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Dictator game giving: Rules of fairness versus acts of kindness

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Abstract. In both dictator and impunity games, one player, the dictator, divides a fixed amount of money between himself and one other, the recipient. Recent lab studies of these games have produced seemingly inconsistent results, reporting substantially divergent amounts of dictator giving. Also, one prominent explanation for some of these differences, the impact of experimenter observation, displayed weak explanatory power in a different but related lab game. Data from the new experiment reported here offers some explanations. We find that dictators determine how much they will give on the basis of the total money available for the entire experimental session, not on the basis of what is available per game. This explains the reported differences between impunity and dictator studies. When distributing a gift among several recipients, individual dictators show little tendency towards equal treatment. Also, we find no evidence for the experimenter observation effect. Comparison with earlier experiments suggests that differences in the context of the game, affected by differences in written directions and independent of experimenter observation, account for differences across dictator studies. We propose a hypothetical decision procedure, based on the notion that dictator giving originates with personal and social rules that effectively constrain self-interested behavior. The procedure provides a link between dictator behavior and a broader class of laboratory phenomena.

Key words: Fairness, dictator game, altruism, anonymity

1 Introduction

Dictator game is a bit of a misnomer. The ‘game’ is actually a one person decision task: one player, the dictator, decides how to distribute a fixed amount of money (a fixed ‘pie’) between himself and one other, the recipient. Dictator and recipient are anonymous in the sense that neither knows the identity of the other.

The standard economic analysis of the dictator game pivots on the assumption that individuals prefer having more money to having less: the dictator should take all the money for himself, leaving nothing for the recipient. Laboratory studies of the dictator game, however, have not yielded this result. In fact, previous investigations report a wide dispersion of dictator game giving. Some dictators do leave nothing, but others give away as much as 50% of the pie. The modal amount left is sometimes as high as 30 percent.

A prominent explanation for this anomaly has to do with the impact of experimenter observation. This hypothesis asserts that some dictators believe that the observing experimenter’s assessment of them will be influenced in a negative way if they exhibit the self-interested – ‘greedy’ – behavior that drives the theory. Subjects might believe that being labeled greedy will lead to exclusion from future experiments, meaning loss of future income, or perhaps a negative assessment simply evokes a sense of social stigma. Either way, dictators would be motivated to leave some proportion of the money in order to avoid the ‘greedy’ tag. Note that this explanation may be consistent with the standard theory: if a reputation for greed leads to some sort of future payoff loss then maximizing earnings may involve avoiding such a reputation. A previous study, discussed below, finds evidence in support of this *anonymity hypothesis*.

But now consider the two-player *impunity game*, a game very similar to the dictator game, but with two distinguishing feature: First, the choice set of the dictator is constrained to either an equal division of the pie or a division that favors the dictator but gives both players a positive amount. Second, the recipient can choose to reject what the dictator has left and leave the game with nothing – although when the recipient rejects the unequal split, it has no bearing on the amount the dictator earns (thus the game’s name ‘impunity’). Under the assumption that more money is preferred to less, equilibrium calls for the impunity dictator to choose the unequal split. Because of the close similarity with the dictator game, it is somewhat of a surprise that a study, also discussed below, finds that impunity dictators play in accord with equilibrium, displaying virtually no inclination to leave more money than required regardless of the amount required, and in spite of the fact that the experimenter observes all impunity play.

This paper reports on a laboratory study which began with two questions: First, what accounts for the divergence in behavior observed between dictator and impunity games? Second, why do subjects leave money in the dictator game? Towards answering these questions, we first enumerated the substantive differences, in terms of both game structure and lab procedures, between dictator and impunity studies. On the basis of these differences we next formulated what we felt to be plausible hypotheses for the divergent behavior. We then constructed an experimental design around these hypotheses with the hope that the experiment would not only answer question one but would also shed some light on the motives for dictator game giving.

Data from the new experiment does indeed shed light on both questions, although not always in a way that could have been predicted from the original design. In particular, even though the experiment completely shatters one of our initial hypotheses, the associated data yields tantalizing clues about how dictators think about their decision task. In addition, our initial results led us to question the explanatory power of the anonymity hypothesis as it pertains to the dictator game. We therefore extended the original design to obtain a direct test. We find no evidence for the hypothesis. However, a comparison of our data set with those of earlier experiments suggests a plausible explanation for the variations observed both within and across experiments. Finally, we propose a hypothetical rule-based decision procedure that links typical dictator behavior to behavior observed in other experimental settings.

2 Previous experiments

A brief review of three earlier studies motivates the design of the new experiment. All are similar although not identical in terms of design and methodology[1], and all used students as subjects. All involve experiments concerning ultimatum games[2] as well as either dictator or impunity. With one exception, the ultimatum experiments are not directly relevant to our present purposes, so (with the one exception) we exclude their description.

2.1 Robert Forsythe, Joel L. Horowitz, N. E. Savin, and Martin Sefton (1994)

This paper studied the replication and statistical properties of the dictator game. The initial experiment involved a \$5 pie and used a 2×2 design of time (April or September) and pay (pay or no pay). In a further experiment, run once, the pie was increased to \$10 in the pay condition. Each participant played a single game with an anonymous partner. Dictators sat in "room A", recipients sat in "room B". Written instructions informed subjects that "a sum of \$5 (\$10) has been provisionally allocated to each pair and the person in room A can propose how much of this each person is to receive." Each subject received a show-up fee, and those in the pay condition also received the payoff from the game.

The study found that outcomes – outcomes being the distribution of dictator giving – were replicable across time (April versus September) in both the pay and no pay condition. The hypothesis that distributions were the same across the pay and no pay conditions was rejected. The hypothesis that the

[1] An interesting earlier study by Kahneman, Knetsch and Thaler (1986) will not be included in our focus because it differs in several methodological respects. For example, in the Kahneman et al. study the probability a subject would actually be paid was very low, about 0.05. All of the studies we focus on involved treatments in which subjects were paid for their actions. Sefton (1992) provides some evidence that randomly paying subjects leads to results that differ substantively from those obtained when each subject is paid.

[2] In the two-player *ultimatum game*, a first mover proposes a division of k dollars to a second mover. If the second mover accepts, the money is divided accordingly. If the second mover rejects, both players receive nothing. Perfect equilibrium requires that the first mover offer the second mover an amount equivalent to the smallest monetary unit allowed (allocating the balance to himself) and that the second mover accept. However, laboratory tests, including those discussed here, find that first movers tend to offer amounts significantly higher than the minimum possible and that a substantial percentage of games end with a second mover rejection. Roth (1995) provides a survey.

distributions of the *proportion* of giving were the same across \$5 and \$10 games with pay was accepted. In the case of the \$10 game, fully 79% left a positive amount of money, with 20% leaving half. The mode of the distribution was \$3 or 30%. So Forsythe et al. exhibit distributions of dictator giving which are both anomalous to standard theory as well as robustly replicable.

2.2 Elizabeth Hoffman, Kevin McCabe, Keith Shachat and Vernon Smith (1994)

This paper begins by noting that a common interpretation of results such as those of Forsythe et al. is that they reflect a concern for fairness on the part of participating subjects. "In this paper we report the results of non-repeated ultimatum and dictator games experiments designed to explore the underlying reasons for this apparent taste for "fairness"." The experiment, as it pertained to the dictator game, had four treatments. In each of these, subjects played a single dictator game with a \$10 pie. Subjects received a show-up fee plus their payoff from the game.

For each game in the random entitlement-exchange treatment (abbreviated to Exchange treatment below), one subject was randomly selected to be a "seller", the other a "buyer". Written instructions explained the game as follows: "The seller chooses the selling PRICE, and the buyer must buy at that price.... Forexample, if the seller chooses PRICE = \$8, the seller will be paid \$8 and the buyer will be paid \$2." The working hypothesis behind the design was that at least some of the dictator giving in the Forsythe et al. experiment can be ascribed to the segment of instructions (cited above) having to do with the money being "provisionally allocated". Hoffman et al. argue that "this instruction suggests that neither bargainer has a clear property right to the money; literally, it provisionally belongs to both of them." By framing the game as a market exchange, the seller (dictator) might feel more comfortable in his property right, and this might induce less dictator giving. A second Hoffman et al. treatment, contest entitlement-exchange (Contest), sought to reinforce this sense of entitlement by replacing random role selection with contest selection. Written instructions told subjects that those who scored highest on a current events quiz "have *earned* the right to be sellers." Other aspects of the treatment design were the same as in Exchange.

A third treatment, Double Blind 1, provided a direct test of the anonymity hypothesis. The design for this treatment was distinguished from the others in several respects. Role selection was random, and, as in Forsythe et al., players were referred to by the room they occupied. One subject was paid \$10 to monitor the experiment – an arrangement that was fully described in the directions. Twelve dictators were each given an envelope that contained 10 one dollar bills together with 10 slips of paper. Two other dictators were each given an envelope containing 20 slips of paper. Written directions instructed dictators to "decide how many dollar bills (if any) and how many slips of paper to put in the envelope. The number of dollar bills plus the number of slips of paper must add up to 10. The person then pockets the remaining dollar bills and the slips of paper." Dictators performed this task in private and deposited the envelope in a common collection box. Consequently, the experimenter could not know which envelope was submitted by which dictator, thus creating *subject-experimenter anonymity*.

