferences (like or dislike of risks). Finally, we elicited how many people "in the other area" a subject knew, i.e., how many receivers dictators knew and vice versa. In Berkeley, we added the question "Why did you decide to share (or not share) the amount you did in the experimental task today? If applicable, why did you decide not to participate?"

B. Results

We analyze the results of Experiment 1 to test Prediction 1: Does the introduction of a sorting option decrease aggregate sharing? A decrease in the percentage of subjects who share something would reveal the relative frequencies of reluctant and willing sharers.

Figures 1A and 1B show the distributions of amounts shared, and the frequencies of subjects who opt out of the sharing environment, for both Barcelona and Berkeley. In the No Sorting treatment, sharing was comparable to the results of previous dictator experiments. Dictators share, on average, €1.87 in Barcelona and $2.00 in Berkeley, and most subjects share a positive amount with the recipient (60 percent in Barcelona and 64 percent in Berkeley).

However, the introduction of a sorting option has a strong effect. Mean aggregate sharing decreases significantly for both locations, to €0.58 in Barcelona and to $1.21 in Berkeley. Both decreases are statistically significant in a non-parametric rank-sum test (Barcelona: $z = 3.39$, $p < 0.001$; Berkeley: $z = 2.34$, $p = 0.02$). As predicted, and consistent with previous results, many subjects choose to opt out (72 percent in Barcelona, 50 percent in Berkeley).

Given the above behavior, we can estimate the relative frequencies of the three postulated types. The proportion of non-sharers is the fraction of subjects who do not share in the No Sorting treatment, 35 percent of the overall sample (33 percent in Barcelona and 36 percent in Berkeley). Willing sharers are those who enter the sharing environment and share in the Sorting treatment, which is 32 percent of the overall sample (28 percent in Barcelona and 38 percent in Berkeley). Reluctant sharers make up the remainder, implicitly sharing in the No Sorting treatment, but not in the Sorting treatment. They comprise 31 percent of the total sample (39 percent in Barcelona and 25 percent in Berkeley).

Thus, consistent with earlier experimental results, when individuals are put in a sharing environment, the vast majority chooses to share a positive amount. However, when subjects are given the opportunity to opt out of the game, the picture reverses. Most subjects share nothing, primarily by opting out of the game, and the aggregate amount shared decreases significantly. Table 1 confirms the statistical significance of these two findings in a simple linear regression framework for the amount shared[17] and a probit estimation for the frequency of sharing: the sorting option significantly reduces sharing, and the effect is similar in Barcelona and Berkeley (insignificantly larger in Barcelona).

These findings provide evidence that, first, sorting matters in the context of social behavior and, second, a large fraction of sharing in an environment without sorting occurs not because the subjects *share willingly*, but because they *share reluctantly*. Most share when forced into a sharing situation, yet many choose to avoid the situation altogether when given the option to opportunity to sort among environments that do and do not allow sorting.

We gauge the importance and robustness of the observed sorting effect by relating it to other potential determinants of sharing. First, we ask whether any of the individual characteristics elicited in the questionnaire predict sharing and, if so, how their impact compares to the impact of sorting. In Table 2, we compare the effect of sorting on the amount shared (Column 1) to the effect of the demographics and self-reported preferences (Column 2). The table presents OLS regressions with robust standard errors. While the effect of sorting is highly significant and large (subjects share, on average, 10.1 percent less of the endowment when sorting is possible), none of the other dummy variables affect sharing to a similar extent. All coefficients are smaller in absolute size, ranging between 0.02 and 0.78, and all but "Sibling=1" are insignificant. (Subjects

[17] For the pooled regressions, we ignore the currency and use amounts shared "out of 10."

with 1 sibling share about €0.78 less than those with 2 siblings, significant at the $p < 0.05$ level.) The results are very similar when including both the sorting binary variable and the individual characteristics (Column 3). The estimated coefficient for sorting is virtually unchanged. Overall, the opportunity to sort appears to be significantly more important than any of the individual characteristics in determining sharing behavior.[18]

A second way to measure of the importance of sorting relative to observable individual characteristics is the portion of explained variance. In the regression with only Sorting as independent variable (Column 1), the adjusted R^2 is 0.06; in the regression with the 11 individual characteristics (Column 2), it is only 0.03. That is, the observable characteristics explain only half as much variance as sorting alone, once we account for the effect of merely adding predictors.[19] More directly, we calculate the partial coefficients of determination in the regression including both sorting and demographic dummies (shown to the right of the standard errors in Column 3). The partial R^2's reflect the strength of association of each independent variable.[20] Each individual characteristic explains between 0.01 and 0.19 of the remaining unexplained variance; sorting explains 0.27. Thus, sorting has not only a larger effect and a lower p-value, but is also a more reliable predictor of sharing in our data than any other variable.[21]

Overall, Experiment 1 provides evidence that the availability of sorting significantly low-

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[18] All findings are highly robust to alternative regression specifications such as refinements of the dummies for age, social class, or major (though a higher number of controls risks saturating the model).

[19] The adjusted R^2 is calculated as $1 - (1 - R^2)\cdot[(N - 1)/(N - K - 1)]$, where N is the number of observations and K the number of predictors.

[20] The partial R^2 for predictor i is calculated as $(R^2 - R^2_{(i)})/(1 - R^2_{(i)})$, where $R^2_{(i)}$ is the R^2 with predictor i removed from the equation.

[21] We also check the robustness of the sorting effect across different subgroups of subjects. For each demographic characteristic (subsamples by gender, ethnicity, socio-economic status, age, siblings), including educational choices (major, university), and for the elicited preferences (donations, risk preferences), we calculate the average amount shared in the treatment without sorting and the treatment with sorting. The results are displayed in Appendix Figure 1. In all but one of the 20 subgroups, the average amount shared without sorting (left bars) is lower than the average amount shared with sorting (right bars). Thus, our baseline result is not only robust to the inclusion of individual characteristics as controls; it is also pervasive throughout all categorizations by such characteristics.

ers sharing. The effect of sorting is large and robust: across different subject populations, many subjects opt out, resulting in significantly less sharing than in the baseline dictator game. The findings indicate that not all sharing is the result of people wanting to share. Instead, we find evidence of the three kinds of preferences postulated earlier, each making up roughly one third.

A different possible insight from sorting behavior relates to the amount shared *conditional* on not opting out: Are those who share willingly more generous than those who share reluctantly? The between-subject treatment makes it difficult to answer this question since we cannot track an individual across environments with and without sorting. We can only compare the amounts shared by those who choose to participate in the treatment with sorting to the amounts shared without sorting. Both in Barcelona and in Berkeley, the average amount shared by those who enter the sharing environment voluntarily is higher, €2.05 versus €1.87 in Barcelona and $2.41 versus $2.00 in Berkeley, but these differences are not statistically significant.

IV. Experiment 2: Costly Sorting out of Sharing Environments

In Experiment 2, we further explore the impact of sorting on the composition of a self-selected sample in two ways. First, we conduct a within-subjects experiment to validate the conditional behavior predicted for the three types. Observing individual behavior across dictator games with and without sorting (at a price of zero), we can classify each individual's type. Second, we test how the sorting behavior of the different types interacts with a cost of sorting. Specifically, to test Prediction 2, we introduce subsidies for entry into the sorting environment ($w > w'$). Since willing sharers enter (and share) whenever $w \geq w'$, the presence of a subsidy should not change their entry decisions. Non-sharers are indifferent between not entering and entering (and sharing zero) when $w = w'$, but should enter and share nothing whenever $w > w'$. The main focus of Experiment 2 is on the reluctant sharers. Since they view entering and sharing as costly, we predict that they will be more likely to enter as the subsidy increases. However, if their disutility from entry is proportional to the amount they share, low subsidies will primarily attract those reluctant sharers who share the least.

The experiment consists of three parts. The first two parts are within-subject replications of the two treatments of Experiment 1. In part 1, the dictator game endowment (*w*) is \$10 and there is no sorting. In part 2, dictators can sort out of the game, in which case they receive \$10 (*w'*) and the (potential) recipient never finds out about the game. In part 3, we increase the amount available in the dictator game while the sum for opting out remains \$10 ($w > w'$). We do not counter-balance the order of the three parts across sessions since the purpose of the within-subject design is to compare the rates of re-entry. Only subjects exposed to the same initial treatment are comparable. And, among different possible "initial treatments," the above order (starting with the standard dictator game) allows us to compare to standard dictator games.[22]

In addition, we explore the robustness of our findings to situations in which dictators are not anonymous but have to face the recipient– as when one encounters a beggar on the street.

