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SORTING, PRICES, AND SOCIAL PREFERENCES

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ABSTRACT

What impact do social preferences have in market settings where individuals can sort between different environments in response to relative prices? In four laboratory experiments, we show that sorting and prices strongly affect which social-preference types select to enter an economic environment and, as a result, the observed sharing behavior. Sorting allows us to distinguish between individuals who share because they obtain utility from sharing and those who share if asked to but obtain higher utility if not asked to share. We find that costless sorting strongly reduces the number of subjects who share their endowment, even when they have a strong motivation for sharing (positive reciprocity). Relative prices significantly alter the compositional effect of sorting. If the sharing environment is subsidized, we observe additional entry, but only by the least generous sharers who keep most of the subsidy for themselves. As a result, a small subsidy significantly reduces the average amount shared by those who enter. If, instead, the sharing environment is costly relative to the outside option only individuals who obtain a high utility from sharing choose to participate. This yields an environment in which entry is low but significantly more is shared on average

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I. Introduction

Social preferences have been shown to strongly affect individual behavior both in the laboratory, e.g., in dictator games, and in the field, e.g., in charitable giving (see, e.g., the reviews in Camerer, 2003, and Andreoni, 2006). A large prior literature provides evidence of specific types of social preferences, such as pure or impure altruism and reciprocity, and links these preferences to sharing behavior (Fehr and Schmidt, 2002). The existence, or even prevalence, of certain types of social preferences, however, does not immediately imply that they are an important determinant of real-world sharing behavior. If individuals have the opportunity to sort between different economic environments that do and do not allow sharing and face different prices in these environments, the impact of social preferences will depend on how different types sort in response to the relative prices and on the resulting population in the sharing environment.

In this paper, we show that sorting strongly affects the composition of social-preference types who are present in a given economic environment. As a result, the observed sharing behavior depends on who sorts into or out of the environment. We also show that relative prices can be used to significantly alter sorting and, hence, the compositional effects. Sorting and its interaction with prices allow us to distinguish between individuals who obtain positive utility from sharing (e.g., due to pure or impure altruism) and those who share if asked (e.g., due to social pressure) but would prefer not be in such a situation in the first place. The differential sorting behavior of these social-preference types explains why average amounts shared differ dramatically in environments with and without sorting and depending on the price of sorting. Similar to Fehr and Schmidt (1999), we find that the heterogeneity 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 illustrate that mere sorting suffices to produce samples (and behavior) that look very different from an unbiased sampling of the population.

Our point is intuitively evident when considering examples from the field, where environments with the greatest opportunities for charitable behavior attract very different people than a representative sample of the population. For example, many foreigners incurred significant costs to reach Haiti following the 2010 earthquake. These people were likely non-representative of the broad populations, and likely included the most generous types. At the same time, disaster areas also often attract disproportionately those who are least likely to behave pro-socially, such as looters and profiteers. On a less dramatic scale, tourists who visit poor countries include those who care about the locals and look for opportunities to help, but also those who do not care at all and enjoy the low prices. Others, who feel obligated to help if asked but do so reluctantly, avoid these countries altogether. In all such cases, the composition of social preferences and the prevalence of giving look very different from what one would obtain if individuals were randomly drawn from the broad population.

To better understand the effects of sorting and prices on sharing, we distinguish three classes of social preferences. First, "willing sharers" share a positive amount and seek the opportunity to do so. Second, "reluctant sharers" share if they are in the sharing environment, but prefer to avoid the environment altogether. Third, "non-sharers" simply never share. These three classes comprise a variety of social preferences discussed in the literature. For example, "willing sharers" might be motivated by pure or impure (warm-glow) altruism (Fehr and Gaechter, 2000; Andreoni, 1989 and 1990), or they might share for self-signaling reasons (Bodner and Prelec, 2002; Benabou and Tirole, 2006; Grossman, 2009). "Reluctant sharers" may share, if asked to, because they feel shame, guilt, or social pressure to conform to a request (Milgram, 1963; Bernheim, 1994; Tadelis, 2008, Battigalli and Dufwenberg, 2007). And non-sharers might have "classical" self-interested preferences, or they may feel social pressure to give, but are able to resist the pressure. Our coarse distinction, based on observed behavior, suffices to generate predictions about differential sorting into different economic environments as the price for entry varies, and about the resulting sample composition.

The analysis consists of four laboratory experiments that use variants of the dictator game, including a double-dictator game intended to induce positive and negative reciprocity. In each experiment, we measure how much of an endowment w participants voluntarily share with another subject. Each experiment has a treatment in which participants are allowed 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 price of entering the sharing environment by varying the endowment in the dictator game (w) relative to the outside option (w').

Experiment 1 introduces sorting into a standard dictator game, similar to Dana, Cain, and Dawes (2006). We find that costless sorting ($w = w'$) reduces the number of sharers by half relative to a game without sorting, implying that giving is utility-decreasing for at least half of the givers in the standard dictator game. Surprisingly, the reduction in giving includes the most generous sharers, suggesting that observed generosity is not a good indicator of sharers' utility from giving. The sorting effect is robust across geographic locations and within demographic subsamples in our data. In fact, in comparison to a large set of demographic characteristics we elicited, including social background and actual charitable giving, sorting has considerably greater economical and statistical predictive power.

Experiment 2 tests whether a strengthened motivation for sharing helps to overcome the sorting effect. Several researchers note that reciprocity is a strong motive for generous behavior relative to other motives such as altruism (Fehr and Gächter 2000; Sobel 2005; Cox, Friedman and Gjerstad 2007). We ask whether reciprocity is strong enough to mitigate reluctance in sharing and therefore prevent individuals from opting out. For example, if small gifts (e.g., 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 address this question, we employ a double-dictator game, in which the ultimate recipient first decides about sharing $2 with the ultimate dictator, not knowing about the ultimate $10 dictator game taking place subsequently.[1] We find that positive reciprocity, induced by the recipient initially sharing money, increases giving relative to the baseline (single) dictator game and that the increase in average giving persists after we introduce sorting. That is, comparing double- and single-dictator games, the reciprocity-induced (additional) giving after a small gift is robust to sorting. However, comparing double-dictator games with and without sorting, we also continue to find a significant decrease in giving when sorting is possible, very similar in size to the effect of sorting in the single-dictator game. Thus, positive reciprocity does not eliminate reluctance to share. We also find that negative reciprocity (induced by receiving $0 out of $2) virtually eliminates giving in the setting with sorting and is strong enough to induce some people *not* to sort out and to then share zero.

Next, price effects are introduced to explore whether relative prices of the environments with and without sharing can be exploited to affect the differential sorting of individuals with different social preferences. In Experiment 3, we subsidize entry into the sharing environment.

[1] Differently from the two-part dictator games in Ben-Ner et al. (2004), we use a "mini"-dictator game in the first stage to distinguish reciprocity from distributional preferences (e.g., inequity aversion). We are also careful to compare the behavior in the double-dictator game to a baseline dictator game, rather than comparing behavior within the double-dictator game, thus avoiding issues of mis-identifying unconditional kindness as reciprocity (see Cox, 2004).

That is, we make the dictator game financially more attractive relative to the outside option ($w > w'$). As a result, all "willing sharers" and "non-sharers" should enter. However, among the "reluctant sharers," only those should enter for whom the additional endowment $w - w'$ more than offsets the disutility from being pressured (or shamed) into giving. We find that the subsidy leads to greater entry into the sharing environment and a higher aggregate amount shared. However, it disproportionately attracts those who share the least – non-sharers and the least generous reluctant sharers. As a result, the introduction of a small subsidy *lowers* the average amount shared among entrants. Thus, subsidies intended to induce individuals to share may have the counter-intuitive effect of attracting those who share the least.

Experiment 3 also utilizes a within-subject design, in which we confront subjects with increasingly higher subsidies. This accomplishes two goals. First, we can show directly that those reluctant sharers who share the most in a standard dictator game (without sorting) are least willing to re-enter the dictator game; they return only for very high subsidies. Second, we use the within-subjects data to classify social-preference types more precisely, relative to the between-subjects design in Experiment 1, where we observe each individual only once.

Experiment 4 increases the cost of entry into the sharing environment relative to the outside option ($w < w'$). As a result, all non-sharers and reluctant sharers should opt out. Among the willing sharers only those who obtain a high utility from giving should enter. We find that few subjects enter, but those subjects share substantially. While the aggregate amount shared is low because of the low frequency of entry, the average amount shared by those who enter is significantly higher than in the standard dictator game. Hence, with a cost of entry, the sharing environment attracts primarily those who share the most.

Our results show that the impact of social preferences on observed sharing behavior is significantly affected by the ability to sort and by the relative prices for doing so. Different types of sharers – those who obtain utility from sharing and those who do not – respond differently to the opportunity to avoid the sharing environment and the cost (or subsidy) of doing so. This main 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 the 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.[2] In the field, individuals sort into and out of environments based on preferences and prices. Thus, individuals who participate in a market are unlikely to be a random sample of the population.[3] Our experiments provide one example of how market-like features of an economic environment can significantly alter the observed behavior. However, rather than asking whether the same person displays different social preferences in a "standard" laboratory game than in a game that incorporates market features (e.g., starts to display less pro-social behavior when the framing changes as in List, 2006), we argue that the changing sample composition alone accounts for significant changes in observed behavior.

In addition, sorting helps identify social preferences. In an environment where opting out is difficult, an individual may appear to be a willing sharer, but may actually prefer to avoid sharing if possible. While our paper does not aim towards nor can 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 distinguish different motives. In this sense, our first baseline 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 interested in the motivation for giving and the question of whether some subjects are willing to pay to avoid the dictator game, we focus on the effect of sorting, interacted with price variation, and ask which social-preference types take advantage of the sorting option and what effect such sorting has on the

[2] 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).

[3] Critics have questioned whether experimental results based on samples selected among college students apply 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.

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

composition of and behavior in the resulting sharing environment.[5] Thus, although previous experimental evidence demonstrates that at least some sharing is "reluctant" or "involuntary," we go further in demonstrating how sorting and the price of sorting interact with such preferences to yield sharing outcomes that look different than the standard dictator game result.[6]

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 without a sharing opportunity, the agent receives a possibly different amount *w'*, 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 the 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 define the proportion shared in the sharing environment to be $a = x / (x + y)$.

We distinguish three types of social preferences, based on the observed behavior 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 of entering the game is less than or equal to zero. Such social preferences capture a range of sharing motives, including

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[5] Broberg et al.'s (2007) data allows exploring some of the hypotheses we consider here, regarding the effects of prices on sorting, which they do not analyze in their paper. In Appendix 2, we re-examine their data and find strong support for our novel predictions.

[6] Another related strand of the experimental literature shows that subjects' willingness to share declines when they have "earned" their endowment, e.g., by exerting effort (see, for example, Rutström and Williams, 2000, and Cherry, Fryckblom, and Shogren, 2002). These findings imply, similar to the results in this paper, that the desire to maximize one's own payoff may be more prevalent than earlier experiments indicate once we account for a "real-world" feature, here, the fact that people typically earn their income. Also related is the experimental literature showing that subjects' willingness to share can be reversed when allowing them to take money from their matched partners (e.g., Bardsley, 2006, and Cox, Sadiraj, and Sadiraj, 2008).

[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 the non-sharing environment, e.g., disutility from (self-)signaling that they prefer not to face the request to share. If such effects exist, our experiments *under*estimate the extent of reluctant sharing.

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 due to social pressure to comply with a sharing norm, out of shame or guilt at not sharing, or due to "emotional altruism," i.e., not wanting to hurt the other person's feelings by saying no to a sharing request. We do not distinguish between these explanations. Our goal is simply to detect the reluctance to share, using the sorting option, to assess its magnitude, and to derive the responsiveness of this broad class of "social preferences" to the existence and 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'*) to avoid sharing. We emphasize the special case where a sufficient condition for $\max_{x\in[0,w]} U(1,x,w-x) <$ $U(0,w,0)$ is that the premium necessary to induce a reluctant sharer to be indifferent between the sharing and no sharing environment is given by *w'*/*w* = 1 - *a*, where *a* is the proportion shared in the sharing environment. This implies that, among the reluctant sharers, those who share the most in the sharing environment, are also willing to pay the most to avoid the sharing environment. We refer to this condition as "relative sharing aversion:" the most generous reluctant sharers are least willing to enter the environment where sharing is possible.[9] (See Prediction 2 below.)

The third type does not share, even if the environment allows for sharing: $\arg\max_{x\in[0,w]}$ $U(1,x,w-x) = w$. We call this type "non-sharer." Most straightforward, non-sharers are standard economic agents who derive utility from their own payoff and are not affected by the presence of a sharing opportunity: $\max_{x\in[0,w]} U(1,x,w-x) = U(1,w,0) = U(0,w,0)$. Holding the endowment constant, they are indifferent between environments with and without sharing, and we have no prediction about their sorting when *w* = *w'*.[10] When the sharing environment yields a

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[9] A modified Cobb-Douglas utility function, described in Appendix 1, which allows for utility from sharing for reluctant sharers, also has this property. Relative sharing aversion also obtains under the simple assumption that the utility of reluctant sharers is determined solely by their own monetary payoff. Relative sharing aversion is, however, not a logical necessity. For example, a person might be close to indifferent between sharing or not, preferring to avoid sharing, but once in the sharing environment chooses to share a great deal. Another, who detests sharing, might part with only a few pennies in the sharing environment, but bears tremendous embarrassment from doing so. The second might pay more to avoid the sharing environment than the first.