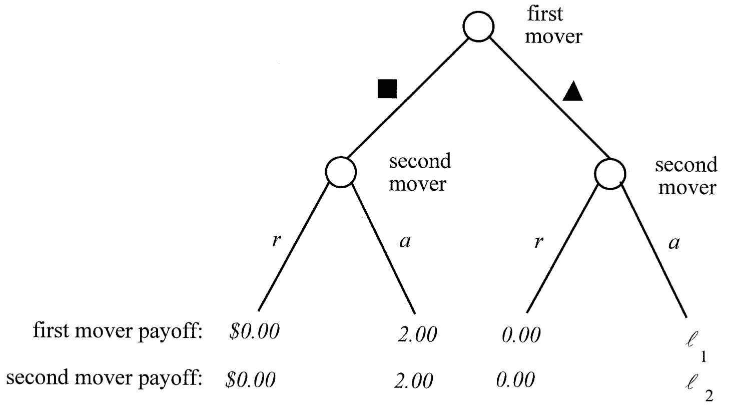
In their write-up, the investigators emphasize a statistical comparison of Double Blind to their other treatments, as well as to Forsythe et al.'s $10 treatment. The distribution of dictator giving for Double Blind 1 was found to be significantly different from that of either Forsythe et al. or Exchange (the later two test statistically the same). Contest, on the other hand, could not be clearly (statistically) distinguished from Double Blind 1. The distribution for these two games was found to be located left of the other two games; so in this sense, Contest and Double Blind 1 exhibited less dictator giving than either Forsythe et al. or Exchange. In Double Blind 1, two thirds of the dictators left nothing, and less than 6% left half. Having established this, the investigators observe that, "The [Double Blind 1] procedures represent a substantial departure from those used in our other experiments.... Which of these procedures are most important?" They then ran a fourth treatment, Double Blind 2, that differed from Double Blind 1 in two respects. For one, the role of subject monitor was eliminated, and the function was performed by the experimenter. Second, the envelopes containing just paper (no money) were eliminated. As it turns out, the dictator giving distribution for Double Blind 2 did not differ significantly from that for Double Blind 1. Hoffman et al. conclude,

These Double Blind experimental results are inconsistent with any notion that the key to understanding experimental bargaining outcomes is to be found in subjects' autonomous, private, other-regarding preferences. At the very minimum, these results suggest that other-regarding preferences may have an overwhelming social, what-do-others-know, component, and therefore should be *derived* formally from more elementary expectational considerations.

2.3 Gary E Bolton and Rami Zwick (1995)

The experiment reported in this paper compared the explanatory power of the anonymity hypothesis to the *punishment hypothesis*; the latter attributes observed laboratory play of the ultimatum game to the willingness of some second movers to punish those who treat them 'unfairly' independent of any considerations of experimenter observation. The experiment had three cells. In the Cardinal Ultimatum cell, subjects played the simplified ultimatum game of Figure 1a with full experimenter observation. In the Zero Knowledge cell, subjects played the same game without experimenter observation (The procedure inducing experimenter anonymity is quite involved and a description is not necessary for our present purposes.). In the Impunity cell, subjects played the impunity game of Figure 1b with full experimenter observation. In all other respects, the design and procedure for the three cells were the same: Subjects were (randomly) separated into a group of 10 first movers and a group of 10 second movers. For each of ten trials, first and second movers were paired (no two were matched together more than once), making for ten observations (games) per trial. Written instructions described the game to subjects as follows: "The game concerns two players, Player A and Player B, along with four boxes of the type displayed in [Figure 2]. Each box specifies a set of payments. Players must select one of these boxes." Subjects were paid their earnings for all ten games. There was no show-up fee.

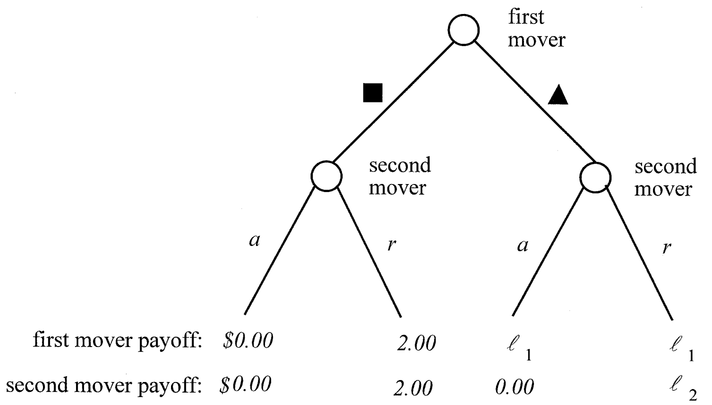
Cardinal ultimatum and impunity games have identical perfect equilibrium paths (see Figure 1): the first mover chooses triangle (▲) followed by the second mover choosing accept *(a)*. Note also the difference between the two



The following sequence for $\begin{pmatrix} \ell_1 \\ \ell_2 \end{pmatrix}$ was used for trials 1 through 5 and 6 through 10:

$\begin{pmatrix} 2.20 \\ 1.80 \end{pmatrix}, \begin{pmatrix} 2.60 \\ 1.40 \end{pmatrix}, \begin{pmatrix} 3.00 \\ 1.00 \end{pmatrix}, \begin{pmatrix} 3.40 \\ .60 \end{pmatrix}, \begin{pmatrix} 3.80 \\ .20 \end{pmatrix}.$

Fig. 1a. Cardinal ultimatum game



The same sequence of $\begin{pmatrix} \ell_1 \\ \ell_2 \end{pmatrix}$'s used as described above.

Fig. 1b. Impunity game

games: in impunity, the second mover lacks a punishment response to the first mover choice of triangle; that is, a rejection (play of *r*) on the part of the second mover does not diminish the first mover's payoff as it does in cardinal ultimatum. If the punishment hypothesis is correct, then the second mover would have no incentive to play *r* in the impunity game, and there should be much more perfect equilibrium play in the Impunity cell than in either Cardinal Ultimatum or Zero Knowledge. The latter two cells should exhibit similar levels. If, on the other hand, the anonymity hypothesis is correct, then there should be much more perfect equilibrium play in Zero Knowledge.

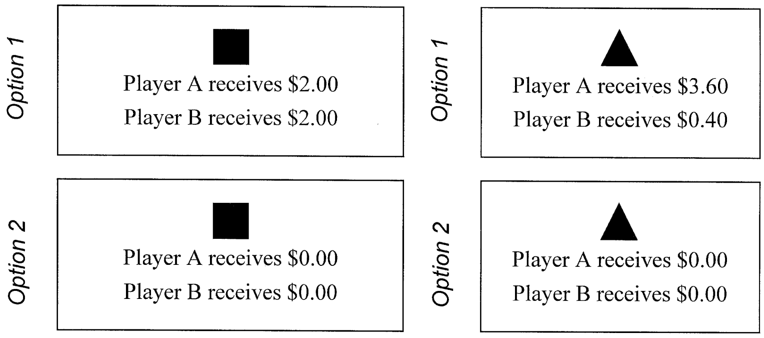


Fig. 2. Box choices, Bolton and Zwick (1995)

Cardinal Ultimatum and Impunity should exhibit similar levels because both are played with experimenter observation, which implies that first movers have the same incentive to make fair offers.

The amount of perfect equilibrium play observed over the last five trials was 30% in Cardinal Ultimatum, and 46% in Zero Knowledge. It was 100% in Impunity. The investigators conclude that these results strongly support the punishment hypothesis over the anonymity hypothesis.[3]

Together, these studies create a certain dissonance. Impunity and dictator are clearly very similar games. Yet in Forsythe et al.'s treatment the modal dictator offer is 30%, and only 20% leave the minimum (zero), while in Bolton and Zwick's impunity game, dictators demonstrate an unwillingness to leave any more than the minimum, regardless of whether the minimum is 5% or 45% of the pie. The anonymity hypothesis can not explain the difference since both treatments were conducted with experimenter observation. What is the explanation? Also, note that none of these studies offers a complete explanation for why dictators leave money in the dictator game: even in Hoffman et al.'s Double Blind 1, under conditions of subject-experimenter anonymity, one third of subjects do leave a positive amount, contrary to the standard theory prediction. What's the explanation?

3 Two hypotheses

We search for clues by examining the differences, in terms of both game structure and lab procedure, between dictator and impunity studies.

3.1 Game structure differences and the I'm-no-saint hypothesis

The games are structurally distinguished by two features: (A1) The impunity dictator's choice set is restricted to two divisions of the pie, one being an equal

[3] This conclusion is confirmed by Fong and Bolton (1997), who provide a detailed statistical comparison of the Cardinal Ultimatum and Zero Knowledge data.

split, the other being a split in favor of the dictator. In contrast, the dictator in the dictator game has a fairly comprehensive set of divisions to choose from.[4] (A2) The second mover in impunity can choose to reject what is offered by the dictator, thereby leaving the game with nothing (but keep in mind that a rejection does not influence the dictator's payoff if the dictator offers the uneven split). In the dictator game, the second mover has no choice but to accept.

While we were unable to think of any reason that (A2) should account for a difference in dictator behavior across games,[5] thinking about (A1) led to a seemingly plausible hypothesis. As motivation, consider an impunity dictator who has $4 to divide. Suppose that, if free to divide as he pleased, his *unrestricted choice* would be $3–$1 (in favor of the dictator); in other words if he were playing the dictator game he would split $3–$1. Impunity, however, allows him only two choices, one being an equal split, the other being an unequal split that favors the dictator. Of course, if the unequal split is exactly equal to the unrestricted choice, we would expect that the dictator would choose the unequal split. Now suppose the unequal split gives the recipient *more* than the dictator's unrestricted choice ($2.20–$1.80, for example). We would still expect the dictator to choose the unequal split since both splits give away more than his unrestricted choice, and the unequal split is closest. On the other hand, suppose that the unequal split gives the recipient *less* than the dictator's unrestricted choice ($3.60–$0.40, for example). The dictator might then reason that "I have a choice between erring in favor of the other person's welfare or erring in favor of my own. Only saints err in favor of the other person and *I'm-no-saint*. Therefore I choose the $3.60–$0.40 split." In summary, this dictator would always choose an unequal split that favors the dictator over an equal split, regardless of the actual value of the unequal split. More generally (this is easy to check), under the assumption that all dictators employ I'm-no-saint reasoning, the unequal split should be selected regardless of the value of the dictator's unrestricted choice – with but one exception: the dictator who would freely choose to leave 50% of the money (or more) would, in the impunity game, choose the equal split. For testing purposes, it is useful to express this hypothesis exclusively in terms of the dictator game:

I'm-no-saint hypothesis: *If the dictator in the dictator game is restricted to two division choices, one being the equal split and the other being an unequal split favoring the dictator, then the percentage choosing the equal split will be equal to the percentage that would choose the equal split in the unrestricted game.*

If this hypothesis is correct, and if the percentage of players who leave 50% is small, which seems plausible given previous data, then this hypothesis would go a long way towards reconciling the differences in impunity and dictator results.

