

**Other-Regarding Preferences:
Egalitarian Warm Glow, Empathy,
and Group Size**

by

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ABSTRACT

An abundance of experimental evidence in recent years has demonstrated the existence of other-regarding behavior in the laboratory. In a series of dictator experiments, we investigate the effects of warm-glow egalitarianism, empathy, and their interaction with group size. We find that the random-payoff method may distort incentives in favor of egalitarian behavior by allowing for path-dependent egalitarian warm glow to outweigh monetary payoffs. Though we find some evidence for empathy, it nevertheless appears that a group size of six or more neutralizes most other-regarding behavior in many-recipient dictator games.

1. Introduction.

The empirical content of game theory crucially depends on knowing the utility payoffs of the players. In recent years, experimental studies have cast doubt on the appropriateness of strictly individualistic preferences (e.g., Guth, Schmittberger and Schwarz, 1982; Andreoni, 1988; Thaler, 1988; Berg, Dichaut and McCabe, 1995; Charness, 1996; Fehr, Kirchler, Weichbold, and Gächter, 1998; Charness and Rabin, 2002). It appears that in some settings, a human player may not be indifferent to the welfare of an anonymous stranger, particularly when his or her choice directly determines the other's welfare¹. On the other hand, many celebrated experiments are based on the maintained hypothesis that the participants care only about their own monetary payoffs. How do we reconcile these two sets of findings? Specifically, how do we accept the other-regarding evidence and at the same time accept that other tests are not invalidated by other-regarding preferences of the participants?

One possibility is that *group size* effects in experiments, in which a large number of participants affect the final outcome, lower individual responsibility for the distribution of outcomes, and make other-regarding preferences negligible. Experiments with many subjects interacting anonymously in a manner that determines the final outcome are fairly common. Market experiments, auction experiments, and some public goods experiments generally involve a many-player design. Similarly, in many other games, players' payoffs are determined by a summary statistic of the entire population's play (e.g., Stahl and Wilson, 1994, 1995; Nagel, 1995; Duffy and Nagel, 1997; Van Huyck, Battalio and Beil, 1990, 1991; Anderson, Goeree and Holt, 2001; Ho, Camerer and Weigelt, 1998, Costa-Gomes, Crawford, and Broseta, 2001).

Incentives related to group size could reduce other-regarding preferences in two related ways. The first effect of group size on other-regarding behavior, which is one focus of this study, is that as the group size increases, the marginal rate of transformation of own income for another participant's income declines. Specifically, in experiments with $n+1$ participants, where the contribution is diffused so that one participant has only a $1/n^{\text{th}}$ effect on another participant, other-regarding behavior might be substantially reduced, or possibly eliminated.

The second effect, which is not a factor in the present study, has to do with the diffusion of responsibility (Darley and Latane, 1968). When individuals know that many others are present,

¹ Charness (2000) showed that if a random mechanism partially affects the outcome, an individual may care less for the welfare of the other player.

then they as individuals do not bear the full burden of responsibility. They make the assumption that someone else will bear the sacrifice that needs to be taken. This concept has been applied to public good contributions (Fleishman, 1980; Barron and Yechiam, 2002). Data from experiments on public goods contributions, unfortunately, cannot separate out the diffusion of responsibility argument from the diminished marginal rate of substitution argument. Both are clearly present and are related to group size in roughly the same way. In contrast, the many-recipient dictator game, which we study here, provides an opportunity to study the latter without the need to account for diffusion of responsibility because the dictator cannot expect others to bear the burden of the social action.