A. Experimental Design

Experiment 2 took place at the Pittsburgh Experimental Economic Laboratory (PEEL) at the University of Pittsburgh. Subjects were graduate and undergraduate students at the University of Pittsburgh and Carnegie Mellon University. We conducted 12 sessions, 6 in each anonymity treatment. A total of 188 subjects participated, 92 (46 dictators) in the No-Anonymity treat-

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[22] Variation in the order of parts 1 and 2 turned out to be redundant since the outcomes in parts 1 and 2 closely replicate the results from the between-subjects experiment. It is, of course, possible that a different order, e.g., experiencing sorting prior to the baseline dictator game, influences sharing. While such an effect would be interesting, we need to eliminate it from the measurement of sharing in the standard dictator games for the purposes of comparison to other dictator experiments. Also note that we do not account, separately, for learning since several recent studies of repeated dictator games have found little change in behavior over time (Duffy and Kornienko, 2005; Hamman et al., 2009), differently from, for example, the case of dominance-solvable games (Weber, 2003; Weber and Rick, 2009).

ment and 96 (48 dictators) in the Anonymity treatment.[23] In all three parts, the procedures replicated those of Experiment 1, other than the changing dictator game endowment.[24] Dictators were informed that they would make a series of decisions, with new instructions distributed prior to each decision. At the end of the experiment, one decision would be randomly selected to count.

Decision 1. Decision 1 consisted of a dictator game with no sorting. The endowment was $10, denoted as 40 tokens. Subjects were told that *if Decision 1 were selected to count at the end of the experiment*, then the experimenter would describe the dictator game publicly the other participants and each recipient would find out how much money he or she had been given. In the No-Anonymity treatment the dictators themselves handed the sheets to the recipients.

Decision 2. In Decision 2, dictators had the opportunity to play exactly the same dictator game as in Decision 1, with a (potentially) new randomly selected participant. Alternatively, they could choose to “pass” (i.e., not to play the game). The procedure mirrored the Sorting treatment in Experiment 1. Dictators had to open one of two envelopes. If they opened the envelope labeled “Play,” they would see the number of their matched participant on the sheet inside, write down their own number, and indicate a division of 40 tokens. If they opened the other envelope, labeled “Pass,” they would not see a participant number, but would write down their own number and mark an “X” on the sheet inside.[26] The experimenter told dictators that if they chose to play the dictator game *and if Decision 2 were selected to count*, then their paired recipient would be brought back into the room and informed about the game and the allocation of tokens.

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[23] One subject was accidentally allowed to participate twice (both times as dictator). We omitted this subject’s second participation from the data. Since subjects’ choices were never revealed to anyone else until the end of the experiment, it is very unlikely that this subject influenced the choices of other dictators in the second session.
[24] Thus, relative to Experiment 1, participants outside the room were slightly worse off: they had to fill in a series of questionnaires rather than one brief questionnaire. This difference was due to the fact that Experiment 2 consisted of several decisions by dictators (vs. only one decision in Experiment 1) and thus took longer.
[26] As in Experiment 1, the procedure prevented dictators from inferring the choice of others, since both those playing and those passing had to open an envelope and write on the sheets.

Remaining Decisions. The remaining three decisions (four in the No-Anonymity case) proceeded exactly as Decision 2, with the exception that the dictator-game endowment increased. Table 3 presents the endowment for each decision.[27]

At the end of each session, the experimenter randomly drew one of the decisions to count.[28] Then either all recipients (if decision 1 was drawn) or only the recipients matched to dictators who decided to play (if decision 2 or higher was drawn) were informed about the game and shown the payoff sheet filled out by their matched dictator. In the No-Anonymity treatment, the dictators themselves handed the sheets to the recipients, revealing their identity. They were then paid their earnings and participation fee. The other (unmatched) participants remained outside, were thanked for their participation and paid $6.

B. Results

Table 3 presents, for each treatment and each decision, the total amount shared per subject and the number of subjects opting to play the game. The behavior in Decisions 1 and 2 strongly corroborates the findings of our between-subjects design in Experiment 1. When dictators are forced to play the game (Decision 1), 74 percent share. When subjects are given the opportunity to opt out of the game (Decision 2), only 30 percent share. As a result, the total amount shared decreases substantially, from an average of $2.68 without sorting (Decision 1) to $1.19 when sorting becomes possible (Decision 2).

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

[28] As in Experiment 1, subjects were told that if the numbers did not add up to the allocation, then the amount to the recipient would determine the allocation. Here, this occurred once.

The impact of sorting is robust to the removal of anonymity. In the standard dictator game (Decision 1), 81 percent share in the No-Anonymity treatment and 67 percent in the Anonymity treatment. They share average amounts of $2.42 (Anonymity) and $2.92 (No Anonymity). Thus, as expected, the lack of anonymity produces slightly more sharing. However, this difference is not statistically significant: the significance fails to reach 10 percent either using a t-test ($t_{92} = 1.17$) or a Kolmogorov-Smirnov test ($D_{46,48} = 0.19$). In the dictator game with sorting (Decision 2), only 25 percent share in the No-Anonymity treatment and 35 percent do so in the Anonymity treatment. The average amounts shared decrease in both treatments, to $1.22 in Anonymity and to $1.17 in No-Anonymity. Thus, the lack of anonymity makes opting out more attractive and reduces sharing slightly more. The difference in sharing is, however, again not significant ($t_{92} = 0.14$ in the two-sample t-test and $D_{46,48} = 0.10$ using the Kolmogorov-Smirnov test).[29] In an OLS regressions of the amount shared in Decisions 1 and 2 on a binary variable indicating whether sorting was available, another binary variable for anonymity, and their interaction (with and without session fixed-effects), the coefficient on sorting is significantly negative in both regressions ($p < 0.001$), while the coefficients for anonymity and for the interaction are always statistically insignificant. (The t-statistic on these coefficients is always less that 1.35.)

The results of first two decisions in Experiment 2 demonstrate the robustness of the sorting effect, both when we relax anonymity and when we conduct a within-subject test.

Classification of subject types

The within-subject design of Experiment 2 allows us to classify individual subjects into the three types posited earlier. Based on their first two decisions, 23 percent of our subjects are *non-sharers*—they share nothing in Decision 1 and either opt not to play or share nothing in De-

[29] Also the amounts by which sharing is reduced (by $1.20 in the Anonymity treatment and by $1.76 in the No-Anonymity treatment) are not significantly different.

cision 2; 29 percent are *willing sharers*, who share both in Decision 1 and in Decision 2. The largest group, 41 percent, is the *reluctant sharers*; they share in Decision 1 and opt out in Decision 2. These three categories account for 95 percent of the subjects.[30] The distributions of types do not differ significantly by anonymity treatment ($\chi^2(2) = 3.49$, $p = 0.18$).[31] Differentiating by gender, males are more likely to be *non-sharers* than women (M: 30%; F: 20%) and less likely to be *reluctant sharers* (M: 30%; F: 47%). However, the difference in distributions of types by gender is not statistically significant ($\chi^2(2) = 1.97$, $p = 0.37$). The proportions differ slightly from those observed in Experiment 1: we have fewer non-sharers and more reluctant sharers.

Given this classification, we can more precisely revisit the question of which type – willing sharers or reluctant sharers – behaves most generously in the dictator game. The average amount shared in Decision 1 by the willing sharers is \$4.46 (\$4.22 in Anonymity and \$4.77 in No Anonymity) and by the reluctant sharers is \$3.10 (\$3.20 in Anonymity and \$3.04 in No Anonymity). The difference between willing and reluctant sharers is significant at the $p < 0.001$ level ($t_{64} = 3.95$). Thus, those who share willingly, i.e., even when they can avoid the sharing environment altogether, are significantly more generous than those who share reluctantly.

Who Do Subsidies Attract into the Sharing Environment?