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[4] This is not to say that the set of divisions is always complete. For example, in Hoffman et al., choices were restricted to divisions that could be expressed in whole dollars.

[5] In both games, the second mover chooses after the dictator. In neither case does the second mover's choice affect the first mover's payoff (unless an impunity dictator's offer of 50% is rejected by the second mover – but this is a highly unlikely response given all available lab evidence). So it is difficult for us to see a reason why the difference in second mover choices described in (2) should influence dictator behavior.

3.2 Differences in lab procedure and the rational giving hypothesis

A second hypothesis is developed from an examination of the differences in lab procedures. Broadly speaking, the three studies are comparable in this respect, but there are two substantive differences:[6] (B1) Impunity dictators played multiple games, while dictator game dictators played a single game. (B2) Instructions framed the games in different manners.

In developing the I'm-no-saint hypothesis, we did not explicitly consider the number of games a dictator plays. Here we develop a second hypothesis – based on (B1) – independent of the I'm-no-saint hypothesis. Consider now a dictator who is 'rational' in the sense that he gives in a manner consistent with balancing the marginal cost of giving against the marginal benefits, where the benefits have something to do with improving the welfare of the recipient. Suppose that in a dictator game with a $10 pie, this dictator chooses to leave $3. Would he leave the same total amount if he played ten $1 pie games, each game with a different recipient? Specifically, as a rational dictator, would he leave each of 10 recipients $0.30 for a sum total leaving of $3? The answer depends on the dictator's assessment of the impact of $0.30 on a recipient's welfare. One could argue that, while giving $3 to one person is worth the $3 cost to the dictator, giving ten people $0.30 each may not have sum total benefits worth $3 since $0.30 would have negligible impact on an individual's welfare. The assessed value of the benefits might be further eroded if the recipients are to be matched with other dictators. A (rational) measure of the benefits from leaving $0.30 per recipient would then have to include an assessment of how much other dictators will leave. As the assessment increases, it seems reasonable to assume that the measure of benefits that accrue from leaving a fixed amount, such as $0.30, will decrease.[7]

The lab procedure used by Bolton and Zwick to study the impunity game involved having subjects play the game ten times, each time with a different partner. The dictator studies, on the other hand, had each subject play a single time. Perhaps, then, the seemingly smaller amounts of giving in the impunity games can be explained by the rational giving argument.

Rational giving hypothesis: *Let k be a fixed dollar amount, and fix dictator and recipient roles, n participants in each role. Suppose that each dictator (recipient) plays n dictator games with n distinct partners, each game having a pie of value*

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[6] Of course, it is impossible to deal with all of the differences, most of which are minute details. One *similarity* which is not immediately obvious is game pie size. Hoffman et al. looked at $10 dictator games, while Bolton and Zwick looked at impunity games with a pie size of $4. Looked at in isolation, this might seem like a large difference in pie sizes, but keep in mind that Forsythe et al. looked at both $5 and $10 dictator games and found no difference in the distribution of the proportion of dictator giving.

[7] The inspiration for this hypothesis came from watching television appeals for donations to charities that aid impoverished children. Many of these organizations ask for money to help an individual child rather than a group of children. In return for a donation, you are promised a regular progress report on 'your child'. This type of appeal – which we assume is successful since it has been around for many years – seems to be based on the idea that perspective donors perceive greater benefit accruing from giving, say $20 a month to a single child, than they do from giving $20 a month (together with other donors) to help a large group of children.

k/n. The proportion of the pie that a dictator leaves for each recipient diminishes as n increases.

Note the implicit auxiliary hypothesis that the dictator will give the same amount to each recipient. Play is between anonymous subjects, meaning the dictator has no basis on which to discriminate, and it therefore seems plausible that whatever he leaves one recipient is what he will leave any other. Note also that the rational giving hypothesis is not necessarily inconsistent with the I'm-no-saint hypothesis. Both (or neither) explanations could have explanatory power.

The differences in directions across experiments, (B2), are subtle and sorting out what, if any, impact they have on subject behavior is potentially very complex. The new experiment does not deal directly with this issue – we attempted to hold the directions as constant as possible across treatments. The new data does, however, compel a more careful consideration of directions across experiments.

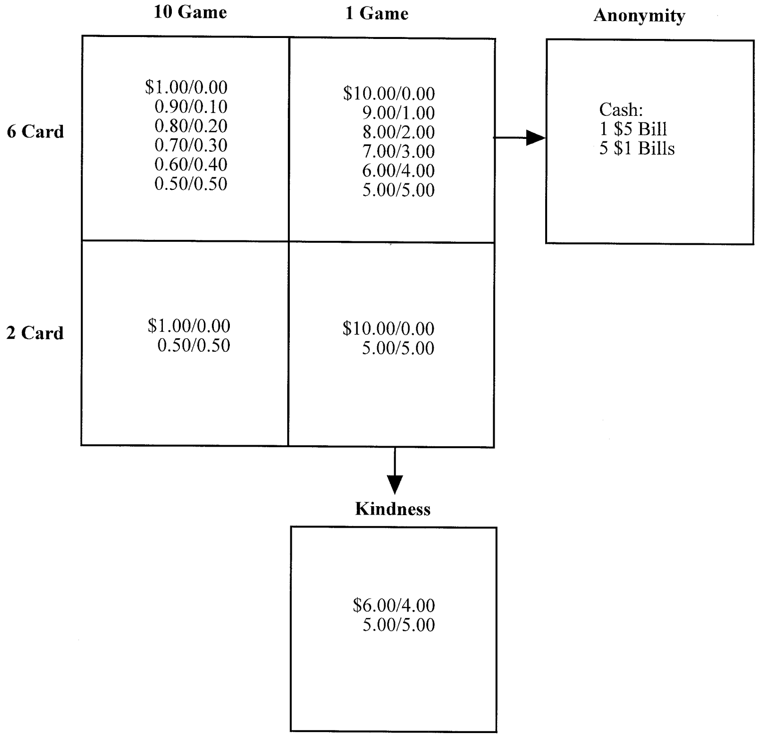
4 The new experiment

The new experiment dealt exclusively with variations of the dictator game; that is, all games involved a dictator choosing a division of a pie, while recipients had no choice but to accept the dictator's decision.

4.1 Design of the initial experiment

The experiment began as a 2×2 design and is represented by the 2×2 box in Figure 3 (the other two treatments in the figure were developed in response to the results from the initial design and are discussed below). The treatment variables were the number of division choices per game ('2Card' or '6Card') and the number of games each subject participated in ('10Game' or '1Game'). In the 1Game treatments, each game involved a \$10 pie. In the 10Game treatments, each game involved a \$1 pie. So in all cases, the total amount available for division by a single dictator was \$10.

The 1Game-6Card treatment represents an attempt to replicate previous dictator game results. Comparing 1Game-6Card with 1Game-2Card provides a direct test of the I'm-no-saint hypothesis. If the hypothesis is correct, then the percentage of dictators offering \$5.00 should be the same across the two cells. Comparing 1Game-6Card with 10Game-6Card provides a direct test of the rational giving hypothesis. If this hypothesis is correct, then dictators should tend to give more in 1Game-6Card then in 10Game-6Card; 'giving' in the later case being defined as the total over all 10 games. Comparing 10Game-2Card with the other three treatments provides a test for the cross-effects of the treatment variables; that is, we can check whether the two hypothesized effects tend to reinforce one another or cancel one another out or possible have some unexpected effects. The 10Game-2Card treatment also represents a rudimentary attempt to replicate the impunity result of Bolton and Zwick; in particular, if the two hypothesized effects, either separately or in combination, explain the basic differences between dictator and impunity then 10Game-2Card play should reproduce impunity play.



The contents of each box represent the choice set available to Player A for each game played within the treatment.

x/y = Player A receives $\$x$ while Player B receives $\$y$.

Fig. 3. Experimental design

4.2 Laboratory protocol for the initial experiment

The complete protocol appears in the appendix. It includes a detailed description of procedures, as well as the written directions given to subjects. In addition, the monitor read all verbal instructions directly from the protocol (so the only monitor-subject communication not included in the protocol are individual subject questions-and-answers).[8] A brief synopsis of the laboratory protocol for the initial 2 × 2 design follows:

For each session, subjects were assembled in a single room. Written directions described a "game" concerning "Player A" and "Player B" in which Player A gets to choose a card ('label' in the 10Game treatments). Each card indicated a division of the money and Player A got to choose one card for each game played. The actual set of choices was determined by the treatment (see Figure 3). After the monitor read the directions aloud and answered any questions, subjects were randomly divided into equal numbers of Players A

[8] In all sessions, Zwick read the protocol and answered all subject questions.

and Players B. Players B were escorted to a second room. Players A were then seated at individual cubicles, thereby allowing them to make their choice(s) in private. Once selections were complete the monitor went around to each cubicle and paid each Player A his earnings in cash. After all Players A left, each Player B was paid his earning in cash.