[10] Alternatively, non-sharers may derive disutility from social pressure or guilt etc. like reluctant sharers, but are able to resist the pressure to share. Such "reluctant non-sharers" opt out even if the endowment is identical in both

greater payoff ($w > w'$), however, we predict that they enter (and share nothing).

Based on the above three kinds of social preferences, we can generate simple predictions about sharing and sorting behavior and its interaction with prices. The first prediction deals with the impact of reluctant sharers if sorting is possible at no cost:

Prediction 1: *The introduction of a sorting option in a sharing environment, with equal endowment in the environment without sharing (w = w'), reduces the aggregate amount shared.*

This follows immediately from the fact that reluctant sharers opt out of the sharing environment when costless sorting is introduced ($w = w'$).

Prediction 1 also gives rise to the question of whether stronger motives for sharing reduce the sorting effect. Specifically, if an initial kind act by the recipient yields more sharing, consistent with previous research on reciprocity, will the introduction of sorting still affect sharing to the same extent? Given that reciprocity is often interpreted as increasing subjects' utility from giving, one may expect a higher fraction of subjects to be willing sharers and, hence, the fraction opting out to decrease relative to the baseline case with no reciprocity. If the inducement of reciprocity does not increase the utility from giving but the social pressure to give, we might expect a larger proportion of subjects to opt out, and the sorting effect would not be diminished. Thus, we test for the differential effect of sorting under strengthened motives for giving below.

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

Prediction 2: *Entry into the sharing environment increases in the size of the positive subsidy* ($w > w'$). *Under relative sharing aversion, entry is decreasing in the portion agents originally share (a).*

First, subsidizing the sharing environment increases entry. Willing sharers enter even when the subsidy is zero, and all non-sharers enter at any positive subsidy. Thus, the prediction depends only on the behavior of the reluctant sharers, who opt into the sharing environment when $\max_{x\in[0,w]} U(1, x, w-x) > U(0, w', 0)$. The larger is w, the more likely is this condition to hold.

Second, under relative sharing aversion, the condition of indifference between the two environments is $w'/w = 1 - a$. Thus, for any given w and w', individuals with lower values of a

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environments, i.e., they prefer not to be asked to share, $U(1, w, 0) < U(0, w, 0)$. Neglecting this type of "reluctance" results in its *under*estimation and makes our estimates of the relevance of sorting more conservative.

are most likely to opt into the sharing environment. Those with the highest values of *a* are most likely to opt out. Intuitively, those reluctant sharers who share the most, if asked, have the most to gain from avoiding the sharing environment. As a result, reluctant sharers' generosity, conditional on giving, is a *negative* predictor of their inclination to enter the sharing environment.

Prediction 3 considers the opposite pricing differential.

Prediction 3: *Making the sharing environment costly relative to the outside option (w < w') decreases entry and the aggregate amount shared but, for some cost. increases average sharing among those who opt into the sharing environment.*

Costly sharing induces both the non-sharers and the reluctant sharers to sort out. Among the willing sharers only those remain who care a lot about the ability to share, i.e., about the payoff of the other person. Hence, while it is clear that aggregate sharing decreases, we expect the conditional average amount shared to be high. Conditional sharing will be strictly higher if the subset of willing sharers who enter at any given cost *w' – w* share at least as much, on average, as the non-sharers, reluctant sharers, and those willing sharers who opt out.

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

III. Experiment 1 – Costless Sorting

Experiment 1 uses a between-subjects design to compare outcomes in dictator games without and with sorting, holding constant the endowment (*w* = *w'*). It provides the general set-up for the more subtle tests in Experiments 2-4.[11]

A. Experimental Design

Experiment 1 was conducted in two locations, Barcelona and Berkeley, with graduate and undergraduate students at the Universitat Pompeu Fabra (UPF), the Universitat Autònoma de Barcelona (UAB), and UC Berkeley. We conducted 16 sessions – eight in each city. Each session consisted of an even number of 10 to 36 participants and lasted 20 to 25 minutes. 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.

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[11] Experiment 1 generalizes the dictator game in Dana et al. (2006) and shifts the emphasis to sorting effects and their heterogeneity across different social-preference types rather than on determining the "average" underlying social preferences. We also add to their findings by showing the persistence of sorting across demographic subgroups.

Upon arrival, subjects were told that they would receive a participation fee (€5 in Barcelona[12] and $5 in Berkeley) and that they might earn additional money. Subjects randomly drew participant numbers, which determined their role. 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, the dictators, were located in a separate room and received instructions, both in writing and aloud. [13] 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, show each of them how much money they received, and then pay them. Each dictator received an envelope with a sheet inside indicating the number of the paired recipient. On the sheet, each dictator wrote his or her own participant number and indicated a division of the endowment in increments of 10 (Barcelona) or 25 (Berkeley) cents. The experimenter then collected the envelopes and asked the dictators to complete the same one-page questionnaire as the recipients.

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, labeled "participate" and "don't participate." 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 a matched participant number) and wrote only their participant number on the sheet inside.[14] After collecting the envelopes, the experimenter

[12] At the time the sessions were conducted €1 was worth about $1.28.
[13] Instructions and materials for experiments at both locations are in 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.
[14] This procedure ensured that subjects participating and not participating wrote roughly the same amount on the sheets, thus preserving anonymity.

separated receivers matched with participating and non-participating dictators. For those matched with non-participating dictators, the experiment was concluded. Those paired with participating dictators received a description of the dictator game, and 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

Experiment 1 allows us to test whether a sorting option decreases the aggregate amount shared and to infer the relative frequencies of reluctant and willing sharers (Prediction 1). Figures 1A and 1B show the distributions of amounts shared, and the frequencies of subjects who opt out of the sharing environment, separately for Barcelona and Berkeley. In the No Sorting treatment, sharing behavior is comparable to 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 sorting has a strong effect. The average amount shared 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 and 50 percent in Berkeley, with the difference between locations being statistically significant in a non-parametric chi-squared test ($\chi^2(1) = 4.18$, $p = 0.04$).

Given the above behavior, we can estimate the relative frequencies of the three postulated social-preference 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, 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 33 percent of the total sample (39 percent in Barcelona and 26 percent in Berkeley).

Thus, consistent with earlier experimental results, when individuals are put in a sharing environment, the vast majority shares 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 and in a tobit estimation for the percentage of the endowment shared, as well as in a probit estimation for the frequency of sharing. (The distributions of amounts given in Figures 1A and 1B suggest the possibility of using a tobit model to account for the $0 corner solution.)[15] Standard errors are robust to heteroskedasticity and, in the linear regressions, adjusted for small-sample bias. Following MacKinnon and White (1985), we use the residual-variance estimator HC3, which approximates a jackknife estimator.[16] Correspondingly, in the tobit model, we perform a jackknife estimation, which produces slightly more conservative standard errors than the robust variance estimator. Table 1 shows that, under any of the estimation procedures, the sorting option significantly reduces sharing, and the effect is similar in Barcelona and Berkeley (insignificantly larger in Barcelona).

In both locations, the average amount shared *conditional* on entry in the sorting treatment is higher than the average amount shared when sorting is not possible (Barcelona: €2.41 vs.

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[15] One may consider alternative methods to account for the two-step decision of subjects – first, whether to participate or not, and second, if participating, how much to give – such as a Heckman sample bias correction method or hurdle models (see Mullahy 1986). However, such two-step correction models are not appropriate for our data or purposes. First, unlike the datasets typically analyzed with hurdle models (including previous work related to dictator games, e.g., Erkal et al. 2009), our data contains two distinct processes generating zero sharing: opting out of the sharing environment and opting in but sharing zero. A standard hurdle model would treat all of these zeros as determined by a single binary process, which is, at best, unnecessary in our data since we can directly observe one of the processes generating zero sharing (opting out or opting in). More likely, this approach is actually problematic since the focus of our experiments is to understand the implication of allowing for two (known) types of zeros for the composition of the sharing environment. Second, these kinds of models are primarily intended to correct for sample composition effects in the final estimates, while we are interested precisely in the effects of sample selection, operating through sorting, on the resulting distributions of sharing. In our case, we do not wish to correct for differences in how much those who opt out would give, and we do not seek to understand how sorting affects the underlying preferences of the population as a whole. Instead, we ask how sorting interacts with such preferences to produce environments that look very different than when there is no sorting. Moreover, while we often examine the first-stage decision of whether to share separately, as in a hurdle model, our analysis of the resulting sharing in the second stage focuses on conditional sharing.

[16] If we clustered by session, standard errors in this and in all other estimations are very similar and typically slightly smaller, though unlikely to be reliable given the few clusters (16 sessions in this table, fewer in other estimations).

€2.00; Berkeley: \$2.05 vs. \$1.87). However, these differences are not statistically significant.

A more detailed analysis of the distributions with and without sorting reveals that many of the givers affected by the sorting option are rather generous. To better assess the distributional effects, we estimate the effect of sorting separately for each of the giving-bins shown in Figures 1A and 1B: 0.00, (0.00-1.00], (1.00-2.00], (2.00-3.00], (3.00-4.00], (4.00-5.00], and more than 5.00, pooling the Berkeley and Barcelona data and neglecting currency differences. (The bins can be interpreted as percentages normalized by 10.) We estimate seven separate linear regressions, using bias-corrected robust standard errors (HC3), with an indicator for one of the bins as the independent variable and an indicator for Sorting as the main explanatory variable. We include zero giving after opting out in the 0.00 bin. To maximize efficiency, we include the full set of socio-economic variables elicited in the survey as well as site-controls for Berkeley, Pompeu Fabra, and Autonoma. (The resulting eleven individual characteristics are listed in Table 2.)

The coefficient estimates of the Sorting indicator as well as the 95% confidence intervals are displayed in Figure 1C. The figure confirms that sorting significantly increases the percentage of subjects giving zero, by 33.7%. For all other bins, the effect of sorting is negative or insignificant. The biggest and most significant shifts occur for the bins of giving between 3 and 4 (–10.0%, p-value = 0.021) and between 4 and 5 (–12.2%, p-value = 0.046). The reduction in giving between 0 and 1 is also relatively large and marginally significant (–9.3%, p-value = 0.083). The results are very similar under various alternative sample splits into different giving bins.[17] Quantile regressions confirm the same pattern, though they cannot be estimated for the lower quantiles due to the mass point at zero of the dependent variable.

These latter findings provide evidence that in the context of social behavior, sorting matters even for the subset of relatively generous givers. A large fraction of those who share, including those who are even very generous in the amount shared, do so reluctantly.

We gauge the importance and robustness of the observed sorting effect by relating it to other potential determinants of sharing. 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. For ease of interpretation, we use the simple linear framework. Table 2 presents OLS

[17] For example, attempting to equalize the size of bins (other than the large, non-separable bin of subjects giving \$0) so that they contain around 7-8% of subjects, we split the sample into 0.00, (0.00-1.00), 1.00, (1.00-2.00], (2.00-3.50], (3.50-4.50], and more than 4.50. Here, the largest and statistically significant reductions in sharing come from the bins of 1.00, (3.50-4.50], and more than 4.50.

regressions, again with bias-corrected robust standard errors, comparing the effect of sorting on the percentage shared (Column 1) to the effect of the demographics and self-reported preferences (Column 2). While the effect of sorting is highly significant and large (the amount shared decreases by 10 percent), none of the other dummy variables affect sharing to a similar extent. All coefficients are smaller in absolute size, and only one other dummy variable enters significantly. 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.[18] Overall, the opportunity to sort is significantly more important than any of the individual characteristics in determining sharing behavior.

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 0.07; 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 coefficients of partial 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 are calculated, for each predictor i, 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, and reflect the strength of association of each independent variable. Each individual characteristic explains between 0.01 and 0.18 of the remaining unexplained variance, but sorting explains 0.28. Thus, the sorting variable not only has an economically and statistically larger effect, but also is a more reliable predictor of sharing than any other variable.[20]

Overall, Experiment 1 provides evidence that the availability of sorting significantly lowers sharing. The effect of sorting is large and robust: across different subject populations, many

[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 $1 - (1 - R^2)\bullet[(N - 1)/(N - K - 1)]$, where N is the number of observations and K the number of predictors.
[20] 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, but also pervasive throughout all categorizations by such characteristics.

subjects opt out, resulting in a significantly lower aggregate amount shared 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. The large effect of sorting reflects that these subgroups react differentially to the costless sorting option: only reluctant sharers and (some) non-sharers take advantage of it and opt out.[21] Surprisingly, though, the largest and most significant reductions in giving occur among relative generous givers, who (reluctantly) share \$3-\$5 if sorting is not possible. This finding is a first indication that sorting may differentially affect different social preference types and that generosity in sharing does not necessarily imply high utility from sharing, which we explore in more detail in subsequent experiments.

IV. Experiment 2: Sorting and Reciprocity

Does the strong effect of sorting persist under alternative, stronger motives for sharing? The next experiment tests the role of sorting in the presence of positive reciprocity. Generous behavior is often observed most strongly in contexts where the other party has previously done something kind (Fehr and Gächter 2000; Sobel 2005; Falk and Fischbacher 2006; Cox, Friedman and Gjerstad 2007), suggesting that individual "willingness" to share might be greater in such instances.[22] If that is the case, sorting may have less of an effect when giving takes place in the context of positive reciprocity. That is, positive reciprocity may turn reluctant or non-sharers into willing sharers and, if so, we might expect a greater proportion of willing sharers, who enter the sharing environment (and share) even when they can avoid it.