The second issue addressed by Experiment 2 is how subsidizing entry into the sharing environment influences the sorting of different types. Willing sharers, by definition, enter the sharing environment when there is no subsidy ($w = w'$) and should continue to enter if there is a

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[30] Of the remaining five subjects, three shared something in Decision 1 (\$0.25, \$2.50, \$5) and shared nothing in the remainder of the experiment (but frequently opted to play). We might classify these three subjects as *reluctant sharers*, though they did not rely on the sorting opportunity. Another subject shared \$2.50 initially, shared \$0.50 in Decision 4, and nothing otherwise (but opted to play every time). A final subject shared nothing initially, but then shared \$4 in all subsequent decisions – possibly a *willing sharer*, with trembles or noise in the first decision.

[31] Behavior in Decisions 3 and beyond does not differ between anonymity treatments either, when controlling for endowments. For example, comparing Decision 3 under No-Anonymity and Decision 4 under Anonymity, both with an endowment of \$11, neither the average amounts shared (\$1.51 and \$1.42) are significantly different ($t_{92} = 0.20$) nor the frequencies of entry ($z = 1.59$).

subsidy ($w > w'$). We find that, in Decisions 3 and up, when the sharing environment is subsidized, willing sharers enter the sharing environment 90 percent of the time.[32] Upon entering, they share significant amounts, at least $3.82 on average for every endowment level.

Non-sharers, by definition, share nothing in Decision 2 when $w = w'$, mostly by opting out (70 percent). We predicted that those non-sharers who opt out re-enter when the sharing environment is subsidized. In Decision 3 and beyond, non-sharers enter the sharing environment 78 percent of the time.[33] As expected, they share very little when they enter, never more than $0.17 on average for any dictator game endowment.

Our main interest is the reluctant sharers. Table 4 reports the marginal effects from probit estimations with subjects' decision to play (1) or to pass (0) as the dependent variable. Since all subjects had to play the game in Decision 1 and since the choice to play the game in Decision 2 is used to construct the types, we exclude these two decisions from the analysis. We include the endowment to be allocated as an explanatory variable. (The results in the table are substantively unchanged when we control for treatment, gender and decision.)

The first regression in Table 4 explores the entry decisions of the three different types (We exclude the five subjects who did not fit the classification scheme). The omitted category, willing sharers, enters at a significantly higher rate than non-sharers (15 percent more often) and reluctant sharers (35 percent more often). The difference between non-sharers and reluctant sharers is statistically significant ($\chi^2(1) = 13.89$, $p < 0.001$). As predicted reluctant sharers are the least willing to re-enter the sharing environment as entry becomes subsidized.

[32] As the percentage is below 100, our classification, while generally accurate, is imperfect. Two participants account for most of the exceptions. They shared positive amounts in Decisions 1 and 2 (and were thus classified as willing sharers), but opted out in every remaining Decision, perhaps discovering they were really reluctant sharers.

[33] Entry among-non sharers increased with the size of the endowment in the game. For example, 61 percent entered in Decision 3, but more than 83 percent did so for all subsequent decision. This reluctance to enter at low subsidies perhaps indicates that some non-sharers experienced disutility from sharing nothing in the dictator game – not enough to lead them to share in Decision 1, but enough induce opting out and foregoing a subsidy.

Prediction 2 stated that a high enough subsidy would lure reluctant sharers back into the game, but that for any given subsidy those most likely to enter would be those who share the least. Figures 2A and 2B show, by the amount shared in Decision 1, the frequency with which reluctant sharers re-enter the sharing environment as the subsidy increases across decisions. By definition, all reluctant sharers opt out in Decision 2. From Decision 3 on, as the subsidy increases, we see that reluctant sharers are attracted back into the dictator game. However, consistent with our prediction, those who re-enter first are those who shared the least in Decision 1. We split reluctant sharers into three groups, based on how much they shared in Decision 1. We see that, by Decision 4 in both treatments, those who shared $1.25 or less all re-enter the game, while those who shared more require greater subsidies to re-enter. In fact, for every decision, across both treatments, the highest entry frequency is for those who shared the least initially.

Returning to Table 4, Columns 2 through 5 test the statistical significance of the relationship between entry and initial amount shared. Columns 2 through 4 excludes non-sharers, who by definition did not vary in the initial amount shared. Taking willing sharers and reluctant sharers together, there is no relation between initial amount shared and entry (Column 2). Reluctant sharers, however, enter at lower frequency, and controlling for reluctance, there is a negative relationship between entry and initial amount shared (Column 3). The negative relationship is mainly driven by reluctant sharers who shared large amounts initially (Column 4). That is, the apparent lower entry rates in Column 3 are the result of low entry rates only by high-sharing reluctant sharers. As Column 5 shows more directly, there is indeed a negative relationship between initial amount shared and entry into the sharing environment when using only the sample of reluctant sharers. The coefficient on Initial Amount Shared, which is almost identical to the interaction coefficient in Column 4, indicates that for every $1 shared initially, reluctant sharers are 8.2 percent less likely to enter the sharing environment.

Overall, we find strong support for our prediction: entry by reluctant sharers for a given subsidy level is inversely proportional to the amount they share.

What kind of sharing environment will result from sorting and subsidies?

We have shown that subsidies foremost attract those least willing to share – non-sharers and reluctant sharers who share the least. A consequence of this is that subsidizing entry into the sharing environment, particularly for a low subsidy, may actually lead to less sharing on average among those who choose to enter.

This is the case. The average amount shared by those who enter in Decision 3, when entry is subsidized, is lower than in Decision 2, when entry is not subsidized. In fact, for every subsidy level below $6 (i.e., when the endowment in the dictator game is $16), average sharing conditional on entering the sharing environment is lower than when there is no subsidy.

For example, in Decision 1, when everyone was required to enter the sharing environment, average sharing was $2.68 ($2.42 with anonymity and $2.92 without). In Decision 2, when sorting was costless, average sharing *among those who opted in to the sharing environment* went up to $2.88 ($2.68 with anonymity and $3.11 without), a small increase similar to what we observed for Experiment 1. But when there was a $1 subsidy for entering (Decision 4 with anonymity and Decision 3 without), average sharing by those who entered decreased to $2.22 ($1.92 with anonymity and $2.59 without).

Of course, people might share less in later rounds for reasons other than our selective sorting hypothesis. To distinguish the effects of sorting from other round-specific influences, we estimate the predicted amount that each individual would share, conditional on entry, if the individual shared the same proportion of the endowment as in Decision 1. (For example, an individual who shared $5 in Decision 1 is predicted to share $5.50 when the endowment is $11.) For

Decision 1, the predicted amount is the actual amount of $2.68. For Decision 2, when there is no subsidy, if subjects who entered shared the same amount as in Decision 1, mean sharing among entrants would be $3.35. For the $1 subsidy case, predicted mean sharing for entrants is $2.76. Thus, while subjects may share less than proportionally, the predicted proportion shared indicates that selection alone produces the perverse effect that a $1 subsidy for entering the sharing environment leads to lower sharing in that environment than no subsidy.

Table 5 explores the difference between sorting and "round effects" in a linear regression framework. The first model uses the actual amounts shared by those who enter as the dependent variable. The next two models use predicted amounts, based either on the proportion shared in Decision 1 (Model 2) or on the absolute amount shared in Decision 1, i.e., assuming entrants pocket the subsidy (Model 3). These predicted amounts provide upper and lower bounds on how much individuals would share if they were to neither pocket nor share more than the full amount of the subsidy, which applies to 74 percent of the decisions. The coefficient estimate is positive in all three models and significant for the last two models, indicating that entrants share more. The second explanatory variable indicates the presence of a subsidy (Decisions 3-6), and the third measures the size of the subsidy ($0.50 to $10.00, see Table 2). In all three models, the presence of a subsidy significantly decreases conditional sharing, by about $0.90, but each dollar of subsidy increases sharing, by between $0.05 and $0.35 for the predicted amount shared. Hence, a small subsidy (e.g., $1 or $2) results in lower average sharing in the dictator game than when no subsidy is present. While the findings in the first model could be influenced by people changing how much they share conditional on entry, Models 2 and 3 show that endogenous sorting alone has the same effect.