4.3 Design and procedure of additional treatments

The results from this experiment led to two further treatments (both shown in Figure 3). Both were '1Game' but each presented the dictator with new choices. In the Anonymity treatment Players A divided the cash directly, placing the amount they wished to leave to the recipient in small unmarked boxes. These boxes, all of which were identical, were then placed in a common collection bin. Players A left without reporting their choice (earnings) to the monitor. The data was recovered when Players B opened the boxes in the monitor's presence. The design of the experiment thus prohibits the monitor from observing the choice of any *individual* Player A while allowing for a recovery of the full *distribution* of choices. So the Anonymity treatment was conducted under a condition of experimenter-subject anonymity. All other procedures, including directions to subjects, were very similar to the 1Game-6Card treatment (see Appendix). The Kindness treatment differed from 1Game-2Card only in the values of the two choices presented to the dictator. The new values are listed in Figure 3. The rationale for both these treatments is explained below.

4.4 Subject pool

All subjects were students at Penn State University. They were recruited through billboards posted around campus. Participation required appearing at a special place and time. Cash was the only incentive offered. Each subject participated in a single treatment, and was paid a $5 fee for showing up on time plus all earnings from the games played.[9]

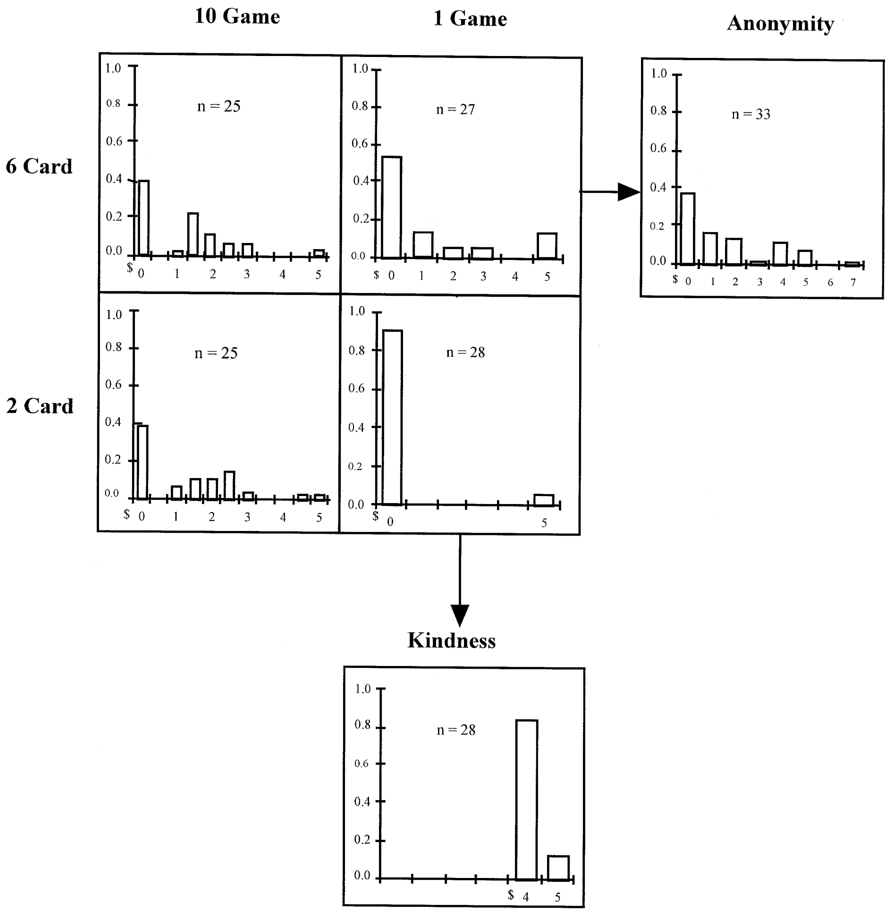
5 Results for the initial, as well as two further, hypotheses

The data for the new experiment – that is, distributions of dictator giving for the various treatments – are displayed in Figure 4.

5.1 I'm-no-saint hypothesis

We test this hypothesis two ways. First, we check to see whether the proportion of dictators leaving $5.00 (half the pie) is the same across 1Game-

[9] All sessions were run in 1993. The 1Game treatments were run on February 17 and March 3. The 10Game treatments were run on April 2 and 8. The Anonymity treatment was run on March 19. The Kindness treatment was run on April 16. All sessions began between 4 and 5 pm and lasted about 30 minutes. The average earning (including show-up fee) was $10.



n = number of observations within treatment

Fig. 4. Frequency of dictator giving by treatment

6Card and 1Game-2Card. The proportion in 1Game-6Card is about 15% (4 out of 27), which is quite similar to 1Game-2Card where it is about 7% (2 out of 28). In fact a Fisher exact test yields a two-tail p-value of 0.63, so we accept the hypothesis that the proportions are the same. Of course accepting a null hypothesis raises concern about the power of the test. This prompted a second test, based on the observation that the I'm-no-saint hypothesis implies that the proportion of dictators who leave zero in 1Game-2Card should be greater than the proportion in 1Game-6Card. This is indeed the case in our sample where the proportion leaving zero rises from about 56% to about 93%. A chi-square test shows this shift to be strongly significant with a p-value of less than 0.002. So our sample does exhibit a statistically significant treatment effect. The evidence is consistent with the I'm-no-saint hypothesis.

5.2 *Anonymity hypothesis*

In analyzing the data, we were struck by the distribution of giving in the 1Game-6Card treatment. Both mode and median are zero, thereby distinguishing 1Game-6Card from the $10 dictator game Forsythe et al. and all but the Double Blind treatments of Hoffman et al.[10] But unlike 1Game-6Card, the Double Blind treatments were run under conditions of experimenter-subject anonymity. Observing this, we ran the Anonymity treatment described in section 4.3. The results are displayed in Figure 4. Since the anonymity hypothesis asserts that there should be a tendency towards less giving in the Anonymity treatment, we judge the hypothesis using a rank correlation (S) test, based on Kendall's tau coefficient.[11] The resulting one tail p-value is about 0.77, so the hypothesis that the two distributions are the same (particularly in the sense that they have the same location) cannot be rejected. In fact, as the p-value indicates, what location shift there is, is in the wrong direction.[12] We therefore find no evidence for the anonymity hypothesis.

5.3 *Rational giving hypothesis*

This hypothesis asserts that the sum giving of each dictator in 10Game-6Card should tend to be smaller than that in 1Game-6Card. A one tail rank correlation test yields a p-value of 0.36. In fact, pair-wise rank correlation tests between 10Game-2Card, 10Game-6Card and 1Game-6Card show no statistically significant location shift (see Figure 5). We therefore find no evidence for the rational giving hypothesis.

Moreover, examination of the data reveals that the auxiliary hypothesis, that each dictator would treat each of the other ten players in like manner, is clearly incorrect. Figure 6 displays the pattern of giving for each individual dictator for the 10Game-6Card treatment. Thirteen out of 25 dictators do not treat recipients in a like manner. Of these thirteen, most appear to be giving

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[10] A formal statistical analysis confirms this observation and is discussed in section 6.2. Also see Figures 9 and 10.

[11] In this context, Kendall's tau coefficient provides a measure of the location shift across two treatment distributions. Tau is calculated from a ranking of the pooled sample data in much the same manner as either the Mann-Whitney or Wilcox statistics. In fact, all three test statistics are linear transformations of one another and the corresponding tests are equivalent. Under the null hypothesis, tau is approximately normally distributed, so the usual way to proceed is to calculate p-values from a z table. The literature warns, however, that if there are many ties in the rankings – as is the case in our data – then the normal approximation may not be very good (ex., Kendall and Gibbons (1990), p. 66). To get a sense of what sort of error might be involved, we generated p-values via empirical simulation. We found that the (more accurate) p-values generated from the simulation tend to be higher than those approximated from the z tables, which is a real cause for concern. Consequently, the p-values we report are the averages of five empirical simulations, each involving 5000 random draws.

[12] Specifically, the derived test statistic corresponds to a Kendall tau coefficient of -0.058. Tau always lies between -1 and $+1$. Larger absolute values of tau indicate larger differences in location. In this case, the negative sign indicates that the anonymity sample was located to the left of the 1Game-6Card sample. To get some sense of the power of the test, we used the standard error and continuity correction derived from the data to calculate the critical value of tau at both .10 and .05 levels; these are 0.116 and 0.140, respectively.

	1GAME6	10GAME2	ANON
10GAME6	51 (0.708)	−23 (0.808)	−96 (0.571)
ANON	136 (0.464)	136 (0.455)	
10GAME2	65 (0.649)		

X (y)	X = S test statistic y = p-value (two tail)

Each p-value was determined by sampling the actual distribution associated with the relevant contingency table. The reported value is the average of five 5,000 trial samplings.

Fig. 5. Summary of Rank Correlation (location) test (S)

money in a random-like pattern: with the exception of dictator 8 (see Figure 6), all pass a turning point test for randomization,[13] and even dictator 8 appears to be giving in a capricious way.

In attempting to make sense of this behavior, we considered several possibilities consistent with the notion that dictators give for reasons of distributional fairness and concern for others' welfare. For instance, one might suppose that dictators first decided on a fair total gift, and then thought about splitting that amount equally among the players before discovering that splitting equally meant shares too small to have any meaningful impact on welfare. They then decided to give only larger amounts, leaving some recipients with nothing. But this hypothesis is contradicted by the data in Figure 6 which shows that a majority of dictators who chose to give at least sometimes giving as little as \$0.10, the minimum positive amount.