To test whether positive reciprocity mitigates the effect of sorting, we conducted a reciprocity variant of the dictator game, a "double dictator game." In this experiment, we gave the recipients an initial choice of sharing \$2 with a matched partner in the other room. After finding out how much of the \$2 was shared, the matched partner plays a \$10 dictator game with the same person. Subjects did not know, initially, that their matched partners would subsequently become

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[21] Among those who chose to enter the dictator game, 21 percent (7 of 34) gave nothing to the recipient. This represents 8 percent of the total population, including those who opted out. Recall that the existence of such behavior is consistent with our interpretation of "non-sharers" as being indifferent between entering and not entering when $w = w'$. Alternatively, it is also consistent with the broader definition, including "reluctant non-sharers."

[22] Cox (2004) cautions that behavior classified as reciprocal may reflect other social preferences. See also Charness and Dufwenberg (2006), who suggest that what is sometimes interpreted as reciprocity may be a strengthened "guilt aversion" induced by the first-mover's kind act and its effect on expectations.

dictators over $10.[23] We conducted the experiment both without sorting and with sorting in the second-stage $10-dictator game. In order to test whether sorting affects sharing less after reciprocity has been induced, we compare the effect of sorting in dictator games without reciprocity (Experiment 1) with its effect in the double-dictator games (Experiment 2). If positive reciprocity turns reluctant sharers into willing sharers, there should be a smaller decrease in sharing (due to opting out) among those who receive an initial share of the $2 in the double-dictator game than there was in the (simple) dictator game.

A. Experimental Design

The sessions were conducted at UC Berkeley. The procedures were very similar to the ones in Experiment 1, but added a first stage in which those who would be recipients in the $10 dictator game could share $2 with the randomly-paired person who would later become their dictator over $10. The ultimate recipients were told to divide $2 between themselves and a randomly-paired participant in the other room, by circling one of two choices: keep $2 and give the paired participant $0; or keep $1 and send $1 to the other participant.[24] After participants made their choices, the experimenter gave the sheets 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 $10 dictator game were identical to the instructions used in Experiment 1 other than emphasizing that their decision (i.e., the $10 dictator game, including the decision to sort in case of the sorting treatment) was the only decision subjects in this room would make and the last decision that anybody (in either room) would make in this experiment.

We varied the availability of sorting in the $10 dictator game. 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

[23] Initial voluntary sharing can thus be interpreted as an act of kindness, rather than an attempt to induce reciprocal behavior, which avoids concerns about interpreting the dictators' reaction as reciprocity (Cox, 2004).
[24] The restriction to passing either $0 or $1 provides more power in the analysis of (ultimate) dictator behavior by reducing the analysis to two subgroups.

The data consists of 192 pairs (54 in the treatment without and 138 in the treatment with sorting). In 89 cases (46 percent), the first-mover shared one of the two dollars with the eventual dictator (no sorting: 26 cases [48 percent]; sorting: 63 cases [46 percent]). In order to make comparisons across identical subject populations, we compare the data to that from (single) dictator games conducted only in Berkeley in Experiment 1.

Figure 2 presents the average amounts shared per potential dictator, both with and without sorting. Subjects who opt out and, hence, share nothing are included as sharing $0. For comparison, the first set of bars presents the mean amounts shared in the (single) dictator games from Experiment 1 at Berkeley. The middle set of bars shows the mean amounts shared in the second stage of the double-dictator game when the first-mover shared $1, and the right set of bars presents the mean amounts shared in the double-dictator game when the first-mover shared $0.

Before we turn to the main results on the effect of sorting, we briefly discuss the "baseline" (dark-colored) bars from treatments without sorting, which provide evidence of reciprocity. The average amount shared by dictators who received $1 from the first mover is $2.39, while dictators who received nothing share on average $0.70. For comparison purposes, the amount shared by dictators in the No Reciprocity session from Experiment 1 is $2.00. Table 3 shows that only the negative-reciprocity effect is significant: We regress the percentages shared by dictators in the $10 game on a dummy for reciprocity treatments (i.e., for double-dictator games), a dummy for sorting, and the amount received from the first mover in case of a double-dictator game. The first column includes only the data from sessions without sorting. The significantly negative coefficient on Reciprocity indicates that, compared to a standard dictator game, dictators share 13% less if they received $0. Relative to this reduction, receiving $1 leads to significantly higher giving, by 17%. However, the difference between baseline sharing ($2) and sharing after receiving $1 ($2.39) is not significant (t-statistic = 0.84; p-value = 0.40). In other words, there is a significant negative-reciprocity effect and an insignificant positive-reciprocity effect, which appears consistent with previous experimental evidence (e.g., weak effects of positive reciprocity but strong "concern withdrawal" in Charness and Rabin, 2002).[25]

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[25] The analysis based on $10 assumes "narrow bracketing" in the second-stage dictator game. An alternative measure of reciprocity adds the (possibly shared) amount of $2 back to the analysis (see Cox, 2004): after sharing $1, recipients end up with $1+$2.39 = $3.39 out of $12 (28.3%) on average, and after sharing zero, with $2 + $0.70 = $2.70 (22.5%) on average, compared to $2 out of $10 (20%) in the single dictator game. Under this measure, positive reciprocity induces a marginally significant increase in giving (t-statistic = 1.88, p-value = 0.06), and negative reciprocity does not have a significant effect. Note that the lack of a significant negative-reciprocity effect reflects

Our main question, however, is: Does reciprocity lead to differential sorting effects? The results show that, as in Experiment 1, the sorting opportunity significantly decreases the amounts shared, even under positive reciprocity. Looking back at Figure 2, we see that average amount shared decreases in all cases (from \$2.00 to \$1.11, or by 44 percent in the single dictator game; from \$2.39 to \$1.71, or by 29 percent, when \$1 was received in the double-dictator game; and from \$0.70 to \$0.31, or by 56 percent when \$0 was received in the double-dictator game). As Columns 2 to 5 in Table 3 reveal, both under a simple OLS and under a tobit specifications, the significant decrease due to sorting 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 amounts shared in situations involving reciprocity as in those that involve non-reciprocal motives for sharing.

The same picture emerges if we consider the frequency of sharing, i.e., the fraction of subjects who share any positive amount. The probit regression in the final column of Table 3 uses as the dependent variable an indicator equal to one if a subject shared a positive amount. The coefficient on sorting is negative and significant, indicating that roughly 26 percent of sharers opt out if possible. However, the two interactions of Sorting with Reciprocity and with Amount Received are statistically insignificant. Thus, the statistical effect of sorting on sharing frequency appears very similar when sharing is motivated by reciprocity and when it is not.

Table 4 provides more insights in the sample composition of self-selected social-preference types. The upper half reports the sharing proportions in the single dictator games (No Reciprocity) and the double-dictator games (Reciprocity treatments), differentiating by amount received in the Reciprocity treatments. In all three columns, sorting reduces the frequency of sharing, by similar percentages under no reciprocity and positive reciprocity (26.3 percent and 23.4 percent) and by a lower percentage under negative reciprocity (10.8%). The lower percentage in the latter case reflects, at least partially, left-censoring – few share under negative reciprocity even without the sorting option. Overall, the effect of sorting on the frequency of sharing remains of similar magnitude as in Experiment 1, even after inducing positive reciprocity.

At the same time, however, there is a persistent effect of reciprocity. We see a lasting increase in the portion of willing sharers after positive reciprocity is induced. For example, com-

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censoring at \$2: Dictators cannot reduce the amount obtained by recipients below \$2 if those kept the initial \$2. The subsequent analysis on the effects of sorting is unchanged when \$12 is used as the relevant endowment.

paring the proportions of subjects who shared something in the treatments without a sorting option (first row) and with a sorting option (second row), positive reciprocity induces a persistent increase in the frequency of sharing even if there is sorting: by 24.1 percent without sorting and by 26.9 percent with sorting. This finding shows the strength of positive reciprocity. As a result, reluctant sharers make up a smaller fraction of all sharers in the setting with inducement of positive reciprocity than in the setting without reciprocity inducement.

The bottom half of Table 4 presents the estimated proportions of different social preference types . While the fraction of reluctant sharers is quite similar in treatments without reciprocity and with inducement of positive reciprocity, as discussed above, the proportion of willing sharers varies substantially. Not surprisingly, it is highest (65.1 percent) when the first mover did something kind and lowest (21.3 percent) when the first mover did something unkind. The proportion of non-sharers varies in a similarly intuitive manner – it is lowest (11.5 percent) when the first mover did something kind and highest (67.9 percent) when the first mover did something unkind. At the bottom of Table 4, we added a fourth type, which we dubbed "spiteful non-sharers" and whose presence attests to the strength of negative reciprocity. 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 spiteful non-sharers is much higher when the first mover shared nothing with the (ultimate) dictator (20 percent) than when the first mover shared $1 (3.2 percent) or did not have the option to share (6.6 percent). Using a non-parametric chi-square test, both the difference between the negative-reciprocity and the no-reciprocity case and the difference between the negative-reciprocity and the positive-reciprocity case are significant ($\chi^2(1) = 5.92$, $p = 0.02$ and $\chi^2(1) = 8.97$, $p < 0.01$ respectively). 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 significantly more likely to want to let the first mover know they could have shared.

The conclusion is that, although positive reciprocity leads to a persistent increase in the portion of willing sharers and negative reciprocity to a persistent increase in the portion of non-sharers, the power of sorting in altering the self-selected sample composition of social-preference types remains strong in all scenarios.

V. Experiment 3: Subsidized Sharing

In Experiments 1 and 2, we explored the basic effect of sorting for different social-preference types and motives. We next introduce price effects to test whether relative prices of the environment with and without sorting can be exploited to affect the differential sorting of individuals with different social preferences. Specifically, in order to test Predictions 2 and 3, we introduce subsidies ($w > w'$) and costs ($w < w'$) for entry into the sharing environment and analyze how the different social-preference types respond.

In Experiment 3, entry into the sharing environment becomes financially attractive ($w > w'$). Here, the main focus is on the reluctant sharers. Since willing sharers enter (and share) whenever $w \geq w'$, 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 $w > w'$. Reluctant sharers, instead, view entering and sharing as costly. Consequently, their responses should depend on the size of the subsidy. If their disutility from entry is proportional to the amount they share initially (relative sharing aversion), low subsidies will primarily attract those reluctant sharers who share the least. As a result, small subsidies may decrease the average amount shared by entrants.

In order to pin down each subject's type and generosity in sharing, we use a within-subject design: Each subject first plays the standard dictator game for $10 both without and with a sorting option, but no price differential ($w = w'$). The behavior observed in the two games determines each individual's type. This part of the experiment is a within-subject replication of the two treatments of Experiment 1. Then, subjects play the sorting treatment with increasing subsidies for entry into the dictator game. The amount available in the dictator game goes up while the amount available after opting out remains $10 ($w > w'$). The observed response to the increasing subsidies reveals how the sorting behavior of the different types interacts with prices.

The within-subject design also provides a measure of the generosity of different types. The between-subject treatment made it difficult to assess whether those who share willingly are more generous than those who share reluctantly since we could not track individuals across environments with and without sorting. Experiment 3 allows us to examine the amounts shared by all types in both the rounds without and with sorting.

In addition, Experiment 3 also explores the robustness of our findings to situations in which dictators are not anonymous but have to face the recipient–as in the many cases in which real-world dictators are directly confronted by someone requesting aid or donations.

A. Experimental Design

Experiment 3 took place at the Pittsburgh Experimental Economic Laboratory (PEEL).

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 treatment and 96 (48 dictators) in the Anonymity treatment.[26] Dictators were informed that they would make a series of decisions, with new instructions distributed prior to each decision. Payoffs would be based on one decision randomly selected at the end of the experiment. In each decision, the procedure replicated that of Experiment 1, other than the changing dictator game endowment.[27]

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 to 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 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, write down their own number, and indicate a division of 40 tokens. If they opened the 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. Subjects were told that if they chose to play the game *and if Decision 2 were selected to count*, then their paired recipient would be informed about the game and the allocation of tokens.

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 5 presents the endowment for each decision.[28]

At the end of each session, the experimenter randomly drew one of the decisions to

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[26] 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.

[27] Recipients were slightly worse off relative to Experiment 1: they had to fill in a series of questionnaires rather than one brief questionnaire, due to the fact that Experiment 3 consisted of several decisions and thus took longer.

[28] The number of decisions and endowments differ between Anonymity and No-Anonymity treatments since an initial pilot session revealed that, under Anonymity, almost all subjects play the dictator game once the endowment reaches about \$13. Since our goal was to explain differences in "re-entry" to the game, we fine-tuned the payoffs to measure such differences. We also decreased the number of rounds to allow the experiment to run more quickly.

count. Then either all recipients (if Decision 1 was drawn) or only those 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. They were then paid their earnings and participation fee. Unmatched participants were not informed and were paid $6.

Note that we did not counter-balance the order of the decisions across sessions. The purpose of the within-subject design was to compare the rates of re-entry, under increasing subsidies, within the group of reluctant sharers. In order to be comparable, all subjects had to be exposed to the exact same initial treatment. Among different possible "initial treatments," the above order (starting with the standard dictator game) permits comparisons of the first decision to standard dictator games. However, other "initial treatments" might also be of interest, and the design and results leave open whether similar classification and similar differential sharing survive different ordering.[29]

B. Results

The behavior in Decisions 1 and 2 strongly corroborates the findings of the 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 average amount shared per subject decreases substantially, from $2.68 without sorting (Decision 1) to $1.19 when sorting becomes possible (Decision 2).