V. Experiment 3: Costly Entry into Sharing Environments

Thus far, we have focused on the negative effect of sorting on sharing. In every comparison the aggregate amount shared has been lower when sorting was possible. However, comparing the amounts shared *conditional on entry into the dictator game* (with costless sorting), we found slightly greater amounts shared among those who enter than in the baseline, across both experiments and all three locations. While none of the differences is statistically significant, the regularity suggests that sorting, if properly designed, may also allow attracting those most likely to share. This would be attractive if a sharing environment has limited capacity for entry and, hence, the conditional sharing of those few who enter is the relevant sharing outcome.

In this experiment, we explore one possible way to induce self-selection of high sharers, namely to reverse the paradigm of Experiment 2. Instead of subsidizing the sharing environment, we introduce a $1 cost to entering the $10 dictator game. The entry cost should prevent non-sharers and reluctant sharers from entering. It might also prevent some willing sharers. As stated in Prediction 3, we expect to see even less aggregate sharing, but, if the utility from entering and sharing among the willing sharers is increasing in the amount shared, high conditional sharing. Put differently, if an individual is a willing sharer, but shares only a small amount such as $0.50, such an individual may not find it worthwhile to pay a $1 cost to enter the dictator game. But a willing sharer who shares a lot, say $5, may be willing to bear the $1 entry cost.

A. Experimental Design

We conducted a treatment with a $1 entry cost both in Berkeley and in Pittsburgh. The procedures and instructions closely followed Experiment 1. The only major difference was that the amount subjects received for not playing the dictator game (*w'*) was $11 instead of the $10.

Thus, subjects could choose to play the dictator game with $10 or not to play the game, in which case they received an $11 payment and the recipient remained uninformed about the game.

Subjects were recruited in the same manner and from the same populations as for Experiments 1 (Berkeley) and 2 (Pittsburgh). Upon arriving at the experiment, all subjects were told they would receive a participation fee ($7 in Pittsburgh, $5 in Berkeley), and were randomly assigned ID numbers. Half of the participants were taken to another room, where they completed questionnaires. The remaining participants received instructions very similar to those from Experiment 1, except for the $11 outside option. Entry decisions were again made by opening one of two envelopes. Recipients in the other room were only brought into the room and informed of the dictator game if the dictator with whom they were paired had opted to play the game.

B. Results

We collected data from 55 pairs of participants across the Berkeley and Pittsburgh locations.[34] Figure 3 presents, in the two rightmost bars, the average per-subject aggregate sharing in this treatment ($0.76) and the average amount shared conditional on entry into the dictator game ($3.50). For comparison purposes, we include the average amount shared in the Berkeley and Pittsburgh baseline dictator games from Experiments 1 and 2 (leftmost bars) and the average amounts shared, both on an aggregate and conditional on entry, from the pooled costless sorting treatments (*w = w' = $10*) in Berkeley and Pittsburgh from Experiments 1 and 2 (middle bars).

As expected, very few subjects choose to enter the sharing environment when there is a $1 price for doing so. Of the 55 potential dictators, only 12 (22 percent) choose to play the game.

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[34] In one early session, a subject chose to enter and shared zero. When the experimenter asked this subject why, the subject responded that he thought he would not get the $5 participation payment otherwise, meaning that the subject perceived the choice between entering (and receiving $5 plus $10 to share) and not entering and receiving only $11. In subsequent sessions, we asked every subject to provide reasons for their choices after the experiment, and excluded subjects misunderstanding the instructions. This excluded a total of four participants from the analysis.

Women enter more frequently than men (28 percent vs. 15 percent), but this difference is not statistically significant. As a result of the very limited entry, aggregate sharing is low, \$0.76 on average. Thus, not surprisingly, imposing a positive price on entry into the sorting environment produces lower aggregate sharing than in the baseline dictator game from Experiments 1 and 2 (\$2.23 vs. \$0.76). This difference is statistically significant in a non-parametric Wilcoxon rank-sum test ($z = 4.742$, $p < 0.001$). The entry price lowers aggregate sharing even below the costless sorting treatments from Experiments 1 and 2 (\$1.21 vs. \$0.76), and this difference is marginally significant ($z = 1.75$, $p = 0.08$).

However, as we predicted, conditional on entering the sharing environment, participants share considerably more than in the baseline dictator game (\$3.50 vs. \$2.23, $z = 2.12$, $p = 0.03$). Thus, introducing a positive price for entering the dictator game attracts fewer people, but those who enter share more than in the typical baseline dictator game. Participants who enter in the \$1-price treatment also share more than participants who enter the dictator game with zero cost sorting (\$3.50 vs. \$2.54), though this difference is only marginally significant ($z = 1.71$, $p = 0.09$).

The results of the new treatment confirm our prediction that sorting and the resulting sample composition of social-preferences types can produce environments in which participants share more than the population on average. While aggregate sharing is considerably lower when there is a price to entering the sharing environment, those who participate share significantly more than the population as a whole (measured by the standard dictator game). This suggests that in environments with limited capacity for entry and in which it is costly to enter, we might observe more pro-social behavior than one would find by sampling from the entire population.

VI. Experiment 4: Sorting and Reciprocity

Our final experiment tests the role of sorting in the presence of a strengthened motivation for sharing, namely positive reciprocity. Thus far, we have explored the effects of sorting in situations without any prior interaction between the potential dictator and recipient. However, generous behavior is often observed most strongly in contexts involving positive reciprocity – when the other party has previously done something kind. Individual "willingness" to share might be greater in environments in which the other party has previously been kind.

Even without sorting, this willingness might manifest itself in higher average sharing when recipients have done something kind than when they have not. This is what previous studies have found. Our interest, however, is the role of sorting. Does the increased willingness to share affect the frequency with which potential dictators use the sorting option to avoid playing the game? If positive reciprocity is indeed a stronger motive than a simple desire to share with someone, such as altruism or inequality aversion, then we might expect a greater proportion of willing sharers, relative to reluctant ones, in environments involving positive reciprocity. This higher portion might come from individuals who would otherwise not share, hence decreasing the fraction of non-sharers and leaving the fraction of reluctant sharers unchanged. Or, positive reciprocity turns reluctant sharers into willing sharers, decreasing the fraction of the latter. In that case, the reluctant sharing motive that is highly susceptible to sorting opportunities in standard dictator games might be less prevalent in contexts involving positive reciprocity.

To address the above question, we conducted a reciprocity variant of the standard dictator game (a "double dictator game") with and without sorting. In these treatments, we gave the recipients an initial choice of sharing $2 with a matched partner in the other room. They did not know, at this point, that the matched partner would subsequently become the dictator over $10. Sharing $1 voluntarily could be interpreted as an initial act of kindness.

A. Experimental Design

The sessions were conducted at UC Berkeley. The procedures were very similar to those used in the baseline and sorting treatments of Experiment 1. The only substantive difference was the introduction of a first stage in which those who would be recipients in the $10 dictator game could share $2 with the person who would later become the dictator.

Subjects were again randomly assigned to two groups, based on the participant number they drew, and placed in separate rooms. One group of participants (the ultimate recipients) was told to each divide $2 between themselves and a randomly-paired participant in the other group, by circling one of two choices: keep $2 and give the paired participant $0; or keep $1 and send $1 to the other participant. After participants made their choices, the experimenter collected the sheets and gave them to the experimenter in the other room.

Participants in the other room were told about the $2 allocation decision and were shown the choice made by their specific paired participant. They were then informed of the $10 dictator game they could play with this same participant. The instructions describing the dictator game were identical to in Experiment 1 other than emphasizing that the $10 decision was their only decision and the last decision that anybody would make in this experiment.

As in Experiment 1, we varied the availability of sorting. In a baseline treatment, the second group was required to play the $10 dictator game. In the sorting treatment, they could opt out (by opening a different envelope, as in Experiment 1). In this case they received $10 and the partner would not find out about the possibility of the second-stage $10 dictator game.

B. Results

We collected data on 192 pairs (54 in the baseline and 138 in the sorting treatment). In 89 of cases (46 percent), the first-mover shared the $2 with the participant (baseline: 26 of 54, 48 percent; sorting: 63 of 138, 46 percent). In order to make comparisons across identical populations, we compare the data from the new reciprocity treatments to the data from the other ("No Reciprocity") dictator games conducted in Berkeley in Experiment 1.