The results from the 10Game treatments raise the possibility that dictators are giving for reasons beyond distributional fairness, a motive that is implicit in both the I'm-no-saint and rational giving hypotheses. The anonymity hypothesis offers an alternative motive, but we have already rejected this explanation. Many alternative motives to distributional fairness are encompassed by the *kindness hypothesis*. The intuition behind the hypothesis is that at least some dictator giving derives from the *act* of gift giving. Dictators might be motivated to *give* a gift for one or more of many reasons; e.g., a sense of moral obligation, pleasure derived from giving, to demonstrate kindness. Opera-

[13] To perform a turning point test, we count the numbers of peaks and troughs on an individual diagram in Figure 6. If the series is randomly generated, then the number of peaks (troughs) approaches a normal distribution with mean and variance dependent on the number of points in the series (see Kendall and Ord (1990), p. 18 for details). The resulting z score for each dictator appears in Figure 6. The test does not pick up all patterns. For example, dictator 2's giving exhibits a clear pattern, but the test fails to detect this. It is not our intention here, however, to prove, or even posit, that dictators generated their offers by a truly random mechanism. Rather, we present the test results to provide a feel for the manner in which dictators distributed money.

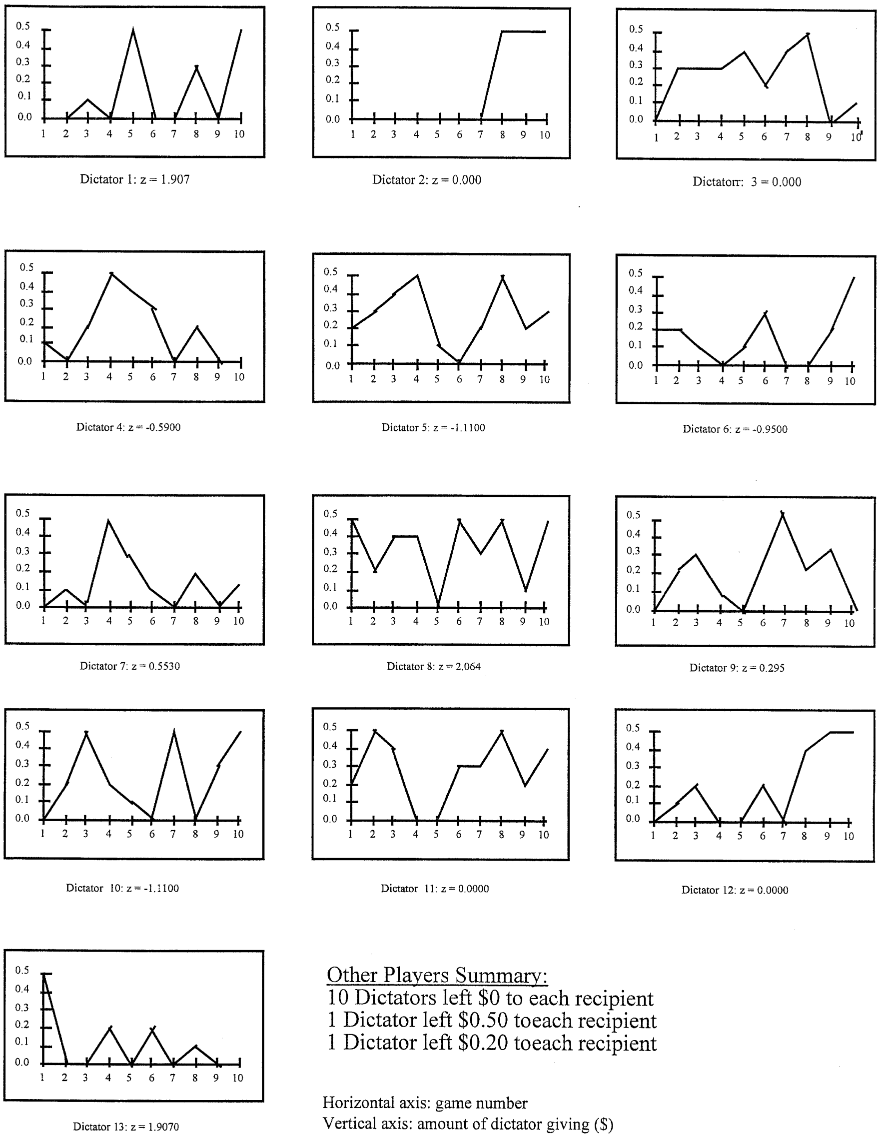

Fig. 6. Dictator giving in 10Game-6Card

tionally, if this sort of motive is at work, then there must exist cases where dictators give amounts of money that exceed what they would give for distributional reasons. One implication is that there should be some tendency to leave more money than the minimum necessary even when that minimum exceeds what the dictator would leave if given an unconstrained choice.

Kindness hypothesis: *Dictators tend to leave amounts of money that exceed the minimum necessary (at least when the minimum is below 50% of the pie).*

Note that the I'm-no-saint hypothesis conflicts with the kindness hypothesis. In fact, it might appear that comparison of the 1Game-6Card data with that from the 1Game-2Card data provides evidence against the kindness hypothesis, but there is a caveat: 1Game-2Card dictators had a choice of leaving either \$5 or nothing, meaning that the opportunity cost of giving anything was quite high. Perhaps if the opportunity cost were lower we would see some kindness.

5.4 Testing the kindness hypothesis

The Kindness treatment was designed to provide a direct test of the kindness hypothesis against the I'm-no-saint hypothesis. The treatment had a 1Game, 2Card design. Instead of choosing between leaving \$5 (50%) or nothing, the dictator chose between leaving \$5 (50%) or \$4 (40%). In this way, the opportunity cost of practicing kindness was much reduced. If the kindness hypothesis is valid, we should see a larger proportion of dictators leaving 50% in the Kindness treatment than in 1Game-6Card (or 1Game-2Card). Figure 4 shows the resulting distribution. A one tail Fisher exact test gives a p-value of 0.63 for the 1Game-6Card and Kindness comparison, a p-value of 0.30 for the 1Game-2Card and Kindness comparison. We therefore find no evidence for the kindness hypothesis – no evidence that giving is motivated by a desire to simply give a gift – but find further evidence for the I'm-no-saint hypothesis.

6 Reconciling the data

We have found evidence for the I'm-no-saint hypothesis, but rejected the other three hypotheses considered. In drawing conclusions, however, we need to pay careful attention to the nature of the evidence in total. In particular, the explanatory power of the I'm-no-saint hypothesis must be interpreted in the context of all the data.

6.1 Reconciling the impunity result with the dictator games

The 10Game-2Card treatment involves dictator games that possess all the characteristics of the impunity game that we believed to be of consequence. In this sense, we intended 10Game-2Card to be a replication of the impunity result. Prior to the experiment, we thought replication meant inducing dictators to leave very little, if anything, to each and every recipient. After all, we reasoned, in the impunity study, dictators left the minimum allowable almost every time, regardless of the value of the minimum. So if we allow them to leave very little in every game, they should do so. Note the implicit assumption that dictators would treat each recipient in a like manner. Both 10Game-6Card and 10Game-2Card data decisively demonstrate that such an assumption is ill-founded. Dictators show little propensity for identical treatment. Given this, the real observation from the impunity study that needs to be replicated is that dictators chose to leave a *total* amount (across all games) that was not appreciably greater than the minimum amount they were forced to leave: 25%. Consider then the hypothesis that the proportion that give more than 25% for both the impunity treatment and 10Game-2Card are the same against the alternative that the proportion in impunity is less.[14] Using a Fisher exact test we obtain a p-value of 0.35. So we accept the proposition that 10Game-2Card replicates the impunity result.

Note that we could have come to the same conclusion by examining 10Game-6Card instead of 10Game-2Card (in testing the hypothesis that the proportion that give more than 25% is the same as in impunity, we obtain the same p-value of 0.35). We point this out to highlight the inversion that has taken place here: While we find strong evidence for the I'm-no-saint hypothesis, we have nevertheless made no use of it in explaining the relationship between dictator and impunity games. On the other hand, we have found strong evidence against the rational giving hypothesis, but that same evidence demonstrates that impunity and dictator results are consistent with one another.

Our basic finding here is that dictators determine how much money they should keep, and consequently how much they should give in gifts, on the basis of the total available for the entire experimental session, not on the basis of what is available per game. So dictators in the impunity study appear to be less generous than those in the dictator studies when considered on a per game basis. But when viewed on the basis of the entire experiment they appear equally, and therefore consistently, generous.

This finding leads to a subtle but significant change in our understanding of what we have demonstrated with respect to the I'm-no-Saint hypothesis. Comparing 1Game-6Card to 1Game-2Card demonstrates that when faced with a choice of leaving either more or less than they would freely choose *for the session*, dictators chose less. But comparing 10Game-2Card to 10Game-6Card shows that this does not imply that restricting choices *for each game* will necessarily have the same effect. Our data exhibits an I'm-no-Saint effect at the session level, not at the game level. Dictators are very particular about how much they leave in total, and prefer to leave less than they would freely choose to leaving more. On the other hand, how the gift is distributed appears far less predictable.

6.2 Comparison with the previous studies

The distributions obtained from 1Game-2Card and Kindness reflect the obviously binding constraints we placed on total dictator giving in those two treatments. On the other hand, 1Game-6Card, 10Game-2Card, 10Game-6Card and Anonymity all exhibit very similar patterns of dictator giving (Figure 7). More formally, we can examine the hypothesis that the giving distributions obtained from the later four treatments are from the same population distribution using a χ^2 contingency table test.

The resulting p-value of 0.39 permits us to pool the data from all four cells (Figure 8).[15] The pooled distribution provides a composite portrait of the

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[14] Out of ten dictators in the impunity game reported by Bolton and Zwick (1995), none left appreciably more than the 25% minimum.