A larger group size, however, is not guaranteed to reduce other-regarding behavior under all experimental procedures, and as we show, increasing group size may actually increase other-regarding behavior. This happens in the random payoff experimental procedure, where one individual's action is selected at random to determine group payoffs *after* the choices have been made, thereby lowering the chance that an individual's action will affect the outcome. In that case, as the group size increases, the individual can choose a 'kind action' while incurring the cost of that action with a smaller probability. The psychological experiences along the path (in this case, the warm glow from the action itself) can matter, as well as the final payoffs. To see this more clearly, suppose an individual must make two choices, each from a set of two actions, with one action being "kind" and the other "selfish"; thus there are four possible choice pairs, each defining a path from the vertex of the decision tree to a node of Nature. At the node of Nature, one of the two choices is randomly selected with equal probability, and the payoff of that choice is implemented. The expected utility at this terminal node is a function of the monetary payoff as well as any non-pecuniary aspects of the path (i.e. the kindness of the two choices). Hence, the kindness of a choice can have a positive effect whether or not the choice is actually implemented, whereas the monetary payoff has an effect only if the choice is implemented. We will refer to such effects other than monetary payoffs as *path-dependent* effects and will show that this path-dependence is related to group size.

Other types of experimental procedures that can result in increased other-regarding behavior are procedures that generate empathy. Such procedures are typically implemented to increase the number of observations. For example, asking each participant to state decisions for

different roles before the role is determined could cause a participant to consider players in different roles when making the decision for a particular role. A related debate in the experimental literature relates to the technique of role reversal to increase the number of observations. While the technique is common and widely accepted (e.g., Charness and Rabin, 2002), Weimann and Reichmann (2003) find that role reversal may significantly affect behavior. In accordance, we closely examine the issue of empathy in our design.

Studies on altruism in varying group size have yielded a confusing set of results. Bolton, Katok and Zwick (1998) find evidence for other-regarding behavior with **ten** participants. Isaac and Walker (1988), on the other hand, examined the relationship between variations in group size and free-riding behavior in voluntary provision of public goods. Their findings strongly indicated that increasing the group size from **four** to **ten**, without a change in the public good production function, unequivocally led to increased free riding as such a group size increase resulted in a decrease in the marginal per capita return to an individual from contribution to the public good. That is, when one's average impact on another individual subject declines, so does his or her propensity to behave in an other-regarding manner.

One experimental protocol for which other-regarding preferences might be reduced along these lines is *mean-matching*. In that protocol, each participant receives the mean payoff from being matched with every other participant. If the average impact participants have on others is declining in the group size (i.e., number of other participants with whom they are matched), then other-regarding behavior should also decline as group size increases. The practical question is whether other-regarding behavior is negligible for group sizes that are feasible in laboratory experiments.

Section 2 presents a theoretical framework. Section 3 presents the experiments; section 3.1 gives an overview of the experimental designs; section 3.2 presents the first study with several many-recipient dictator games and varying group sizes; section 3.3 presents the second study with a five-recipient dictator game and *ex ante* selection of the dictator; section 3.4 presents the third study with a three recipient dictator game and *ex ante* selection of the dictator; and section 3.5 presents the fourth study with one five-recipient dictator game and *ex post* selection of the dictator. Finally, Section 4 draws conclusions.

2. Theoretical Framework.

Let T denote a finite *extensive-form* game tree, with properly labeled nodes and branches, and a partition of the nodes into information sets. As part of T , let Z denote the set of terminal nodes. Let I denote the set of players; and let $s_i \in S_i$ denote a pure strategy for player $i \in I$. A profile of pure strategies, $s = (s_i, s_{-i})$, generates a unique path from the vertex to a terminal node; let $z(s)$ denote this mapping. Finally, let $u_i : Z \rightarrow \Re$ denote the payoff function for player i ; and let $u \equiv \{u_i\}_{i \in I}$. (T, u) is an *extensive-form game*.

Assumption 1. Each player $i \in I$ seeks to maximize the expected value of $u_i()$.

Assumption 2. The history before T and the future after T is irrelevant.