Figure 4 presents the mean aggregate amounts shared per potential dictator in the Reciprocity treatment, both when the first-mover shared $1 (first set of bars) and when the first-mover shared $0 (second set of bars). The third set of bars presents the mean amounts shared in the No Reciprocity dictator games from Experiment 1 conducted at Berkeley. Each set of bars contrasts the average amount shared both with and without sorting.

When sorting is not possible ("baseline" bars), we find evidence of a reciprocity motive, consistent with previous experimental findings. Mean sharing when the dictator received nothing from the first mover is $0.70, while if the dictator received $1 it is $2.39. For comparison purposes, the amount shared by dictators in the No Reciprocity session from Experiment 1 is $2.00. (Note that, after sharing $1, recipients end up with $1+$2.39 = $3.39 out of $12 (28.3%) on average, a significantly higher fraction than in the baseline dictator game (20%, difference significant at $p < 0.001$)). Table 6 presents regressions of the amounts shared by the dictator in the $10 game on a dummy for Reciprocity (with No Reciprocity consisting Experiment 1 in Berkeley), a dummy for Sorting, and a control for the amount received from the first mover. The first column includes only the data from sessions where the dictator could not opt out of playing the dictator game. The coefficient on amount received reflects the strong presence of reciprocity.

As in every experiment thus far, the no-cost sorting opportunity decreases sharing significantly, even under positive reciprocity. Looking back at Figure 4, we see that aggregate sharing decreases in all cases (from \$2.39 to \$1.71, or 29 percent, when \$1 was received; from \$0.70 to \$0.31, or 56 percent when \$0 was received; and from \$2.00 to \$1.11, or 44 percent under No Reciprocity). As Columns 2 and 3 in Table 6 reveal, the significant decrease due to sorting option does not differ significantly by Reciprocity treatment or by Amount Received. (A test of the restriction that the two interaction terms are equal to zero fails to reach statistical significance, $F(2, 307) = 0.50$). Therefore, sorting appears to exert very similar effects on aggregate sharing in situations involving reciprocity to those that involve non-reciprocal motives for sharing.

At the same time, however, we do observe a persistent effect of reciprocity. Returning to our primary interest, the self-selected sample composition of different types, willing and reluctant sharers, we see an increase in willing sharers after positive reciprocity is induced. As a result, the relative fraction of reluctant to willing sharers decreases.

Table 7 calculates the fractions for the Reciprocity and No Reciprocity treatments, differentiating by amount received under Reciprocity. In all three columns, we see that sorting reduces the frequency of sharing, by similar amounts under positive reciprocity and no reciprocity (23.4 percent and 26.3 percent) and by a lower amount under negative reciprocity. The lower amount in the negative reciprocity might, however, simply reflect censoring – sharing is low even without the sorting option. Overall, we see a continued effect of sorting of similar magnitude as in the basic experiments, even after inducing positive reciprocity. The final column of Table 6 tests the relationship between sorting, reciprocity and sharing more formally in a probit regression, using as a dependent variable whether a subject shared a positive amount. The coefficient on sorting is negative and significant, indicating that roughly 26 percent of sharing is done reluctantly. However, the two interactions with Reciprocity and with Amount Received are statistically insignificant. Thus, the statistical effect of sorting options on sharing appears very similar when sharing is motivated by reciprocity than when it is not.

At the same, comparing the proportions of subjects who shared something in the no sorting treatment (first row) and in the sorting treatment (second row), we see that positive reciprocity induces a persistent increase in sharing even if there is sorting (24.1 percent under no sorting, 26.9 percent under sorting, comparing the behavior after receiving $1 to the behavior in No Reciprocity experiments). This result shows the strength of positive reciprocity. And, as a result, reluctant sharers make up a smaller fraction of all sharers in the Reciprocity (Received $1) case (23.4/88.5 = 26 percent) than in the No Reciprocity case (26.3/64.4 = 41 percent).

The bottom half of the table also shows that the proportion of willing sharers varies significantly, depending on the Reciprocity treatment and the actions of the first-mover. Not surprisingly, this proportion is highest (65 percent) when the first mover did something kind and lowest (21 percent) when the first mover did something unkind. The proportion of non-sharers varies in a similarly intuitive manner – it is lowest (12 percent) when the first mover did something kind and highest (68 percent) when the first mover did something kind.

A third result of interest attests to the strength of negative reciprocity. In the classification of social-preference types at the bottom of Table 7, we add a fourth type, the "jerks." These subjects choose to enter the sharing environment but share nothing (thus revealing to the recipient that the game was played but nothing was shared). The proportion of jerks is much higher when the first-mover shared nothing with the subsequent dictator (20 percent) than when the first mover shared $1 (3 percent) or did not have the option to share (7 percent). That is, when the first-mover shared nothing but had the opportunity to do so, second movers are not just less likely to share, but also more likely to let the first mover know they could have shared. Using a non-parametric chi-square test, we see that the proportion of subjects who entered but did not share is significantly higher in the Reciprocity (Received \$0) case than in either the Reciprocity (Received \$1) case ($\chi^2(1) = 8.97$, $p < 0.01$) or the No Reciprocity case ($\chi^2(1) = 5.92$, $p = 0.02$). Thus, when a first mover has done something unkind, second movers are not just less likely to not share, but they are more likely to want to let the first mover know that they did not share.

VII. Conclusion

People regularly sort into and out of economic environments such as firms, markets, and institutions. In the laboratory, instead, subjects are typically placed in one particular situation and forced to make a choice that they might avoid making outside the laboratory. The goal of our analysis is to model the influence of a sorting decision in the context of social preferences and to investigate how it affects conclusions drawn from laboratory environments without sorting.

One such laboratory setting where subjects typically do not have the option to sort is the dictator game, a common laboratory test of sharing and altruism. We introduce the possibility of sorting into this laboratory environment and find that sorting significantly reduces sharing. Choosing subjects randomly, and forcing them to play the dictator game, might lead us to believe that sharing is pervasive outside the laboratory. However, allowing people to avoid the sharing situation leads to the opposite conclusion: a subset of individuals share, but the majority avoids situations where sharing is possible.

While the above point has been made before, we demonstrate that the impact of sorting reflects the differential sorting of different subjects with different social preferences, and that the relationship between the amount an individual is willing to share and that individual's willingness to enter the sharing environment is not always linear or intuitive. The laboratory setting allows us to influence self-selection using varying prices (or subsidies) for sorting, i.e., for the outside option relative to the sharing environment. Subsidizing entry into the sharing environment attracts primarily those who share the least. Thus, while subsidies encourage entry and may increase the aggregate amount shared, low enough subsidies result in environments in which less is shared, on average, by those who enter. Making entry into the sharing environment costly, instead, attracts primarily those who share large amounts. Thus, when entry is costly, few people enter and aggregate sharing is low, but those who enter the sharing environment share a lot, on average.[35] Finally, we show that the sorting effect is sufficiently strong to persist even after inducing positive reciprocity.

The possibility of sorting influences the impact of social preferences outside the laboratory. Sorting options determine the type of social preferences encountered in a given field setting. Our work provides an example of how this influence needs to be accounted for when generalizing laboratory results to non-laboratory environments and how the laboratory can be used to systematically study the responsiveness of different types to varying sorting options.

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[35] These properties of sorting and sharing cannot be found by simply correlating the amount an individual shares with the price at which she enters the sharing environment, which was the test used by Broberg et al. to study the relationship between preferences and sorting. Indeed, as we report in Online Appendix 2, when we reexamine their data we find strong support for our specific predictions, which complement the results we report here.

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Table 1 – Effects of sorting on amount shared and likelihood of sharing (Experiment 1)

Model:	**OLS**		**Probit (marginal effects)**	
Dependent variable:	***Amount shared***		***Shared something***	
	(1)	(2)	(3)	(4)
Sorting	-1.025** (0.374)	-0.793* (0.385)	-0.309*** (0.086)	-0.253** (0.097)
Barcelona		-0.126 (0.653)		-0.041 (0.099)
Sorting X Barcelona		-0.503 (0.722)		-0.139 (0.176)
Observations	168	168	168	168
(pseudo) R^2	0.070	0.084	0.070	0.082

Robust standard (errors clustered by session) in parentheses
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 2. Determinants of Sharing (Experiment 1)

OLS regressions with Total Amount Shared (out of 10) as the dependent variable .