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. As shown in Table 5, subjects share average amounts of $2.42 (Anonymity) and $2.92 (No Anonymity). Thus, as expected, the lack of anonymity produces slightly more sharing, but this difference is not statistically significant (t_{92} = 1.17). In the dictator game with sorting (Decision 2), only 25 percent share in the No-Anonymity treatment and 35 percent in the Anonymity treatment. The average amounts shared decrease, to $1.22 in Anonymity and to

[29] Note, however, that variation in the order of Decisions 1 and 2 turned out to be redundant since the outcomes closely replicate the results from the between-subjects 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 (Rick and Weber, 2010). In Appendix 2 we demonstrate that our findings hold in an alternate dataset (Broberg, et al, 2007), in which subjects first played a dictator game (Decision 1) and then stated a reservation price for exiting the game (the remaining decisions collapsed into one). Unlike in our data, where we directly observe re-entry into the sharing environment, re-entry in this other experiment is implicit in the reservation price.

$1.17 in No-Anonymity. Thus, the lack of anonymity makes opting out more attractive and reduces sharing slightly more, but the difference in amounts is again not significant ($t_{92} = 0.14$).[30]

The results of first two decisions in Experiment 3 demonstrate the robustness of the sorting effect, both when we relax anonymity and when we conduct a within-subject test.

Classification of Social-Preference Types

The within-subject design of Experiment 3 allows classification of individual subjects into the three posited types. Based on their first two decisions, 23 percent of the subjects are *non-sharers*—they share nothing in Decision 1 and either opt not to play or share nothing in Decision 2; 29 percent are *willing sharers*, who share both in Decision 1 and in Decision 2. The largest group, 41 percent, consists of *reluctant sharers*; they share in Decision 1 and opt out in Decision 2. These three categories account for 95 percent of the subjects.[31] Compared to Experiment 1, the proportions differ slightly: here, we have fewer non-sharers and more reluctant sharers. The distributions of types do not differ significantly by anonymity ($\chi^2(2) = 3.49$, $p = 0.18$).[32]

Given the above classification, we can determine which type – willing sharers or reluctant sharers – behaves most generously in the dictator game. Recall that the average amount shared conditional on entry in Experiment 1 was slightly higher under sorting, but the between-subject design did not allow calculation of the unconditional sharing of both types. Experiment 3 reveals that the average amount shared (in Decision 1 without sorting) is $4.46 for willing sharers ($4.22 under Anonymity and $4.77 under No Anonymity) and $3.10 for reluctant sharers ($3.20 under Anonymity and $3.04 under No Anonymity). The difference 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, are on average significantly more generous than those who share reluctantly.

Compositional Effect: Who Do Subsidies Attract into the Sharing Environment?

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[30] 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$).

[31] 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.

[32] 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 principal issue addressed by Experiment 3 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 subsidy ($w > w'$). In fact, 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.[33] 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.[34] As expected, they share very little when they enter, never more than \$0.17 on average for any dictator game endowment.

Most important is the behavior of reluctant sharers. Table 6 reports the marginal effects from probit estimations with subjects' decisions 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 control for the endowment in each round. (The results in the table are substantively unchanged when we also control for treatment, gender and decision.)

Column 1 explores the relative entry frequencies 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 than 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.

Most interestingly, Prediction 2 also 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 3A and 3B show the frequencies with which different subgroups of reluctant sharers re-enter the sharing environment as the subsidy increases across deci-

[33] As the percentage is below 100, our classification 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.

[34] 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, as we discussed above, some non-sharers experience disutility from sharing nothing in the game – not enough to lead them to share, but enough induce opting out and even foregoing a (low) subsidy.

sions. We split reluctant sharers into three groups, based on how much they shared in Decision 1. Consistent with Prediction 2, those who re-enter first are those who shared the least in Decision 1. For example, those who shared only $1.25 or less in Decision 1 all re-enter the game by Decision 4 in both treatments, 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. Thus, for the subgroup of reluctant sharers, generosity in sharing turns out to be a negative predictor of subjects' willingness to enter the sharing environment.

Columns 2 through 5 of Table 6 test the statistical significance of the relationship between entry and (initial) generosity in sharing. Column 2 shows that, taking willing sharers and reluctant sharers together, there is no relation between initial proportion shared and entry. Controlling for reluctance, however, we see that reluctant sharers enter at lower frequency and that there is a significantly negative relationship between entry and initial proportion shared (Column 3). The negative relationship appears to be driven by reluctant sharers who shared a lot initially (Column 4). That is, the lower entry rates of reluctant sharers in Column 3 are driven by generous reluctant sharers. As Column 5 shows more directly, there is a strong negative relationship between amount shared initially and entry into the sharing environment when using only the sample of reluctant sharers. The coefficient on Initial Proportion Shared indicates that for every additional percentage unit of the endowment shared in Decision 1, reluctant sharers are 0.82 percent less likely to enter the sharing environment. Thus a reluctant sharer who initially shared $5 is roughly 33 percent less likely to enter than one who shared $1.

Overall, we find strong support for Prediction 2: Subsidized entry by reluctant sharers is inversely proportional to the amount they share if the sharing environment is unavoidable.

How Large Are The Compositional Effects?

We have found that a subsidized sharing environment foremost attracts those least willing to share – non-sharers and the least generous reluctant sharers. The economic magnitude of the differential sorting effect is large. In our data, the average amount shared by those who enter the sharing environment *decreases*, relative to costless sorting, for a low subsidy. For example, in Decision 2, when sorting was costless, the average amount shared by those who entered the sharing environment was $2.88 ($2.68 with anonymity and $3.11 without), a small increase relative to the $2.68 ($2.42 with anonymity and $2.92 without) in Decision 1, when everyone was required to enter, similarly to what we observed for Experiment 1. But when there was a $1 subsidy for entering (Decision 4 with anonymity and Decision 3 without), the average amount shared by those who entered *decreased* to $2.22 ($1.92 with anonymity and $2.59 without). In fact, for every subsidy level below $6 (i.e., for all decisions with endowments below $16), the average amount shared conditional on entry is lower than when there is no subsidy.

A possible concern about the large raw effect of sorting under subsidies, however, is that it may reflect influences other than differential sorting by different social-preference types, such as the increasing endowment or round effects. In order to distinguish these and other unspecified confounds from the compositional effect of differential sorting, we estimate the effect of mere sorting under two hypothetical sharing rules that impose some consistency on sharing behavior. One hypothetical sharing rule is that individuals always share the same proportion of the endowment as they did in Decision 1 if they decide to enter. This assumption amounts to individuals proportionally sharing the subsidy. An alternative hypothetical sharing rule is that individuals always share the same absolute amount as in Decision 1, conditional on entry. This assumption amounts to individuals fully pocketing the subsidy. These two hypothetical sharing rules 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 accurately describes 74 percent of the actual choices made by those entering the sharing environment.

Table 7 estimates the magnitude of the compositional sorting effect under these two hypothetical sharing rules. For comparison, we show the estimations using the actual amount shared as the dependent variable in Columns 1 (OLS) and 2 (tobit). We then show the estimations using the predicted amount shared if individuals stick to the same proportion of the endowment as in Decision 1 (Columns 3 and 4) or to the same absolute amount as in Decision 1 (Column 5 and 6). (We use amounts rather than proportions as dependent variables to more clearly illustrate the monetary consequences of selective sorting). We regress each of these dependent variables on three explanatory variables: "Sorting Option," a binary variable equal to one in all rounds with a sorting option (Decisions 2-6); "Presence of Subsidy," a binary variable equal to one in all rounds with subsidies (Decisions 3-6); and "Amount of Subsidy ($0.50 to $10.00, see Table 5). Standard errors are clustered by subject.

The positive coefficient estimate of Sorting Option under the two hypothetical sharing rules indicates that those who always enter the sharing environment (willing sharers) share more on average than those who opt out when it is costless to do so (reluctant sharers). The coefficient of Presence of Subsidy is significantly negative and similar in magnitude under all three sharing rules, both in the OLS estimations and the tobit estimations. The OLS estimates indicate that the presence of a subsidy decreases conditional amounts shared by about \$0.90, controlling for the amount of the subsidy. However, each dollar of subsidy increases sharing amounts, 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 amounts shared in the dictator game. While the net negative effect in the first model (actual amount shared) 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.

Experiment 3 provides strong support for Prediction 2. Subsidizing the sharing environment creates entry, but foremost by those who share the least. As a result, the composition of the sharing environment, for low subsidies, yields lower sharing on average than under no subsidy.

VI. Experiment 4: Costly Sharing

Our experiments illustrate that, allowing for price variation, endogenous sorting can lead to drastically different sample compositions of social-preference types. So far, all results imply a negative effect of sorting on sharing. In every comparison, the aggregate amount shared has been lower when sorting was possible. In Experiment 3, we showed that subsidizing entry into the sharing environment has the additional effect of foremost attracting those who share the least, thus often leading to lower sharing on average.

However, comparing the amounts shared conditional on entry into the dictator game, we found slightly greater average amounts shared among those who enter if sorting is costless than in the baseline, across both Experiments 1 and 3 and all three locations. While none of the differences is statistically significant, the regularity suggests that sorting, if properly designed, may allow attracting those who share most: If a subsidized sharing environment attracts more but less generous types, does costly entry into the sharing environment attract fewer but more generous types? This would be attractive if a sharing environment has limited capacity for entry and, hence, the conditional sharing behavior of those few who enter is the relevant sharing outcome.

In Experiment 4, we explore whether it is possible to induce self-selection of high sharers by reversing the paradigm of Experiment 3. Instead of subsidizing entry into the sharing environment, we introduce a \$1 cost for entering the \$10 dictator game. That is, subjects could choose to play the dictator game with an endowment of w = \$10, or not to play the game, in which case they received a payment of w' = \$11 and the recipient remained uninformed about the game. As stated in Prediction 3, we expect to see low aggregate sharing, as the entry cost should prevent non-sharers and reluctant sharers from entering, as well as some willing sharers who do not receive high utility from sharing. But, by attracting only those willing sharers who place the highest value on acting generously, we should also observe high conditional sharing.

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 subjects received *w'* = $11 (instead of $10) if not playing the dictator game. Subjects were recruited in the same manner and from the same populations as for Experiments 1 (Berkeley) and 3 (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 54 pairs of participants across the Berkeley and Pittsburgh locations.[35] Figure 4 presents, in the second set of bars, the average amount shared per subject in the new costly entry (p = $1) treatment and the average amount shared conditional on entry into the dictator game. For comparison purposes, we also include the average amounts shared from Experiments 1 and 3 in the Berkeley and Pittsburgh baseline no-sorting dictator games on the left.

As expected, very few subjects choose to enter the sharing environment when there is a $1 price for doing so. Of the 54 potential dictators, only 12 (22 percent) choose to play the game. 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, 0.78 on average. Thus, not surprisingly, imposing a cost on entry into the sharing environment produces

[35] In one early session, a subject chose to enter and shared zero. When the experimenter later 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 clearly misunderstanding the instructions. This excluded a total of four participants from the analysis, including one who opted out of the sharing environment and two who shared positive amounts.

lower overall sharing relative to the baseline dictator game (\$2.21 vs. \$0.78). This difference is statistically significant in a non-parametric Wilcoxon rank-sum test ($z = 4.742$, $p < 0.001$).

However, conditional on entering the sharing environment, participants share significantly more than in the baseline dictator game (\$3.50 vs. \$2.21, $z = 2.12$, $p = 0.03$). Thus, as stated in Prediction 3, introducing a positive price for entering the dictator game attracts fewer people, but those who enter share more than the average person in the typical baseline dictator game. Of course, aggregate sharing is considerably lower when there is a price to entering the sharing environment, but those who enter share significantly more than the population as a whole. 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 at random from the entire population.[36]

VII. Conclusion

People regularly sort into and out of economic environments such as firms, markets, and institutions. Their sorting decisions are based on relative prices and governed by their personal preferences. Due to the endogenous selection, actual market outcomes can look very different from those we would expect if the entire population participated in a market. While a large literature in economics analyzes endogenous selection both theoretically and empirically, there has been less emphasis on its role in the context of social preferences. Much of the literature on social preferences builds on laboratory findings. But in the laboratory, 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, along with varying prices for sorting, into this laboratory environment and find that sorting significantly affects the resulting levels of sharing. Therefore, choosing subjects randomly and forcing them to play the dictator game might lead to a biased estimate of how much sharing

[36] One possible concern with this finding is that the high conditional sharing rests on the behavior of a small sample of participants. i.e., the 12 subjects who chose to pay \$1 for the opportunity to share. This is the result of the low entry rates (in one 10-person session, all subjects opted out). However, confidence in our results is strengthened by our novel reanalysis, in Appendix 2, of Broberg et al.'s (2007) data, which yields very similar results.

one is likely to observe outside the laboratory. Our key novel finding is 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. Our work provides an example of how the influence of sorting 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.

We also present several novel laboratory results. First, we demonstrate that the effect of sorting is robust, both in its economic magnitude and statistical strength, to "strengthening" the motivation for sharing. Even when we induce positive reciprocity among our dictators, sorting diminishes the extent to which individuals repay the first-movers kindness.