[15] The reported p-value is the average of five trial samplings of the contingency table distribution. This insures a more accurate p-value than could be obtained from any test that relies on a χ^2 table approximation. Dictators in the 10Game treatments had more choices of total amounts to leave open to them (ex., they could leave $1.50) than did dictators in 1Game-6Card and Anonymity,

	1GAME6	10GAME2	ANON
10GAME6	3.62 (0.489)	3.33 (0.922)	5.60 (0.370)
ANON	5.62 (0.369)	5.62 (0.362)	
10GAME2	6.96 (0.212)		

X (y)	$X = \chi^2$ test statistic y = p-value (two tail)

Each p-value was determined by sampling the actual distribution associated with the relevant contingency table. The reported value is the average of five 5,000 trial samplings.

Fig. 7. Summary of the test for homogeneity of distributions (χ^2)

total giving found in the new experiment, and we use it to compare our results to those of the other dictator game studies.

In addition to the Pooled distribution, our comparison involves the four $10 dictator games of Hoffman et al. and the one $10 dictator game of Forsythe et al. We compare the distributions along several dimensions. Figure 9 provides a formal statistical comparison in terms of the general overall homogeneity of the distributions.[16] Figure 10 provides a formal statistical comparison in terms of location.[17] Figure 11 provides a chart comparing the distributions in terms of standard measures of central tendency and dispersion. Examining these three figures suggests that the data can be broken down into essentially

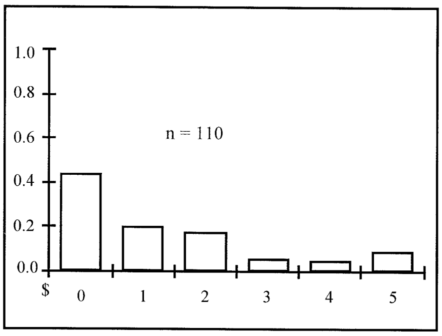
who were constrained to leave whole dollar amounts. In order to compare distributions, it was therefore necessary to categorize each 10Game dictator's total gift into either the 0, 1, 2, 3, 4 or 5 dollar category. We did so in accord with the I'm-no-Saint hypothesis which implies that a 10Game dictator who left a non-whole dollar amount would round his gift down if constrained to leave a whole dollar amount.

[16] Homogeneity tests based on the χ^2 statistic examine many facets of the two distributions being compared. If any one feature of one distribution differs substantially enough from its counterpart in the other distribution, the test will register a low p-value. The price for such a catch-all test is that it does not put too much weight on any single feature; i.e., the test does not have strong power with respect to any one feature. So if you are particularly interested in location, which is often true when comparing dictator distributions, then you want a test that emphasizes location differences, as does a rank correlation test (discussed in ftnte 11). For our purposes it probably makes sense to do two tests: a rank correlation test to determine location differences and a χ^2 test to get a general feel for whether there are any other (strong) differences.

[17] Although our testing procedures for both location and homogeneity differ from those used by either Forsythe et al. (1994) or Hoffman et al. (1994), our results are nevertheless very similar. In particular, with but one exception, reported results of hypothesis testing at the .05 level are the same regardless of whose p-values we use. For the exceptional case, the Double Blind 1-Contest comparison, Hoffman et al. reject the null hypothesis for location (one tail) and accept for homogeneity, both at the .05 level. Out tests do just the opposite (adjust our reported location p-value for a one tail test). So even for the exceptional case, the basic conclusion is the same regardless of whose test results are considered: evidence is mixed on whether the two distributions significantly differ.

χ^2 test statistic = 15.98
p-value = 0.392

Pooled Data



n = total number of observations

P-value was determined by sampling the actual distribution associated with the relevant contingency table. The reported value is the average of five 20,000 trial samplings.

Fig. 8. Pooled Data: contingency table test for pooled data (10Game-6Card, 10Game-2Card, 1Game-2Card, and Anonymity)

two equivalence classes. One class consists of Forsythe et al. and Hoffman et al.'s Exchange treatments. The other class consists of the remaining three Hoffman et al. treatments and the Pooled distribution. The distributions within each class appear quite comparable with respect to the measures considered.[18]

We next consider explanations for the difference across classes. Since treatments in both classes involved dictator games with a total pie of $10 with anonymous playing partners, we must look to differences in experimental procedures. Broadly speaking, potential differences can be categorized as

(1) subject pool (includes selection process as well as demographics);
(2) venue (physical location of experiment);
(3) extent of subject-experimenter anonymity;
(4) reward structure (show-up fee and performance-to-payoff mapping);
(5) trial structure (single shot game or repeated, partner rotation scheme);
(6) game frame (how the game was explained to subjects and method of role selection).

[18] The only treatments that present any ambiguity in classifying are Hoffman et al.'s Contest and Double Blind 1 treatments. One could make an argument for classifying Contest in either of the two classes we created. One could argue that we should create a separate category for Double Blind 1. In both cases, we made the choice that we felt the evidence was the strongest for. Alternative classification of these two treatments, however, would not alter the conclusions drawn in this section.

	POOL (BKZ)	FORS (FHSS)	CONTEST (HMSS)	BLIND1 (HMSS)	BLIND2 (HMSS)
EXCHANGE (HMSS)	33.91 (0.000)	7.83 (0.160)	13.50 (0.010)	26.43 (0.000)	20.57 (0.000)
BLIND2 (HMSS)	6.54 (0.263)	14.00 (0.011)	10.16 (0.057)	2.59 (0.838)	
BLIND1 (HMSS)	6.84 (0.231)	15.82 (0.002)	11.06 (0.031)		
CONTEST (HMSS)	5.78 (0.321)	8.98 (0.096)			
FORS (FHSS)	17.91 (0.038)				

X (y)	$X = \chi^2$ test statistic $y =$ p-value (two tail)

Each p-value was determined by sampling the actual distribution associated with the relevant contingency table. The reported value is the average of five 5,000 trial samplings.

EXCHANGE (HMSS) refers to "Random Exchange" cell in Hoffman et al. (1994)
BLIND1 (HMSS) refers to the first "Double Blind" cell in Hoffman et al. (1994)
BLIND2 (HMSS) refers to the second "Double Blind" cell in Hoffman et al. (1994)
CONTEST (HMSS) refers to the "Contest Exchange" cell in Hoffman et al. (1994)
FORS (FHSS) refers to the $10 Dictator cell in Forsythe et al. (1994)
POOL refers to our pooled 10Game-6Card, 10Game-2Card, 1Game-2Card and Anonymity data.

Fig. 9. Comparing Studies: summary of the test for homogeneity of distributions (χ^2)

We rule out (4) and (5) because these were very similar across all three studies. We rule out (1) and (2) because these differences cannot explain why the Hoffman et al. treatments are split across classes. We rule out (3) because the new experiment was unable to find any subject-experimenter anonymity effect (also see comments in the next section).

That leaves differences in the game frame, (6), and in this we can find a consistent explanation. Recall that the description of the game was the same for all of the new experimental treatments (subjects were told they would play a "game" in which Player A chose one of several options). The resulting distributions tested homogenous. Forsythe et al.'s description of the task was quite different (the pie had been "provisionally allocated") which plausible explains why the resulting distribution is significantly different from those of the new experiment. The Hoffman et al. descriptions were distinct from those used by either of the other studies. In fact, Hoffman et al. varied their description across treatments, and this can explain why the Hoffman et al. treatments are split across categories. Specifically, the description of the task for the Exchange treatment differed from that for the other treatments. The treatment with directions most similar to Exchange was Contest, but there is nevertheless a substantial difference: dictators in Contest were told that they

	POOL (BKZ)	FORS (FHSS)	CONTEST (HMSS)	BLIND1 (HMSS)	BLIND2 (HMSS)
EXCHANGE (HMSS)	1127 (0.043)	75 (0.542)	283 (0.020)	487 (0.004)	473 (0.013)
BLIND2 (HMSS)	−664 (0.768)	−428 (0.021)	−154 (0.777)	145 (0.316)	
BLIND1 (HMSS)	−897 (0.139)	−449 (0.013)	−214 (0.116)		
CONTEST (HMSS)	38 (0.955)	−203 (0.097)			
FORS (FHSS)	887 (0.106)				

X (y) — X = S test statistic; y = p-value (two tail)

Each p-value was determined by sampling the actual distribution associated with the relevant contingency table. The reported value is the average of five 5,000 trial samplings.

EXCHANGE (HMSS) refers to "Random Exchange" cell in Hoffman et al. (1994)
BLIND1 (HMSS) refers to the first "Double Blind" cell in Hoffman et al. (1994)
BLIND2 (HMSS) refers to the second "Double Blind" cell in Hoffman et al. (1994)
CONTEST (HMSS) refers to the "Contest Exchange" cell in Hoffman et al. (1994)
FORS (FHSS) refers to the $10 Dictator cell in Forsythe et al. (1994)
POOL refers to our pooled 10Game-6Card, 10Game-2Card, 1Game-2Card and Anonymity data.

Fig. 10. Comparing Studies: summary of Rank Correlation (ocation) test (S)

had "earned the right" to divide the pie because they had won a current event contest – none of this was featured in Exchange. So the distribution of dictator giving appears to be conditional on the framing of the task as well as possibly the method of role selection. It is not clear what, if any, thread unites the task descriptions within each class.