	(1)	(2)	(3)	
				Partial Coefficients of Determination
Sorting	-1.012		-1.007	
	(0.289)***		(0.292)***	0.27
Gender: Female		-0.117	-0.154	
		(0.311)	(0.301)	0.04
Ethnicity: Catalan		0.288	0.415	
		(0.447)	(0.433)	0.08
Ethnicity: Asian		0.222	0.091	
		(0.506)	(0.490)	0.02
Ethnicity: White		-0.756	-0.746	
		(0.652)	(0.630)	0.10
Socio-economic status: middle class		-0.020	0.097	
		(0.385)	(0.373)	0.02
upper to middle class		-0.056	-0.052	
		(0.396)	(0.382)	0.01
Age group: Graduate Student		-0.078	-0.087	
		(0.488)	(0.471)	0.02
Major: Business or Economics		-0.419	-0.385	
		(0.347)	(0.336)	0.09
University: Berkeley		0.094	0.188	
		(0.613)	(0.593)	0.03
University: Pompeu Fabra		-0.649	-0.726	
		(0.471)	(0.455)	0.13
Siblings: 0 siblings		0.532	0.367	
		(0.567)	(0.550)	0.05
1 sibling		-0.777	-0.828	
		(-0.360)**	(0.348)**	0.19
3 or more siblings		-0.385	-0.467	
		(0.506)	(0.489)	0.08
Donation (during past year)		-0.451	-0.312	
		(0.289)	(0.316)	0.08
Risk-seeking		0.450	0.329	
		(0.317)	(0.308)	0.09
Constant	1.942	2.107	2.583	
	(.206)***	(0.628)***	(0.622)***	
Observations	168	167	167	
Adjusted R-Square	0.06	0.03	0.10	

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3. Aggregate amount shared and sorting behavior by treatment and decision (Experiment 2)

Sorting Option ($10)	Endowment in Dictator Game	Anonymity			No Anonymity		
		Decision	Average amount shared	Percent entering	Decision	Average amount shared	Percent entering
No	$10.00 (40 tokens)	1	$2.42	100%	1	$2.92	100%
Yes	$10.00 (40 tokens)	2	$1.22	46%	2	$1.17	38%
Yes	$10.50 (42 tokens)	3	$1.34	57%			
Yes	$11.00 (44 tokens)	4	$1.42	74%	3	$1.51	58%
Yes	$12.00 (48 tokens)	5	$1.52	76%			
Yes	$13.00 (52 tokens)				4	$2.07	73%
Yes	$16.00 (64 tokens)				5	$3.21	90%
Yes	$20.00 (80 tokens)				6	$4.53	100%
Number of sessions			6			6	
Number of subjects (dictators)			92 (46)			96 (48)	

Table 4. Determinants of Entry into Sharing Environment
(Experiment 2, Excluding Decisions 1 and 2 and Excluding Unclassified Subjects)

	Sample				
	All Classified Subjects	**Willing and Reluctant Sharers Only**			**Reluctant Sharers Only**
	(1)	(2)	(3)	(4)	(5)
Initial Amount Shared		0.000 (0.017)	-0.050^{***} (0.018)	0.028 (0.042)	-0.082^{***} (0.026)
Non-sharers	-0.154^{**} (0.077)				
Reluctant sharers	-0.346^{***} (0.060)		-0.350^{***} (0.052)	-0.025 (0.196)	
Initial Amount Shared X Reluctant sharers				-0.088^{*} (0.046)	
Endowment in Dictator Game	0.068^{***} (0.009)	0.059^{***} (0.010)	0.067^{***} (0.010)	0.066^{***} (0.010)	0.086^{***} (0.014)
Observations	312	234	234	234	141
Pseudo-R^2	0.228	0.113	0.270	0.279	0.223

The table reports marginal effects of probit estimations. Robust standard errors in parentheses
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 5. Effects of sorting and subsidies on average amount shared conditional on entry (Experiment 2)

Dependent variable:	***Actual amount shared*** (1)	***Predicted amount shared (fixed proportion)*** (2)	***Predicted amount shared (fixed amount)*** (3)
Sorting option (Decisions 2 – 6)	0.200 (0.293)	0.674*** (0.253)	0.674*** (0.253)
Presence of Subsidy (Decisions 3 – 6)	-0.941*** (0.263)	-0.955*** (0.270)	-0.894*** (0.253)
Amount of Subsidy	0.260*** (0.065)	0.350*** (0.065)	0.052 (0.041)
Constant	2.678*** (0.214)	2.678*** (0.214)	2.678*** (0.214)
Observations	382	382	382
(pseudo) R^2	0.072	0.143	0.015

The table reports OLS estimates. Robust standard (errors clustered by subject) in parentheses
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 6. Average amount shared across Reciprocity and No Reciprocity Treatments (Experiments 1 and 4)

	Sample			
	Baseline only (No sorting)	**All data**		
	(1)	(2)	(3)	(4)
Dependent variable:	*Amount shared (opt out = $0)*			*Shared something*
Model:	*OLS*			*Probit (marginal effects)*
Reciprocity treatment	-1.303*** (0.443)	-0.961*** (0.213)	-1.304*** (0.441)	-0.323*** (0.114)
Amount received	1.698*** (0.499)	1.484*** (0.243)	1.698*** (0.496)	0.575*** (0.101)
Sorting		-0.681*** (0.224)	-0.888** (0.367)	-0.263*** (0.091)
Sorting X Reciprocity treatment			0.498 (0.502)	0.135 (0.150)
Sorting X Amount received			-0.294 (0.569)	-0.185 (0.172)
Constant	2.000*** (0.299)	1.870*** (0.228)	2.000*** (0.297)	
Observations	99	313	313	313
(pseudo) R^2	0.113	0.136	0.138	0.139

Robust standard errors in parentheses
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 7. Proportion sharing something across Reciprocity and No Reciprocity Treatments (Experiments 1 and 4)

	"Positive" Reciprocity (Received $1)	**"Negative" Reciprocity (Received $0)**	**No Reciprocity (Experiment 1)**
a. Proportion sharing something in no sorting treatment	23/26 (88.5%)	9/28 (32.1%)	29/45 (64.4%)
b. Proportion sharing something in sorting treatment	41/63 (65.1%)	16/75 (21.3%)	29/76 (38.2%)
c. Proportion opting out in sorting treatment	20/63 (31.7%)	44/75 (58.7%)	42/76 (55.3%)
Estimated frequencies of types:			
Willing sharers (b)	65.1%	21.3%	38.2%
Reluctant sharers (a-b)	23.4%	10.8%	26.3%
Non-sharers (100-a)	11.5%	67.9%	35.6%
"Jerks" (100-b-c)	*3.2%*	*20.0%*	*6.6%*

Figure 1A. Distributions of Amounts Shared (Experiment 1, Berkeley)