Second, we demonstrate that prices strongly affect sorting decisions. The price effects interact with different types of social preferences to dramatically alter the composition of the sorting environment. Subsidizing entry into the sharing environment attracts people who share little, relative to when there is no subsidy, thereby leading to *lower* (conditional) average amounts shared in the sharing environment. Conversely, making entry into a sharing environment costly attracts people who share more than the population average, leading to a greater (conditional) average amount shared in the environment than is representative of the population. While previous research by Broberg, et al. (2007), explored the relationship between prices and sorting decisions, their paper stops short of considering the compositional effects of costs and subsidies, as we do here. In Appendix 2, we also demonstrate these effects in their experimental data, which was collected using different procedures but never previously analyzed in light of our Predictions 2 and 3. This confirmation of our results highlights that the strong compositional effects of sorting and prices we find here are not unique to our experimental procedures.

Finally, an important caveat merits repeating. While we find that subsidized entry decreases average conditional sharing, the total aggregate amounts shared increases; and the opposite holds if entry is costly. But, in the many situations where a sharing environment has limited capacity for entry, average *conditional* sharing might be the relevant measure, since this is the only behavior one will ultimately observe in the environment of interest. Our results show that prices determine who sorts in and how much giving ultimately results.

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Table 1 – Effect of Sorting on Sharing
(Experiment 1)

Model:	**OLS**		**Tobit**		**Probit**	
Dependent variable:	*Proportion Shared*		*Proportion Shared*		*Shared Something*	
	(1)	(2)	(3)	(4)	(5)	(6)
Sorting	-0.102** (0.029)	-0.079* (0.043)	-0.234*** (0.0578)	-0.173** (0.078)	-0.309*** (0.073)	-0.253** (0.102)
Barcelona		-0.013 (0.045)		-0.024 (0.074)		-0.041 (0.112)
Sorting X Barcelona		-0.050 (0.058)		-0.145 (0.124)		-0.139 (0.154)
Observations	168	168	168	168	168	168
(pseudo) R^2	0.070	0.084	0.086	0.107	0.070	0.082

Notes: Sorting is a dummy equal to one in treatments where subjects can opt out. The dependent variable Proportion Shared is zero for subjects who opted out. The dependent variable Shared Something is a dummy equal to 1 if the subject shared a positive amount. The tobit model accounts for 89 observations being left-censored at zero. The probit model shows marginal effects. Robust standard are in parentheses (with bias-correction (HC3) in the linear case, see MacKinnon and White (1985)) and are calculated using jackknife estimation for the tobit model.
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 2. Determinants of Sharing (Experiment 1)

	(1)	(2)	(3)	Coefficients of Partial Determination
Sorting	-1.025***		-1.042***	
	(0.291)		(0.329)	0.28
Gender: Female		-0.096	-0.128	
		(0.332)	(0.321)	0.04
Ethnicity: Catalan		0.285	0.415	
		(0.445)	(0.442)	0.07
Ethnicity: Asian		0.162	0.011	
		(0.576)	(0.571)	0.03
Ethnicity: White		-0.750	-0.738	
		(0.571)	(0.535)	0.07
Socio-economic status: middle class		-0.050	0.064	
		(0.394)	(0.376)	0.06
upper to middle class		-0.058	-0.055	
		(0.431)	(0.429)	0.01
Age group: Graduate Student		-0.101	-0.117	
		(0.530)	(0.530)	0.01
Major: Business or Economics		-0.401	-0.361	
		(0.367)	(0.366)	0.14
University: Berkeley		0.108	0.209	
		(0.673)	(0.627)	0.02
University: Pompeu Fabra		-0.650	-0.730	
		(0.502)	(0.492)	0.12
Siblings: 0 siblings		0.535	0.365	
		(0.669)	(0.708)	0.04
1 sibling		-0.779**	-0.832**	
		(0.372)	(0.354)	0.18
3 or more siblings		-0.472	-0.581	
		(0.570)	(0.548)	0.11
Donation (during past year)		-0.465	-0.325	
		(0.340)	(0.319)	0.10
Risk-seeking		0.426	0.294	
		(0.342)	(0.343)	0.08
Constant	1.942***	2.139***	2.639***	
	(0.221)	(0.756)	(0.699)	
Observations	168	166	166	
(Adjusted) R-Square	0.07	0.03	0.10	

Notes: OLS regressions with Total Amount Shared (out of €10.00 endowment) as the dependent variable. Bias-corrected robust standard errors (HC3) in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3. Effect of Sorting on Sharing with Reciprocity
(Experiments 1 and 2, Berkeley data)

Model:	***OLS***			***Tobit***		***Probit***
Dependent variable:	*Proportion Shared*					*Shared Some-thing*
Sample:	***Baseline (No Sorting)***	***All Data***				
	(1)	(2)	(3)	(4)	(5)	(6)
Reciprocity Treatment	-0.130*** (0.045)	-0.096*** (0.021)	-0.130*** (0.045)	-0.222*** (0.052)	-0.259*** (0.091)	-0.323*** (0.114)
Amount Received	0.170*** (0.051)	0.148*** (0.025)	0.170*** (0.051)	0.348*** (0.052)	0.348*** (0.091)	0.575*** (0.101)
Sorting		-0.068*** (0.023)	-0.089** (0.037)	-0.145*** (0.040)	-0.178*** (0.064)	-0.263*** (0.090)
Sorting X Reciprocity Treatment			0.050 (0.051)		0.058 (0.113)	0.135 (0.150)
Sorting X Amount Received			-0.029 (0.058)		-0.001 (0.111)	-0.185 (0.172)
Constant	0.200*** (0.030)	0.187*** (0.023)	0.200*** (0.030)	0.111*** (0.034)	0.130*** (0.048)	
Observations	99	313	313	313	313	313
(pseudo) R^2	0.113	0.136	0.138	0.189	0.191	0.139

Notes: Reprocity Treatment is an indicator for double-dictator games. Amount Received is the amount shared in the first-stage (small) dicator game ($0 or $1). Sorting is a dummy equal to one in treatments where subjects can opt out. The dependent variable Proportion Shared is zero for subjects who opted out. The dependent variable Shared Something is a dummy equal to 1 if the subject shared a positive amount. The tobit model accounts for 89 observations being left-censored at zero. The probit model shows marginal effects. Robust standard are in parentheses (with bias-correction (HC3) in the linear case, see MacKinnon and White (1985)) and are calculated using jackknife estimation for the tobit model.
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 4. Distribution of Sharing Types without and with Reciprocity
(Experiments 1 and 2, Berkeley data)

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

Table 5. Average Amounts Shared under Subsidized Sharing
(Experiment 3)

		Anonymity			No Anonymity		
Sorting Option ($10)	**Endowment in Dictator Game**	Decision	Average amount (percent) shared	Percent entering	Decision	Average amount (percent) shared	Percent entering
No	$10.00 (40 tokens)	1	$2.42 (24.2%)	100%	1	$2.92 (29.2%)	100%
Yes	$10.00 (40 tokens)	2	$1.22 (12.2%)	46%	2	$1.17 (11.7%)	38%
Yes	$10.50 (42 tokens)	3	$1.34 (12.8%)	57%			
Yes	$11.00 (44 tokens)	4	$1.42 (12.9%)	74%	3	$1.51 (13.7%)	58%
Yes	$12.00 (48 tokens)	5	$1.52 (12.7%)	76%			
Yes	$13.00 (52 tokens)				4	$2.07 (15.9%)	73%
Yes	$16.00 (64 tokens)				5	$3.21 (20%)	90%
Yes	$20.00 (80 tokens)				6	$4.53 (22.7%)	100%
Number of sessions			6			6	
Number of subjects (dictators)			92 (46)			96 (48)	

Notes: All averages are unconditional, i.e., subjects opted out are included (as sharing $0).

Table 6. Determinants of Entry into Sharing Environment
(Experiment 3, excluding Decisions 1 and 2)

Sample:	***All Classified Subj.***	***Willing and Reluctant Sharers***			***Reluctant Sharers***
	(1)	(2)	(3)	(4)	(5)
Initial Proportion Shared		0.003 (0.175)	-0.502*** (0.182)	0.282 (0.417)	-0.823*** (0.265)
Non-sharers	-0.154** (0.077)				
Reluctant Sharers	-0.346*** (0.060)		-0.350*** (0.052)	-0.025 (0.196)	
Initial Prop. Shared X Reluctant Sharers				-0.882* (0.460)	
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

Notes: The table reports marginal effects of probit estimations. The dependent variable is an indicator equal to one if the subject shared any positive amount. Robust standard errors are in parentheses.
* - p < 0.1; ** - p < 0.05; *** p < 0.01

Table 7. Effects of Sorting and Subsidies on Conditional Sharing: Actual and Hypothetical Sharing Rules
(Experiment 3)

Dependent variable:	*Actual amount shared*		*Predicted amount shared (fixed proportion)*		*Predicted amount shared (fixed amount)*	
	OLS	Tobit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)
Sorting Option (Decisions 2 – 6)	0.200 (0.293)	0.153 (0.450)	0.674*** (0.253)	0815** (0.350)	0.674*** (0.253)	0.779** (0.320)
Presence of Subsidy (Decisions 3 – 6)	-0.941*** (0.263)	-1.134*** (0.374)	-0.955*** (0.270)	-1.214*** (0.377)	-0.894*** (0.253)	-1.096*** (0.329)
Amount of Subsidy	0.260*** (0.065)	0.288*** (0.082)	0.350*** (0.065)	0.391*** (0.078)	0.052 (0.041)	0.075 (0.054)
Constant	2.678*** (0.214)	2.122*** (0.341)	2.678*** (0.214)	2.156*** (0.351)	2.678*** (0.214)	2.305*** (0.313)
Observations	382	382	382	382	382	382
(pseudo) R^2	0.072	0.011	0.143	0.024	0.015	0.003

Notes: Robust standard (errors clustered by subject) are in parentheses. The tobit model accounts for 114 observations being left-censored at zero in column (2) and 96 observations in columns (4) and (6).
* - p < 0.1; ** - p < 0.05; *** p < 0.01

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

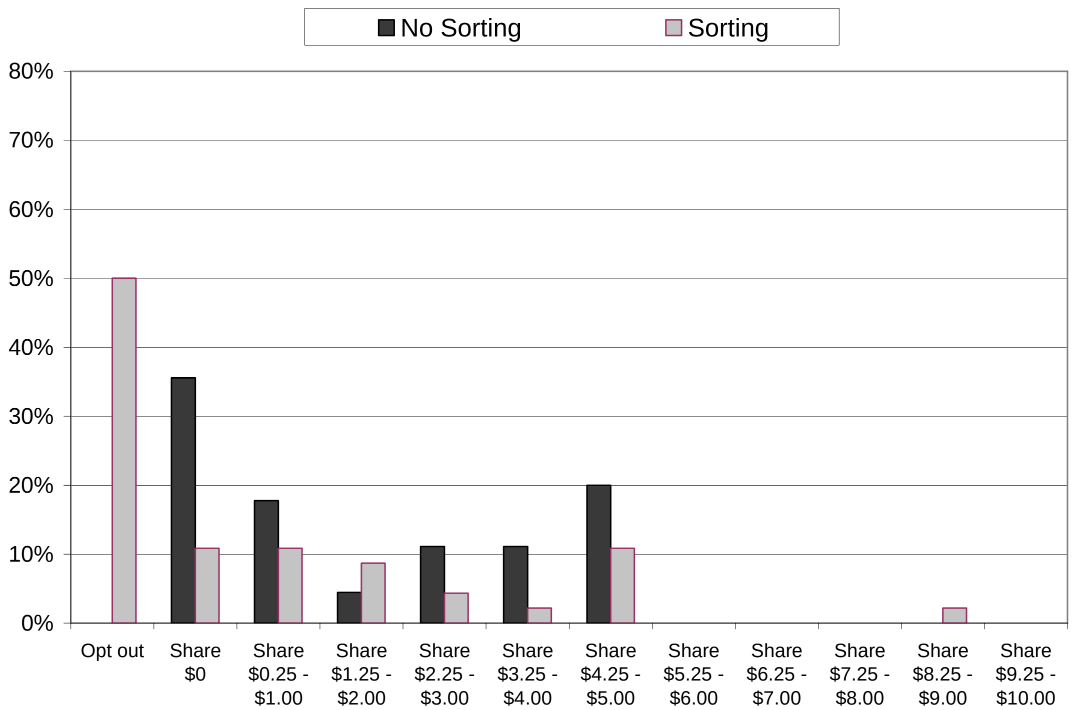


Figure 1B. Distributions of Amounts Shared
(Experiment 1, Barcelona)