6.3 Conclusions concerning the anonymity hypothesis

The game frame argument also suggests an alternative explanation to the anonymity effect claimed by Hoffman et al. That claim is based on the clear data shift between Double Blind 2, on the one hand, and Exchange and the Forsythe et al. treatments, on the other. The game frame argument suggests that part or all of the shift might be due to the different ways the treatments framed the decision task to the dictators, rather than to experimenter-subject anonymity. Framing the task as dividing a sum that had been "provisionally allocated to each pair," as did Forsythe et al., or as a market transaction, as did Exchange, may evoke a sense of obligation to others not evoked if the task is framed as an individual decision problem involving putting money in an envelope, as in the Double Blind treatments, or as a "game," as in our

	N	Mean	Median	Mode	Standard Deviation	Inter Quartile Range 25%	75%
POOL (BKZ)	110	1.35	1.0	0.00	1.61	0.00	2.00
CONTEST (HMSS)	24	1.25	1.0	0.00	1.29	0.00	2.00
BLIND2 (HMSS)	41	1.05	0.0	0.00	1.66	0.00	1.00
BLIND1 (HMSS)	36	0.92	0.0	0.00	1.47	0.00	1.00
FORS (FHSS)	24	2.33	2.5	3.00	1.79	1.00	3.00
EXCHANGE (HMSS)	24	2.67	3.0	3.00	1.66	1.00	4.00

EXCHANGE (HMSS) refers to "Random Exchange" cell in Hoffman et al. (1994)
BLIND1 (HMSS) refers to the first "Double Blind" cell in Hoffman et al. (1994)
BLIND2 (HMSS) refers to the second "Double Blind" cell in Hoffman et al. (1994)
CONTEST (HMSS) refers to the "Contest Exchange" cell in Hoffman et al. (1994)
FORS (FHSS) refers to the $10 Dictator cell in Forsythe et al. (1994)
POOL refers to our pooled 10Game-6Card, 10Game-2Card, 1Game-2Card and Anonymity data.

Fig. 11. Comparisons of central tendency and dispersion

study.[19] Where the frame evokes this sense of obligation, we might expect greater giving.

There is at this time no evidence that the anonymity hypothesis has substantive explanatory power when the game frame is held fixed. Of course, there may be some game frame in which the hypothesis will prove to have substantive explanatory power. Our results suggest, however, that any anonymity effect must be qualified with reference to context.

7 Dictator behavior as a rules-based decision procedure

It is useful to have a summary of the data regularities discussed in the previous section:

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[19] Our interpretation of the Exchange frame may seem counterintuitive to those accustomed to viewing an economic transaction context as an arena for unfettered self-interest. There is evidence, however, that the public does not always see it the same way. A survey of public attitudes by Kahneman, Knetsch and Thaler (1986), for example, finds that under certain circumstances exploitation of market power is perceived to be unfair, sometimes by a large majority. Given this, it seems quite plausible that placing a subject in the role of a seller who can force the buyer to buy at any price (as did Exchange) might lead the subject to associate taking a large portion of the money with the kind of monopolistic market practices that the subject himself considers unfair.

R1. Dictator giving exhibits considerable heterogeneity.

R2. Whether dividing a pie of $10 with one other, or dividing 10 pies of $1 with ten others, the resulting distribution of total dictator giving is the same.

R3. (I'm-no-saint) When given a choice of leaving an amount that is either greater or lower than he would freely choose, the dictator leaves lower.

R4. When dividing several small pies with many recipients, dictators tend to give gifts of various sizes, and distribute them among recipients in a capricious manner.

R5. The distribution of giving is a function of the game frame.

R6. The amount the dictator leaves is independent of experimenter observation (at least within the game frame studied here) and does not appear to be motivated by a desire to simply give a gift.

In this section, we describe a hypothetical decision procedure for a typical dictator that is consistent with all of the listed regularities. Construction of the procedure begins with the observation that all the regularities are consistent with the notion that dictators give for the purpose of implementing what they consider to be a fair distribution. Other motives examined do not appear to have significant influence (R6). On the other hand, rejection of the anonymity and rational giving hypotheses means we must posit a fresh (previously unconsidered) rationale for the observed concern for fair distribution.

7.1 Description of the dictator procedure

The intuition is as follows: Dictators exploit – or refrain from exploiting – strategic advantage in accordance with individual standards of conduct – 'rules' – which stipulate when and to what extent strategic advantage can be fairly used. These rules effectively define a dictator's 'fair share' of the pie. Some rules may be thought of as socially or culturally based in the sense that they are common to most all in the population, others may be quite individual-specific. The pertinence of any particular rule, however, is assumed to be dependent on the two elements of the game frame. The first is game context. Some who are comfortable with unmitigated strategic play in a contest context may not be comfortable with it in a context which emphasizes, say, group welfare or religious values. Second is the mechanism by which the strategically advantageous role of dictator was obtained. Some may feel that strategic advantage can be fairly used only if it was in some sense fairly gained.

More precisely, suppose a dictator is matched with one or more recipients. The proposed decision making procedure can be described as follows: The dictator first decides on his fair share α of the total pie k. The share α is at least half the total pie but depends on the role selection mechanism as well as on the context evoked by the game frame. The nature of this dependence is determined by potentially individual-specific rules and may therefore vary from dictator to dictator. The dictator keeps α for himself. If keeping α is not an option, he keeps more. If keeping more is not an option, he keeps as much as is allowed. He then distributes any remaining portion of the pie among the recipients, perhaps in a very uneven fashion.

As an example, consider 1Game-6Card where the total pie available is \$10. A dictator's appraisal of his fair share α is at least \$5. The exact value pivots on his having acquired the right to be Player A, in this case by a mechanism equivalent to a coin toss, in the context of a "game" between two "players." Some dictators assess the marginal increment due to them for winning the toss to be equal to half the pie; i.e., $\alpha = 10$. Others assess this value as positive but something less that half; i.e., α strictly between \$5 and \$10. Still others can not convince themselves that the luck of the toss entitles them to any additional increment at all; i.e., $\alpha = 5$.

7.2 Consistency with the data

For the most part, inspection will verify that the proposed procedure is consistent with the listed regularities, but certain comments are in order.

The procedure allows for gift heterogeneity (R1) and it does imply that the distribution for a given population is fixed by a selection of game frame. It does not say, however, what the distribution of giving should be for any particular frame. In order to make this sort of prediction, we would have to know which game frames evoke which rules. This relationship probably depends heavily on a description of cultural norms that is beyond the scope of this study.

On the other hand, the procedure does imply a set of comparative static predictions: Shifts in the distribution of dictator giving should be related to shifts in the game frame. For example, the game frame remained fixed across all treatments of the new experiment, so our procedure would predict that the distribution of dictator giving should remain fixed across treatments that place similar constraints on total dictator giving. This is consistent with the data (R2 and R6). As argued above, variations in the game frame appear sufficient to explain movements in distributions observed within and between previous experiments (R5).

The procedure is also consistent with the I'm-no-saint effect (R3). The constancy of the game frame across dictator treatments suggests that the distribution of what is considered a fair split should be stationary. The priority of goals stipulated in the dictator procedure then implies that if faced with a choice between giving more or less than he would freely choose, the dictator will choose less. Finally, the procedure is consistent with our finding that, apart from the case of giving nothing to everyone, equal treatment of recipients is the exception rather than the rule (R4).

7.3 Link with theories and phenomena previously reported in the literature

Components of the dictator procedure are present in models that have proven to have explanatory power in various lab situations. In addition, several investigators working in various settings, have demonstrated that a key component, the influence of the game frame, produces results consistent with those posited here.

Bolton (1991) analyses a "comparative model" designed to explain bargaining behavior in shrinking pie games, a class that includes the ultimatum game. This model is driven by the assumption that second movers are willing to pay a cost to obtain a division that they consider fair. In other words, second movers strive to obtain their fair share of the pie, similar to the dictator's first priority postulated by our decision procedure. Unlike the comparative model, however, the dictator procedure makes no mention of what cost the dictator is willing to pay to obtain his fair share. Specifying this was not necessary since the dictator is never confronted with a money-or-fairness tradeoff. It would do no harm, however, to add such considerations. So player objectives in the comparative model are basically consistent with the dictator procedure.[20]

Equity theory has a long history in the social psychology and sociology literatures (see Walster, Berscheid and Walster (1973) for an overview). Essentially, the theory asserts that a person will judge an exchange relationship to be "equitable," or fair, when the reward-to-contribution ratio is the same for each individual in the relationship. Exactly what constitutes a contribution depends on the circumstances of the exchange. If we interpret a dictator's contribution as the proportion of strategic power he can fairly use, then our procedure is consistent with equity theory.[21] Selten (1987) shows that equity theory, similarly modified to account for strategic power, can explain a substantial amount of behavior observed in a three-person coalition game experiment.

Consider now the game frame, composed of a selection mechanism and a context. Hoffman and Spritzer (1985) studied a two person division problem and found that players were more likely to exercise their strategic power if it was gained by winning a contest than if it was gained by a coin flip. Context effects have been demonstrated in many studies. For example, see Neale and Bazerman (1992) for an overview and references pertaining to bargaining experiments.

8 Conclusions

The dictator game experiment reported here led us to basically five conclusions. First, dictators determine how much money they should keep, and consequently how much they will give in gifts, on the basis of the total available for the entire experimental session, not on the basis of what is available per game. So dictators in the impunity study appear to be less generous than those

[20] Complete consistency requires that we modify the comparative model to reflect a propensity on the first mover's part to leave some money. Inspection of the comparative model should convince the reader that the basic results of the model are left unchanged by this modification as long as most first movers notion of a fair share split gives them more than the second mover's notion. The data suggests that this is indeed the case (see Forsythe et. al (1994)).

[21] Specifically, suppose that, on the basis of the game frame, the dictator assigns a non-negative weight to both dictator, w_d, and recipient, $w_r; w_d + w_r = 1$. The dictator then gives a gift of $w_r k$ to the recipient, keeping $w_d k$ for himself. Defining an individual's contribution to be equal to the assigned weight, the reward-to-contribution ratio is the same for both dictator and recipient (to avoid definitional ambiguity when one weight is zero, define the ratio $0 : 0$ as equal to k). So in this sense, the dictator procedure is consistent with equity theory.

in the dictator studies when considered on a per game basis; but when viewed on the basis of the entire experiment they appear equally, and therefore consistently, generous. Second, when distributing a gift among several recipients, individual dictators show little tendency towards equal treatment. Third, dictators appear to be primarily concerned with securing what they consider to be their fair share. When faced with the choice of leaving either more or less than they would freely choose, dictators choose less. This is the I'm-no-saint hypothesis. Fourth, we find no evidence for the anonymity hypothesis. Fifth, a comparison of our data with that of previous studies suggests that differences in the context of the game, affected by differences in written directions and independent of experimenter observation, account for the observed differences across dictator studies.