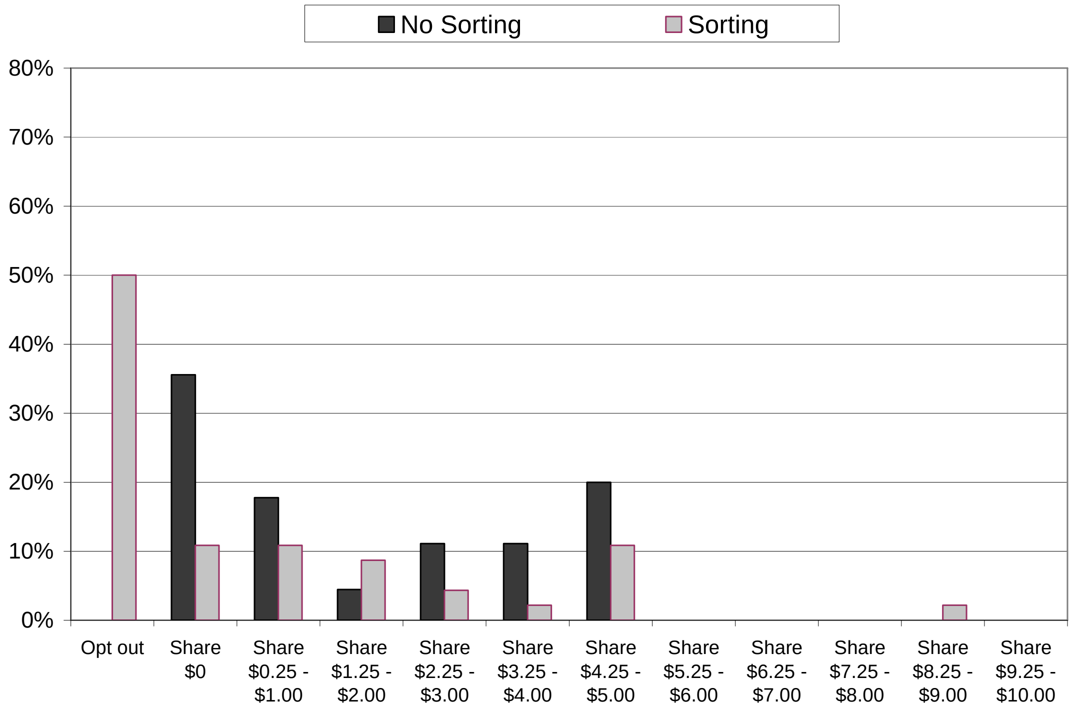


Figure 1B. Distributions of Amounts Shared (Experiment 1, Barcelona)

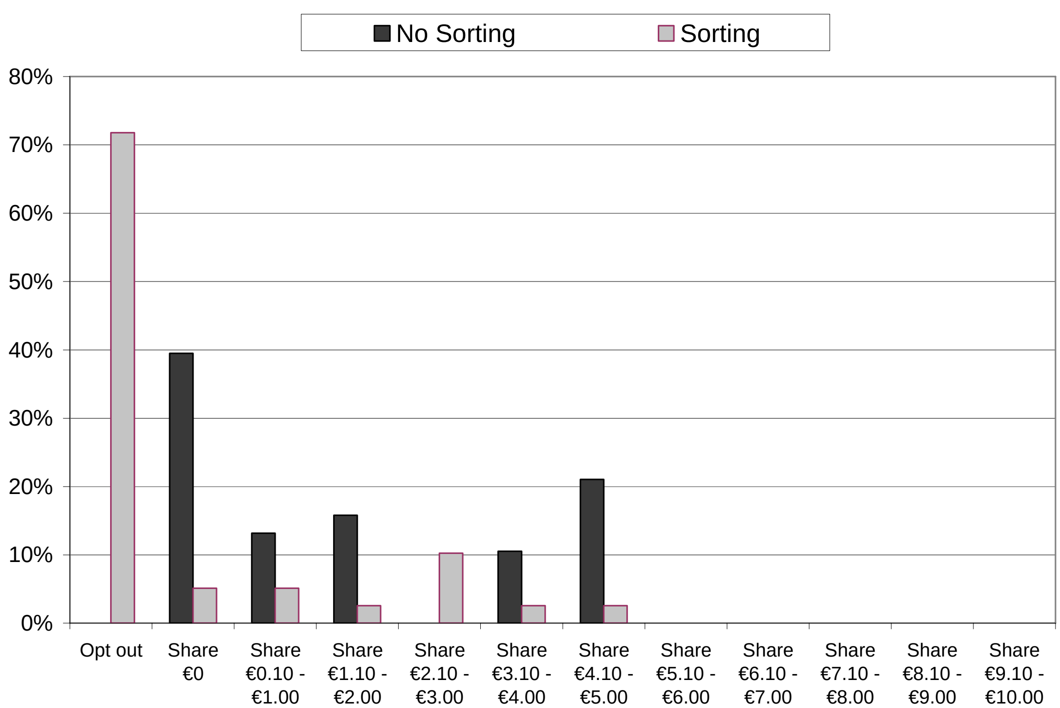


Figure 2A. Proportion of Reluctant Sharers Choosing to Play by Decision and Initial Amount Shared (Anonymity)

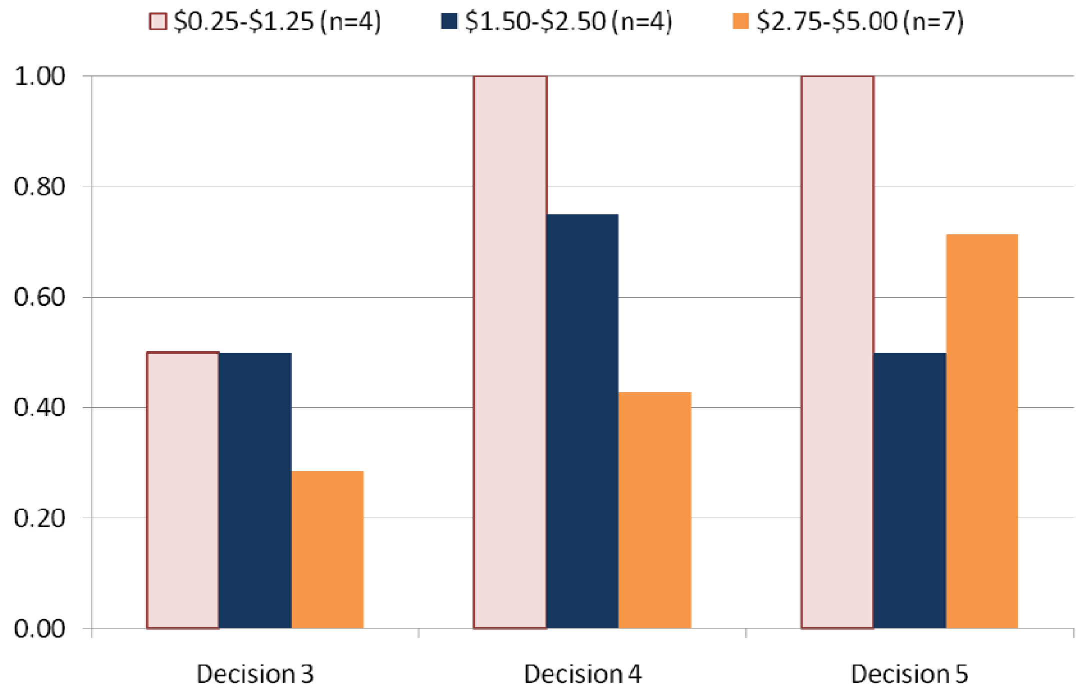


Figure 2B. Proportion of Reluctant Sharers Choosing to Play by Decision and Initial Amount Shared (No Anonymity)

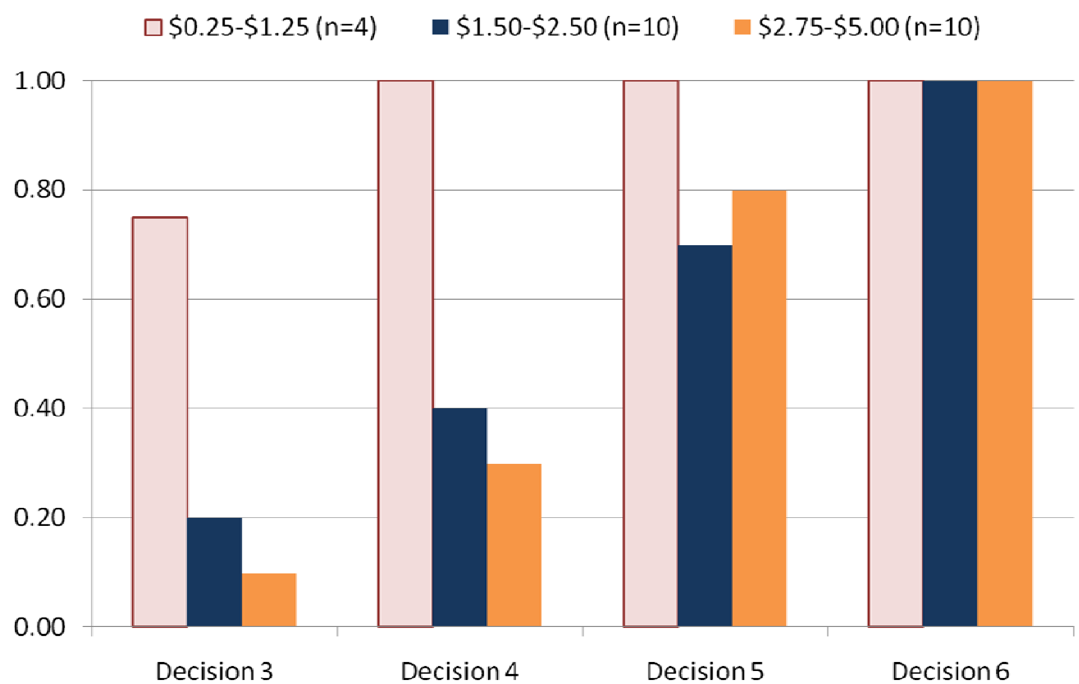


Figure 3. Mean Aggregate and Conditional Sharing by Treatment (Pooled Berkeley and Pittsburgh Data from Experiments 1, 2, and 3)