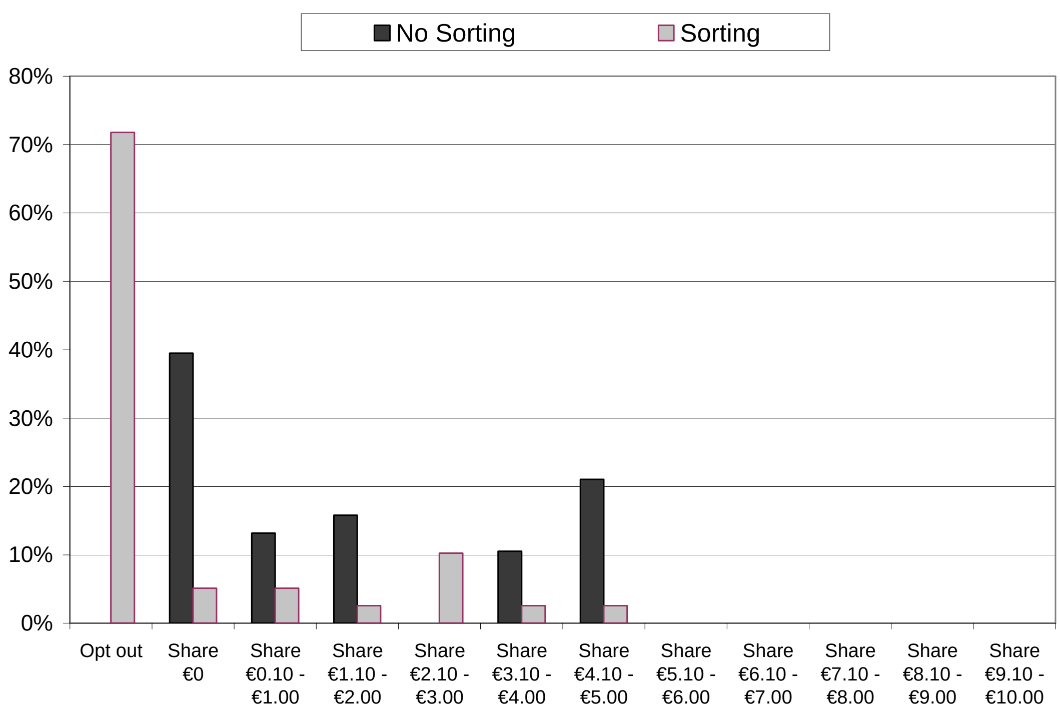
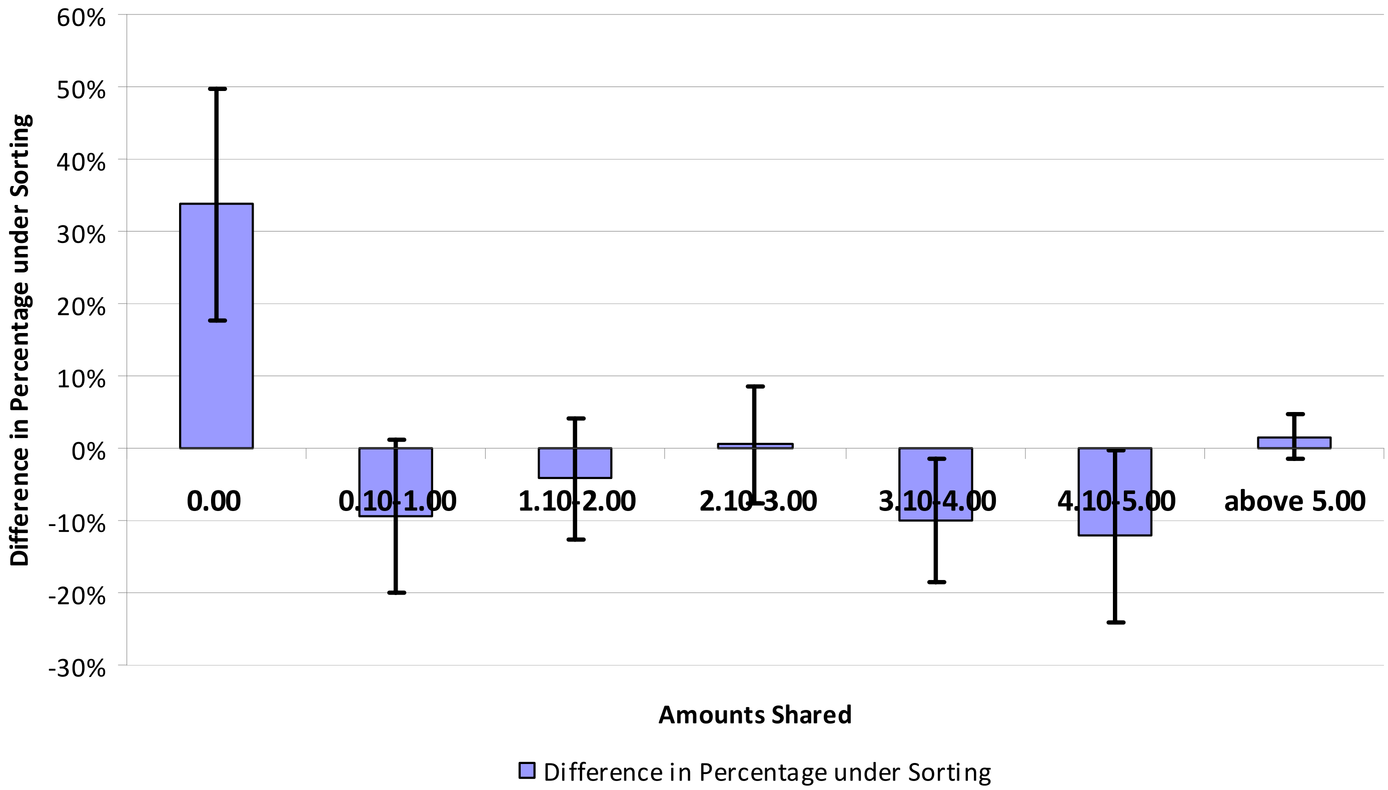
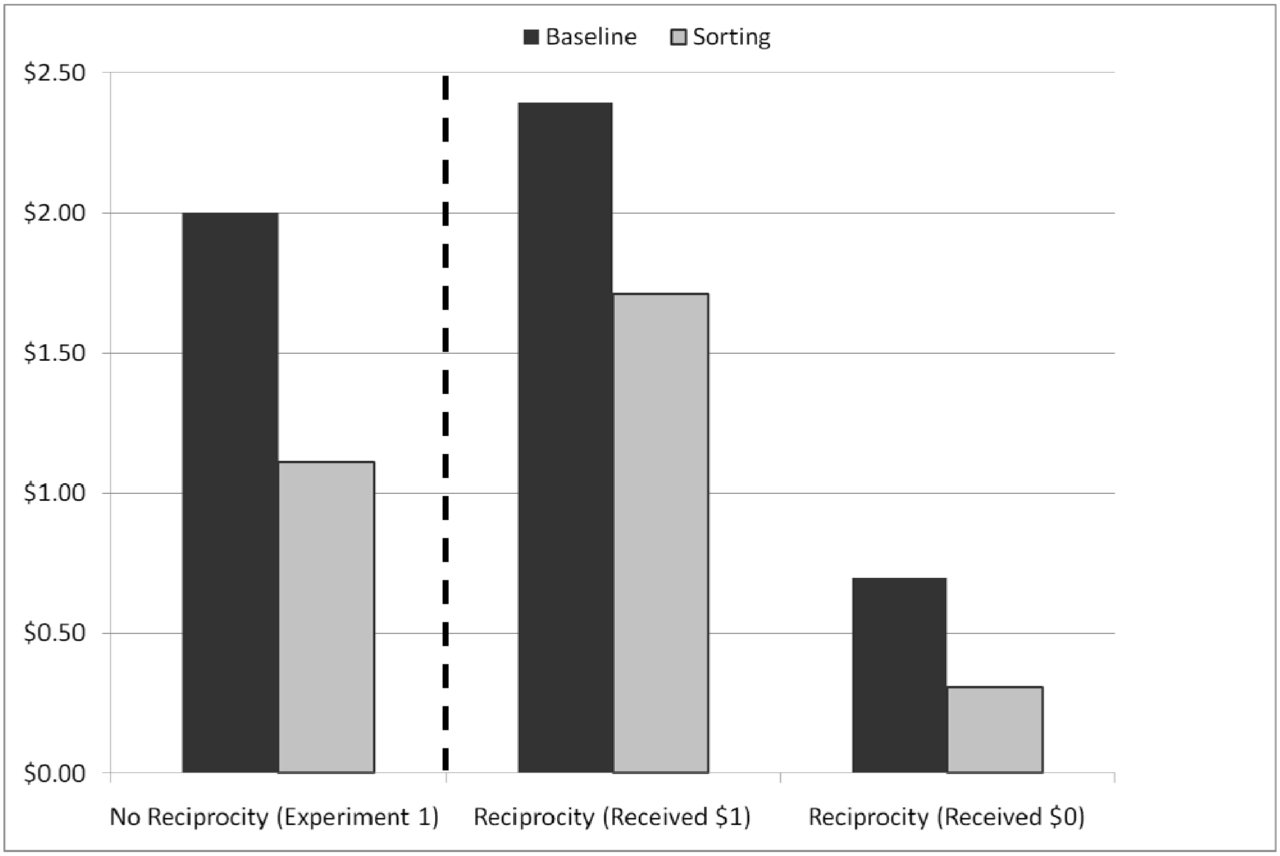


Figure 1C. Distribution of Difference in Amounts Shared
(Experiment 1, Berkeley and Barcelona)

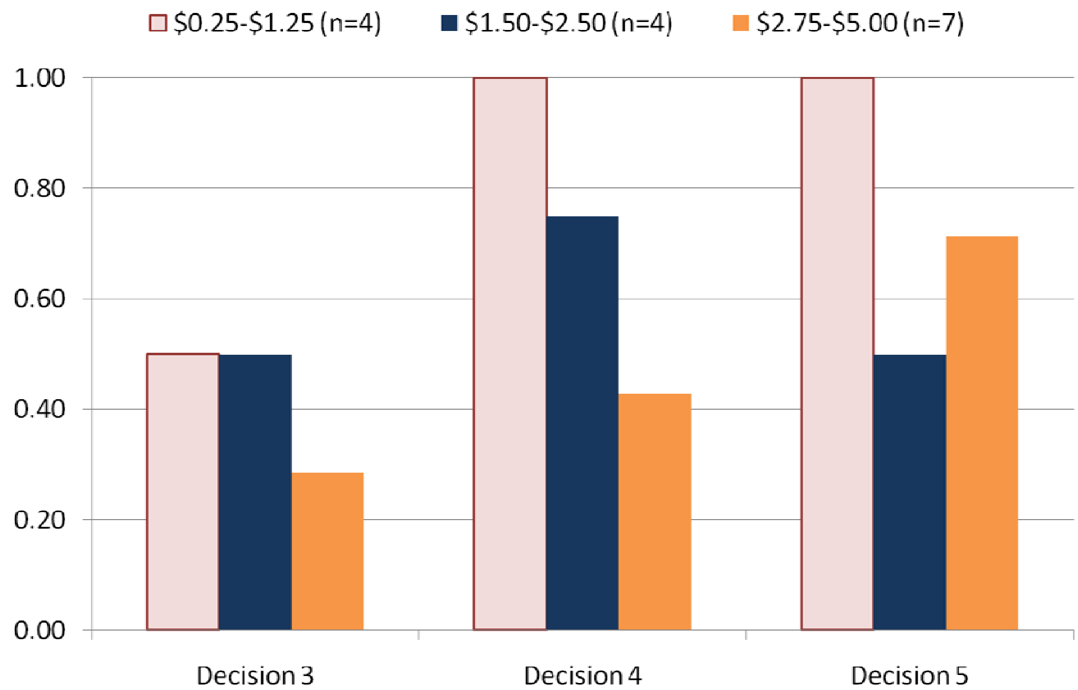


Notes: Coefficient estimates of the Sorting indicator and confidence intervals are from linear regressions of indicators for giving bins on the treatment dummy (Sorting) and full set of socio-demographic control variables (see Table 2.) Confidence intervals use bias-corrected robust standard errors (HC_3).