Also, by way of explaining dictator behavior, we presented a decision making procedure that is consistent with the data regularities found both in this study as well as the previous studies surveyed in section II. The procedure effectively synthesizes aspects of equity theory with aspects of the comparative bargaining model; both of the later have been found to have explanatory power in various lab situations. The dictator procedure, then, links dictator game behavior to a broader range of phenomena.

Our procedure suggests that dictator giving arises from a concern for fair distribution on the part of dictators. This is not to say that dictators give in order to improve the welfare of others. In our procedure, concerns for a fair distribution originate from personal and social rules that effectively constrain self-interested behavior – although within these constraints dictators *do* behave in a self-interested manner (they act first to secure what they consider to be their own fair share). What purpose these rules ultimately serve, whether it be to improve others' welfare or otherwise, is not clear from the data examined here. Identifying such a purpose will no doubt require much further empirical and theoretical study.

Appendix: Laboratory protocol

This section contains a record of the 1-Game sessions (1Game-2Card, 1Game-6Card, and Kindness). Virtually all statements made to participants (with the exception of answers to individual questions) appear in non-italicized print. Directions for the monitor appear in italicized print. Underlined text was replaced with bracketed text for the 10-Game sessions (10Game-2Card and 10Game-6Card). The text for the Anonymity sessions is marked as such, and is bold.

Seating. Upon entering the room, participants are seated. When all are seated (an even number), each is given a randomly chosen folder. Half the folders are red and half are blue.

Once the experiment has started no new participants are allowed in. May I have your attention please. We are ready to begin. Thank you for coming. Each of you will receive a five dollar show-up fee, to be paid in cash at the end of the session. You will now have a chance to earn additional money.

With the exception of the folder, please remove all materials from your desk. Open your folder and take out the sheet marked 'Instructions' together with the sheet marked 'Consent Form'. At this time would you please read the

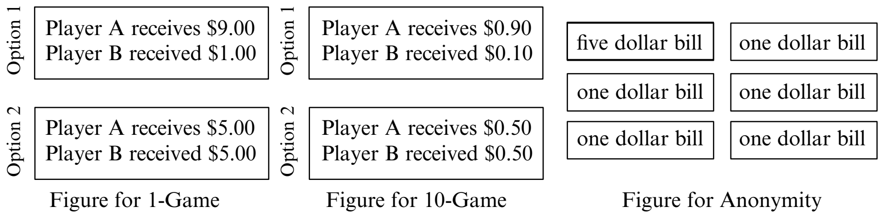

Fig. A1. Illustration of the game in the written instructions given to participants

instructions and the consent form. *Allow participants to read the instructions silently:*

Instructions

General. The purpose of this session is to study how people make decisions in a particular situation. Feel free to ask the monitor questions as they arise. From now until the end of the session, unauthorized communication of any nature with other participants is prohibited.

During the session you will participate in a game that gives you the opportunity to earn money. Immediately upon completion of the session we will pay you your total game earnings, in cash. Earnings are confidential: only you and the monitor will know the amount of money you make. (In the Anonymity cells the last sentence read: *Earnings are confidential*).

Description of the game: The game concerns two players, Player A and Player B, along with cards of the type displayed in the figure (note different figures for different sessions). Each card specifies a set of payments. For the actual game, the number of cards as well as the payment values may differ.

In the game, Player A chooses a card. Each player then receives the payment specified on the chosen card. For the figure: Player A chooses either the card labeled "Option 1" or the one labeled "Option 2."

If A chooses "Option 1," then A received *$9.00* [$0.90] and B received *$1.00* [$0.10].

If A chooses "Option 2," then A receives *$5.00* [$0.50] and B received *$5.00* [$0.50]

(In the Anonymity cells, the last two paragraphs read: *The game concerns two players, Player A and Player B, along with a set of bills like those displayed in the figure. For the actual game, the set of bills may differ.*

In the game, Player A chooses how much money, if any, to place in a box. Any combination of bills in and out of the box is allowed. Player B receives a payment equal to the money placed in the box. Player A receives a payment equal to the money not placed in the box. For example,

If A places $1 in the box, then A receives $9.00 and B receives $1.00.

If A places $5 in the box, then A receives $5.00 and B receives $5.00.)

Conduct of the Session: During this session you will play the game *one time* [ten times]. *You will be either a Player A or Player B* [You will have the same role, Player A or Player B, for all games]. Your role will be determined randomly after completion of the Consent Forms, *and you will be randomly matched with a partner* [You will play each game with a different player, never the same partner twice]. You will not know your partner's identity, nor will they know yours. Nor will these identities be revealed after the session is completed.

[Written instructions handed out to participants end here.] To make sure that we all understand, I will now read the instructions out loud. *Read instructions.*

Are there any questions? *Answer questions.* If you wish to participate in this study, please sign the accompanying consent form. We ask that you plan on staying until the end of the session, which will last about 30 minutes. We will now come around to collect the consent forms. *Collect filled-out consent forms.*

Role Selection. First, we shall determine who shall be Players A and who shall be Players B. I have here two cards, both identical on the side that is showing. *Cards are taped to the back of a piece of cardboard. Hold the cardboard up so that participants can see*. The reverse side of one card is marked 'A' and the other is marked 'B'. The participant nearest me will choose one card. S/He has a red (blue) folder. If the card s/he chooses is marked A, then all participants with a red (blue) folder will be Players A. If it is marked B then all participants with a red (blue) folder will be Players B. *Have a subject choose a card and announce role assignments*. The chosen card is A (B). All participants with a red (blue) folder are Players A, and all participants with a blue (red) folder are Players B. So that you know that the choice was fair, let me show you the other card. *Show the card not chosen by the subject*.

The game procedure is as follows: Players B are escorted to a waiting room. Each Player A then plays the game at one of the cubicles situated around this room. This procedure insures that Players A can play the game anonymously. Each Player A will be paid a $5 show-up fee plus his or her earnings from the game. Players A will then leave. After this, each Player B will see the choice of the Player A they have been matched with. Each Player B will be paid a $5 show-up fee plus his or her earnings from the game. Are there any questions?

At this time would all Players B, the participants with the red (blue) folders, please rise and follow the monitor to a waiting room. Take your personal things plus your folder with you.

Players B are taken to a waiting room. Each Player A is moved to a cubicle. On top of each cubicle is a tray holding the cards for the game together with a manila envelope. (In Anonymity cells, the tray is holding a box together with $10 (one $5 bill plus five $1 bills).

Once Players A are all seated at cubicles: Players A, please pull down the tray sitting atop your cubicle. On the tray you will find *a set of game cards. You must select one card. The card you select will determine your payment as well as that of the player B you are matched with.* [a Games Form together with a set of game cards for each of ten games. The cards are printed on the backs of labels. You must select one card for each game. The card you select will determine your payment as well as that of the Player B you are matched with for the game.]

At this time please select a *game card. Put the card you select into the empty manila envelope provided on the tray. Turn over the remaining card the one you did not choose, so that the blank side of the card is showing.* [game cards. Place the cards you select onto the Games Form as indicated on the form]. Are there any questions? When you are finished, please place you tray on top of your cubicle. *Pause until task is completed.*

Now take the check out form from your folder. Fill out the form. When you are finished please wait quietly. *Wait for all the trays to be placed on top of the cubicles.* I will come around to pay you. Once you have been paid you are free to leave. Thank you for participating.

(In Anonymity cells, the following was substituted for the last three paragraphs: *Players A, the game procedure is designed to insure that your decisions and the money you make are confidential. Neither other participants nor the monitors will know this information. At the conclusion of the session, you will not have to report your earnings to us.*

Players A, please pull down the tray sitting atop your cubicle. On the tray you will find a set of bills together with a box. You must decide how much money, if any, to put in the box. Any combination of bills in and out of the box is allowed. Your choice will determine your payment as well as that of the Player B you are matched with.

When you have made your decision, place the bills going to Player B into the box and close the lid. Place the bills going to you, Player A, into your pocket or purse or other safe place. When finished, insert the box into the Mail Bin located next to the door and pick up one of the envelopes next to the Bin. Each envelope contains a $5 cash show-up fee. You are then free to leave. Are there any questions?

Please follow the procedure. Thank you for participating.

Players A fill out check out forms. Monitor goes around the cubicles to make payments. (In the Anonymity cells, Players A drop off boxes, pick up the show-up fee, and leave). *Go to Players B room with envelopes containing game* cards [completed Games Forms] (in Anonymity cells boxes*). Distribute envelopes to Player B, one per player. [Sum up earnings for each player B] (In the Anonymity cells, distribute* boxes and Check Out forms to Players B, one box per player.)

Players B, *the envelope you have been handed contains the game card selected by the Player A you have been matched with. You may open the envelope and look at the card. When you are finished, please return the card to the envelope. Pause.*

Now take the Check Out form from your folder. Fill out the form. [take the Check Out form from your folder. Fill out all the items on the form except for the payment section.] When you are finished please wait quietly. I will come around to pay you. Once you have been paid you are free to leave.

(For the Anoymity cells, the following was substituted for the two previous paragraphs: *Players B, the box you will be handed contains the amount of money left to you by the Player A you have been matched with. I will come around to give you your box and to pay you. In the mean time, please complete all items on the Check Out form. Leave the total amount of money earned blank until I give you your box. Once you have been paid you are free to leave.*) Thank you for participating.

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