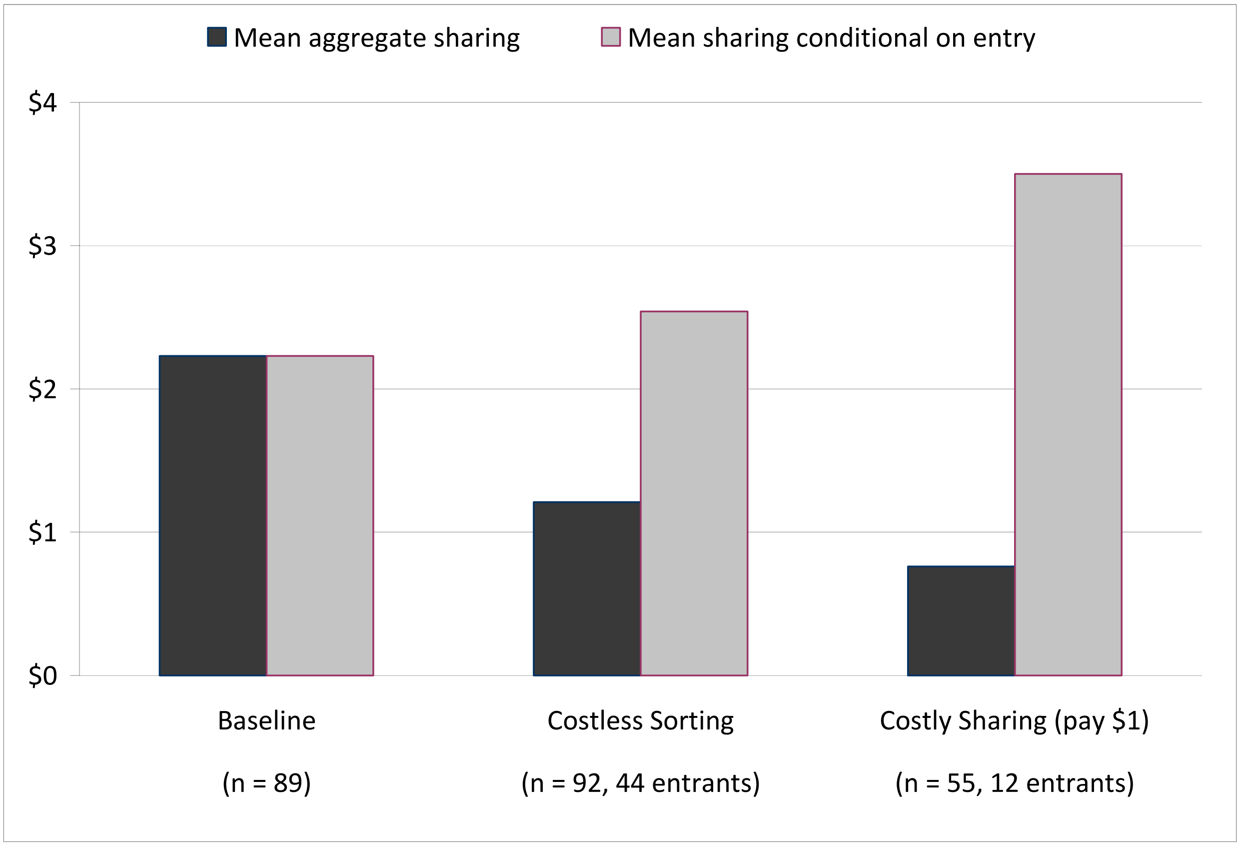
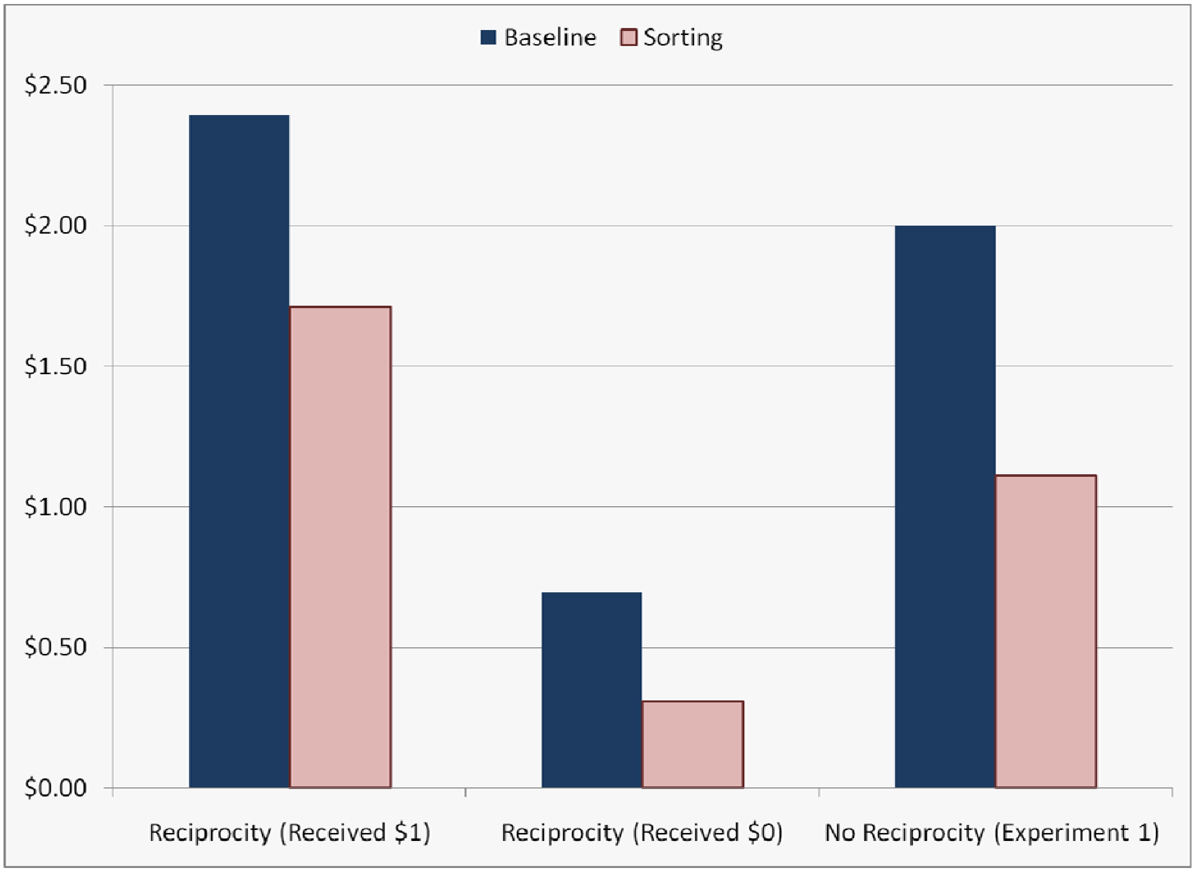


Figure 4. Mean aggregate amount shared by Reciprocity and Sorting treatments (includes data from Experiment 1 Berkeley sessions)



Appendix Figure 1. Sharing by Subsample (Experiment 1)

Sharing by subsamples based on demographics and elicited preferences. The number in parentheses next to each subgroup indicates the number of dictators. The left bar in each subgroup indicates the average amount shared in the treatment without sorting; the right bar the average amount shared with sorting.