<u>Figure 2. Average Amounts Shared without and with Reciprocity</u>
(Experiment 2; No-Reciprocity data from Experiment 1 Berkeley sessions)



<u>Figure 3A. Proportion of Reluctant Sharers Choosing to Enter</u>
by Decision and Initial Amount Shared (<u>Anonymity</u>)



<u>Figure 3B. Proportion of Reluctant Sharers Choosing to Enter</u>
by Decision and Initial Amount Shared (<u>No Anonymity</u>)

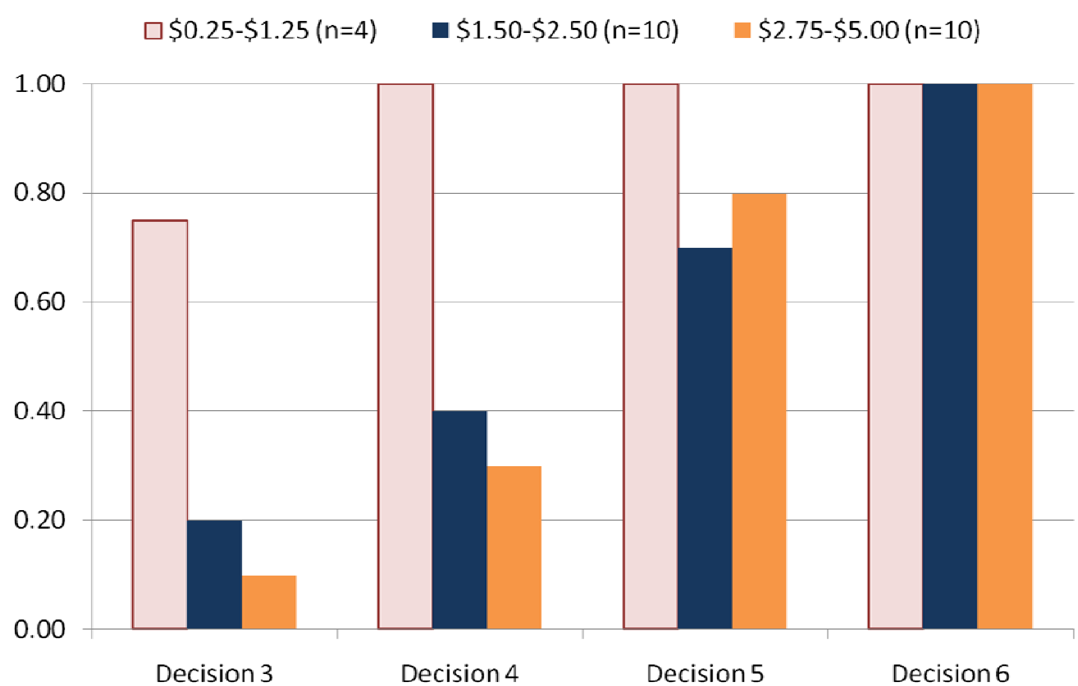
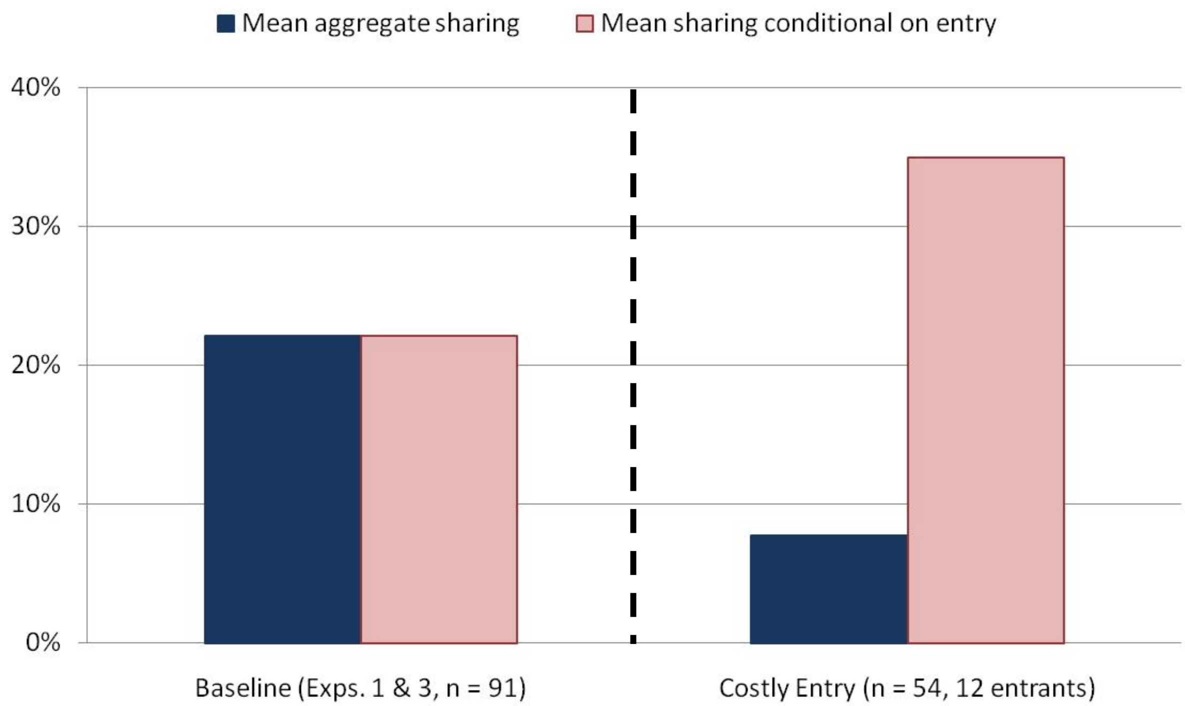
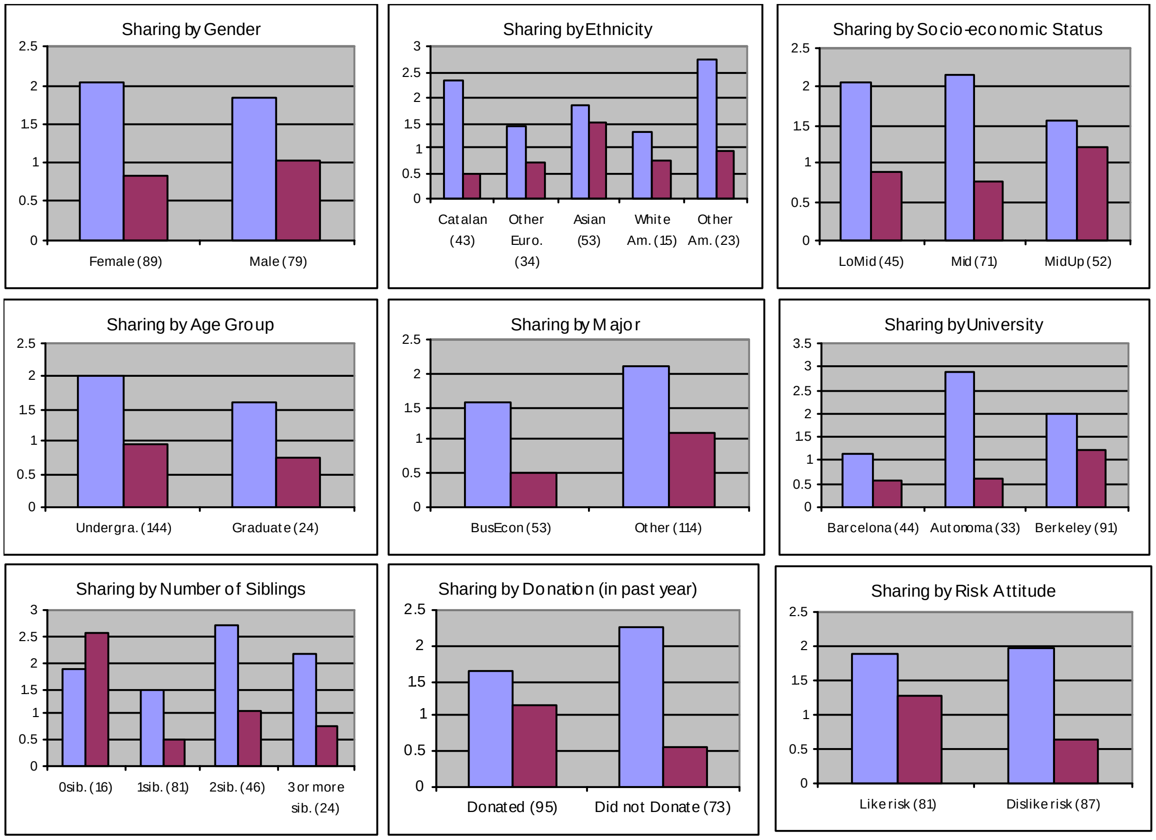


Figure 4. Average Proportions of Endowment Shared: Costly Entry
(Includes Berkeley and Pittsburgh Baseline Data from Experiments 1 and 3)



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.



Appendix 1: Cobb-Douglas Specification of Social Preferences

Appendix 1 provides more details of the theoretical framework and provides one concrete example, a modified Cobb-Douglas utility function, for which relative sharing aversion holds. As in the main text, consider an agent who is endowed with an amount w, which she has to divide into payoffs for herself (x) and for another agent (y), as in the classic dictator game:

(1) $x + y = w$.

We allow utility to depend on the payoffs x and y as well as on the sharing environment:

(2) $U = U(D, x, y)$

where D is a dummy variable equal to 1 if the environment allows sharing and 0 if the allocation of w is exogenously determined. That is, under $D = 1$ the agent decides how to split w with the other person. Under $D = 0$ the agent has no influence on how w is allocated. When $D = 0$, the individual is precluded from sharing and thus $y = 0$. It is possible that even with the opportunity to share (i.e. when $D = 1$), $y = 0$, but this depends on individual choice. We assume that utility is increasing in the endowment. Using equation (1), the utility function can be rewritten as $U = U(D, x, w - x)$.

We characterize an individual's propensity to share in the sharing environment with the parameter a. The agent allocates $y = aw$ to the other person and $x = (1-a)w$ to herself. Individuals who choose $a = 0$ are denoted as *non-sharers*. Individuals with $a > 0$ are either *willing sharers* or *reluctant sharers*. *Willing sharers* would pay to be in the sharing environment. *Reluctant sharers* would pay to avoid the sharing environment. That is, holding the endowment constant, willing sharers prefer the sharing environment and reluctant sharers prefer the non-sharing one. However, both kinds of agents behave identically in the sharing environment if they have the same sharing propensity a.

Now, suppose that the endowments in the two environments differ. In the sharing environment the individual is given w to divide, while in the non-sharing environment she receives a fixed amount w', which cannot be shared. We parameterize an individual's willingness to pay by the endowment in the sharing environment $\hat{w}$ at which she is indifferent between entering the sharing environment and opting out, given a fixed endowment w' outside the sharing environment. The higher $\hat{w}$ is, the larger is the individual's disutility from being in the sharing environment. *Willing sharers* have $\hat{w} < w'$ because they are willing to pay for the opportunity to share. *Reluctant sharers* have $\hat{w} > w'$ because they are willing to pay to avoid that environment altogether. *Non-sharers* are not willing to pay to avoid the sharing environment ($\hat{w} = w'$).[1] The premium $\hat{w} - w'$ that an individual is willing to pay to avoid the sharing environment relative to an outside option of w' is implicitly defined by

(3) $U(1, x', \hat{w} - x') = U(0, w', 0)$

where x' is the own payoff chosen in a sharing environment with allocation $\hat{w}$. We define

(4) $\lambda(w') = \hat{w}/w'$.

[1] This is a simplifying assumption, not a general statement (but also not required for our analysis). The model can be generalized to allow a more subtle distinction of types. For example, agents who share nothing in the sharing environment may still pay something to avoid being put in that environment and, hence, have $\hat{w} > w'$. Other agents may get *some* utility from sharing but feel compelled to share too much in a sharing environment. As a result, such agents avoid the sharing situation (and thus share nothing) despite their preference for sharing. These agents have $\hat{w} > w'$. Additionally, individuals might be reluctant to share over some ranges of w and willing to share in other ranges. For brevity and simplicity we distinguish only the three basic types, based on their observable ("net") sharing decision.

Willing sharers have a $\lambda < 1$, and *reluctant sharers* have a $\lambda > 1$.

Special case: Modified Cobb-Douglas utility function

We consider a branched Cobb-Douglas utility function which allows for individuals to have the opportunity to share (or not). Its value depends on *x*, *y*, *D*, and a parameter λ^a, which we describe below, as follows:

(5) $$U(D,x,y) = x^{(1-a)[D+(1-D)/(1-a)]}[y^a D + 1 - D][D + (1-D)\lambda^a (1-a)^{(1-a)} a^a]$$

with $a \in [0;1]$. This seemingly complex function is nothing more than the summary of rather simple preferences under $D = 1$ and under $D = 0$. When $D = 1$, (5) becomes $U(1,x,y) = x^{(1-a)} y^a$, which is the standard Cobb-Douglas formulation. The optima of *x* and *y*, given this utility function, are $x^* = (1-a)w$ and $y^* = aw$ so that

(6) $$U(1,x^*,y^*) = [(1-a)w]^{(1-a)}[aw]^a = (1-a)^{1-a} a^a w$$

When $D = 0$, (5) becomes $U(0,x,y) = \lambda^a (1-a)^{(1-a)} a^a x$ and, with $x = w'$ and $y = 0$,

(7) $$U(0,w',0) = \lambda^a (1-a)^{1-a} a^a w'$$

This is also Cobb-Douglas, with one variable where the coefficient on *x* is 1. In this specification $\lambda(w') = \lambda^a$, which we obtain after solving for $\hat{w}/w'$ in expression (3). *Willing sharers* have $\lambda(w') = \lambda^a < 1$, and thus $\lambda < 1$; *reluctant sharers* have $\lambda(w') = \lambda^a > 1$, and thus $\lambda > 1$. Note further that, under $D = 1$, the allocation of *w* to *x* and *y* does not depend on λ. Agents with equal *a* share the same amount *aw*, when placed into a sharing environment, though those with $\lambda > 1$ are *reluctant sharers* and those with $\lambda < 1$ are *willing sharers*.

Proposition 1 (cf. Prediction 2): *The lowest endowment $\hat{w}$, at which reluctant sharers enter the sharing environment increases in a.*

Proof of Proposition 1. The endowment $\hat{w}$ at which agents are indifferent between the two environments is defined by (3). Comparing (6) to (7), this implies

(8) $$w' = \lambda^a \hat{w}.$$

Differentiating (8) with respect to *a* shows that $\hat{w}$ is increasing in *a*.

Appendix 2 – A re-examination of Broberg et al's data

We reanalyze data from Broberg et al.s' (2007) experiment, in light of our three predictions. Their data allows an analysis of the likely composition of the sharing environment, based on individuals' preferences and on whether there is a cost or subsidy to entering the environment. Such an analysis, however, is not reported in their paper. Therefore, our re-examination of their data, in conjunction with the results we report in our paper, yield complementary evidence supporting our novel predictions (i.e., Predictions 2 and 3). Moreover, since the procedures used in their experiments differ substantially from ours and their experiments were conducted in Sweden, this section demonstrates the robustness of our findings to alternative procedures and populations.

Description of Broberg et al.'s experiment

In Broberg et al.'s experiment, 119 subjects participated in a two-part experiment. In the first, part, each subject played a dictator game in which they allocated SEK 100 (w, approximately \$14) between themselves and another anonymous student at the same university in Stockholm. We refer to the allocation to the other player as a. In the second part, subjects indicated a reservation price to exit the game (p^*). Following Becker et al.'s (1964) mechanism for incentive-compatible value elicitation, the experimenter then randomly drew a price (p). If the randomly-drawn price was equal to or higher than the elicited price ($p \geq p^*$), the subject received p and did not play the dictator game. Otherwise, the subject played the dictator game.

Using our classification of social-preference types, we can classify subjects as either non-sharers ($a = 0$), willing sharers ($a > 0$, $p^* > w$), or reluctant sharers ($a > 0$, $p^* < w$). In what follows, we assume that those who are indifferent at a given price will opt out.

Prediction 1

Prediction 1 states that the introduction of a costless sorting option decreases the aggregate amount shared. This is equivalent to positing the presence of reluctant sharers in the population, since they will always share less when costless sorting is available.

In Broberg et al.'s data, the 119 subjects share on average SEK 27.15 when there is no sorting option, but aggregate sharing declines to SEK 5.16 per subject when sorting is available. This difference is highly statistically significant ($t_{118} = 10.50$, $p < 0.001$). Thus costless sorting leads to significantly less sharing, as we predict and also find in our experiments.

Interestingly, and as we also find in our experiments, sharing conditional on entering the sharing environment is higher for those who choose to enter when entry is costless, i.e., willing sharers (SEK 34.11), than it is for the population as a whole.

Prediction 2

Prediction 2 states that introducing a subsidy increases aggregate sharing because more reluctant sharers are attracted into the sharing environment. At the same time, the subsidy primarily attract non-sharers and those reluctant sharers who share the least. Thus, under relative sharing aversion, low subsidies lead to lower average sharing among those who enter, than when there is no subsidy (we find this to be the case in our Experiment 3).

Table A2.1 below reports the amount shared, by those who would enter the sharing environment based on their reservation price, for different values of the outside option at or below the value of the endowment ($w' \leq w$). Entry is subsidized whenever the outside option yields a smaller payoff than the endowment ($w' < w$).

As the table reveals, very few enter when there is no subsidy (15%), but those who do enter share a large amount (34.11). The introduction of a small subsidy (w' = 90) attracts more people but decreases average sharing among those who enter. The relationship in the table is clearly the one we predicted – those who are first attracted by subsidies are those who share smaller amounts. For example, the 26 people who re-enter when w' = 90, share only 13.12 on average. More generally, we expect a negative correlation between reservation price and amount shared, but *only for those who opt out when sorting is costless* ($w = w'$). (Broberg et al. report only the correlation for their entire sample, which is 0.069 and statistically insignificant.) We find that the correlation for those with reservation prices of 100 or below is negative and statistically significant, as we predicted (-0.35, $p < 0.001$).

Table A2.1. Amount shared conditional on entry when entry into sharing environment is subsidized

Value of outside option (w')	Number (percentage) choosing to enter	Mean amount shared by entrants
No sorting	119 (100%)	27.15
100	18 (15%)	34.11
90	44 (37%)	21.70
80	53 (45%)	20.85
70	64 (54%)	22.89
60	73 (61%)	24.12
50	79 (66%)	24.63
40	109 (92%)	26.22
30	112 (94%)	27.19
20	112 (94%)	27.19
10	115 (97%)	27.27

Prediction 3

Finally, our third prediction deals with costly entry into the sharing environment. Here, we predict that fewer people will enter, but that those who enter will share more than the population as a whole.

We first consider an entry cost of 10 percent of the endowment (SEK 10), corresponding to the design of our Experiment 4. As shown in Figure A2.1 below, the results are virtually identical to the results in our paper; cf. Figure 4 in our paper.

More generally, we explore the extent to which Prediction 3 holds for alternate entry costs. Table A2.2 below reports outcomes as entry becomes costly ($w < w'$). As we predicted, costly entry leads to significantly fewer people opting in to the sharing environment. But those who enter share large amounts. For example, those who are willing to forego SEK 150 in order to play the SEK 100 dictator game share 49 percent of the endowment, which is higher than for any other subset of the population. This is consistent with the fact that, *for willing sharers*, there is a positive and statistically significant correlation between amount shared and reservation price (0.75, $p = 0.001$).

Figure A2.1: Effects of 10 percent entry cost on overall sharing and sharing conditional on entry (Broberg, et al. (2007), n = 119)

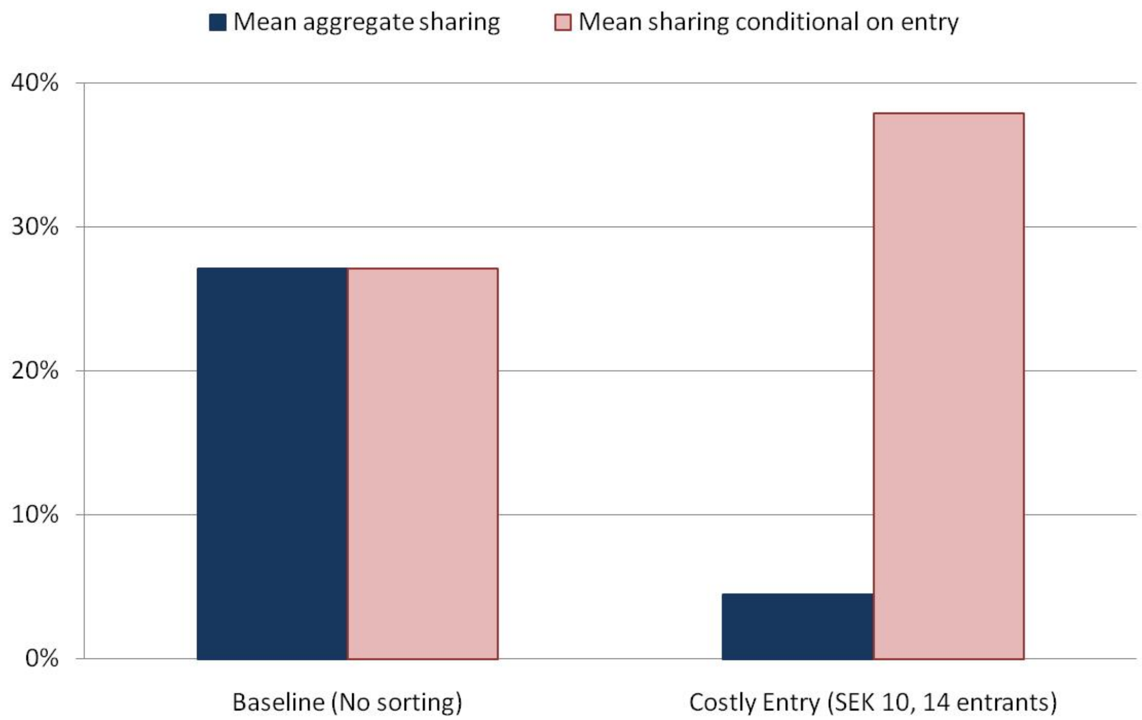


Table A2.2. Amount shared conditional on entry when entry into sharing environment is costly

Value of outside option (*w'*)	Number (percentage) choosing to enter	Mean amount shared by entrants
No sorting	119 (100%)	27.15
100	18 (15%)	34.11
110	14 (12%)	37.79
120	12 (10%)	43.25
130	12 (10%)	43.25
140	12 (10%)	43.25
150	6 (5%)	49.00

Appendix 3: **Sample Instructions for Experiment 1 (Between-Subjects Design)**

The text in brackets and in italics appears only in treatments with sorting option.

General Instructions

Thank you for attending the experiment. The purpose of this session is to study how people make decisions. During the session, you are not permitted to talk or communicate with the other participants. If you have a question, please raise your hand and I will come to answer it.

During the session you will earn money. Everyone will receive €5 for their participation, which will be the minimum compensation for everyone. In addition, there exists a possibility that some may earn more money. At the end of the session the quantity that you have earned will be paid to you in cash. The payments are confidential; we will not inform any of the other participants of the quantity that you earn.

In a moment, you will receive an envelope. Once everyone has received an envelope, you may open it and you will see a card with a number. This is your identification number for the experiment. After looking at it, please keep this number since it will be used during the experiment. This number is private and should not be shared with anybody else.

In a moment, I will ask that all of the participants with even numbers, meaning 2, 4, 6, 8, etc., follow me outside this room. These participants will go to an adjacent area, where they will complete a brief questionnaire, and will receive the €5 payment from the experimenter for their participation. When leaving the room, please take all of your belongings.

Instructions for participants with odd numbers

In this experiment, each of you will **[***decide whether to participate or not***]** participate in an activity. **[***That is, participating in the activity is optional***]**. The activity is the following:

> The activity: You will be paired with one of the participants who just left this room. That is, each of you will be paired with one of the participants with an even number (2, 4, 6,…). The pairings will be made randomly and anonymously, which means that nobody will know the identity of the person with whom he or she is paired. You will have to decide how to distribute €10 between yourself and the person with whom you are paired. That is, you will decide how much money, between €0.00 and €10.00, to give to the other person and how much to keep for yourself. For example, you may decide to give €9.00 to the other person and keep €1.00 for yourself, or you may instead decide to give €1.00 to the other person and keep €9.00 for yourself. You may select any distribution of the €10 between yourself and the other person, in increments of €0.10. The assigned amounts will be paid to you and to the other person (in addition to the €5 for participation).

Are there questions about the activity?

The participants in the adjacent area do not know anything about this activity. They received a questionnaire and were asked to complete it.

[*You must decide whether to participate or not participate in the activity.*

- *If you opt to participate in the activity, you will be paired with one of the other participants and will distribute the €10 between yourself and this participant.***]** At the conclusion of the session the participant with whom you are paired will reenter this room and I will explain the activity to him or her. This participant will then discover how much money he or she received from you and how much you kept for yourself. You and the other participant will receive these quantities, plus the €5 for participation.

- **[***If you opt not to participate in the activity you will not be paired with any other participant and you will not distribute any money. In this case you will receive a fixed amount of €10 (plus the €5 for participation), but you will not have the option to distribute this money. At the conclusion of the session, I will go to the adjacent area and I will pay €5 to the people who are not paired with anyone in this room. These people will not receive any information about the activity.***]**

This session will now proceed as follows:

1) Each of you has an envelope **[***…two envelopes: one labeled "participate" and another "don't participate"***]**. Please do not open this envelope **[***either envelope***]** yet.

2) **[***If you decide to not participate in the activity, you will open the envelope labeled "don't participate." Inside this envelope is a sheet. Once you open the envelope, you will remove the sheet and write your participant number in the indicated space. You will receive €10.*

3) *If you decide to participate in the activity, you will open the envelope labeled "participate."***]** Inside the envelope is a sheet with the number of the participant with whom you are paired and on which you will indicate how to distribute the €10 between the other person and yourself. Once you open the envelope, you will remove the sheet and will write your participant number in the indicated space. In addition you should look over the sheet to see the number of the participant with whom you are paired. You should then indicate how you wish to distribute the €10 between the other participant and yourself. The total of the two quantities should sum to exactly 10.00. If they do not sum to 10.00, then the other participant will receive the amount that you specify and you will receive the remainder.

4) **[***In either case,***]** Once you finish, place the sheet back in the envelope and I will collect the envelopes.

At the end of the session, we will do the following:

5) The experimenter will go to the adjacent area and will bring the other participants. **[***…only those participants who are paired with someone who opted to participate in the activity. The rest of the participants in the adjacent area will not be paired, will receive the €5 for their participation and for them the experiment will have concluded.*

6) *If you opted to participate in the activity, the participant with whom you are paired will reenter this room and will …***]** These participants will receive a brief explanation of the activity. The participant with whom you are paired will receive the sheet that you completed, indicating how much money he or she received from you, out of the €10.

7) The experimenter will then anonymously pay the other participants **[***who are paired with someone in this room***]** their total earnings, and will then pay you anonymously. This will conclude the experiment.

Are there questions? Once we answer any questions we will proceed to open the envelopes. **[***Please open only one of the two envelopes.***]**

Decision sheet

Number of the person with whom you are paired: ___________

Your number (***please write your number in the space on the right***): ___________

Amount of money to give to the other person: €_____.____

(*in €0.10 increments*)

Amount of money to keep for yourself: €_____.____

(*in €0.10 increments*)

(*These two quantities must sum to €10.00*)

Decision sheet

You have opted to not participate in the activity. You will not be paired with another participant. At the end of the session, you will receive €10 plus the €5 for participation.

Your number (***please write your number in the space on the right***): ___________

Instructions for participants with even numbers

During the next few minutes, please complete the questionnaire on the attached sheet. After finishing, please wait a few minutes quietly for me to return. At that time, I will pay you the €5. In addition, it is possible that I will require the participation of some of you for a brief additional activity in the session.

While you wait, you may complete the payment receipt. Please leave the amount blank.

Final information for participants with even numbers

While you were out of this room, [*some of*] the participants here participated in an activity in which they distributed €10 between themselves and one of you. You are paired with one of these participants. This other participant decided how much money, from €0.00 to €10.00, to give to you and how much to keep for him- or herself. In a moment you will see a sheet on which this participant has indicated how much money to give to you. This amount, along with the €5 for participation, will be your payment for this session.