The payoff-relevant consequences entailed by the path from the vertex to a terminal node z include the vector of monetary payoffs, $x(z) = \{x_i(z)\}_{i \in I}$, to the players. Other possible payoff-relevant consequences are the psychological experiences along the path (such as anxiety, excitement, anger, gratitude, etc.). A player's preference could also depend on the precise time of events along a path (e.g. time preference for a variable stream of monetary payoffs). We will refer to all of these effects other than monetary payoffs as *path-dependent* effects. Figure 1 below provides an example of path dependence.

Figure 1. An example of path dependence.

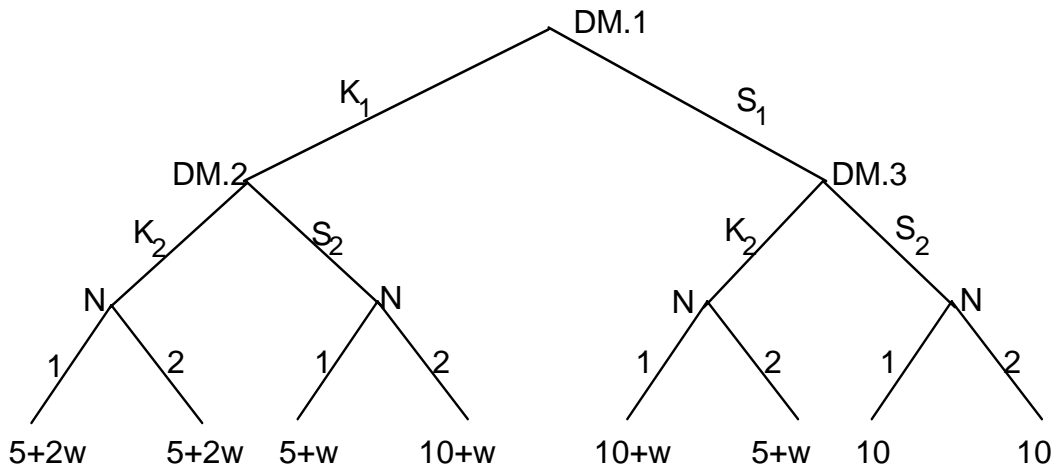


Figure 1 depicts an example of the path-dependence effect. In this example, a Decision Maker 1 is facing two dictator decisions sequentially. At each decision node, the dictator is facing a choice between Selfish (S) assignment (keeping \$10 for himself), and a Kind (K) assignment (splitting the \$10 equally with a passive recipient). After the Decision Maker choices, Nature selects only one of the two decisions to be implemented, with equal probability. A choice of 1 by Nature means the first decision is to be implemented and a choice of 2 by Nature means that the second decision is to be implemented. Path dependence implies that the warm glow, w , from choosing a kind action is received, regardless of whether the decision is picked for payment or not. As such, a kind action (K_1 or K_2) yields an unconditional payoff of w in addition to the conditional payoff. Since Nature selects 1 or 2 with equal probability, the expected direct cost of kindness is $\frac{1}{2}(10-5)$, while the benefit is w . If w is greater than 2.5, the kind action would be preferred to the selfish action. If we extend the above diagram to 15 decisions (as in our first study), with each equally likely to be chosen, a kind action still receives a warm glow of w , but costs only $1/15 \times 5$, so if w is greater than $1/3$, the kind action yields a higher expected payoff than the selfish action.

Often, especially in laboratory experiments, the monetary payoffs are assumed to be the only relevant consequence. We express this absence of path-dependent effects in the following assumption.

Assumption 3. $u_i(z) = u_i(x(z))$, for all $i \in I$.

Since $x(z)$ entails the distribution of monetary payoffs across players, Assumptions 1-3 permit *other-regarding* preferences. Moreover, the utility functions $u_i(x(z))$ completely express those other-regarding preferences; in other words, if we have correctly specified $\{u_i(x(z))\}_{i \in I}$, there is no need to contemplate whether player i cares about u_j for some $j \neq i$.² The next assumption formerly rules out other-regarding preferences, or equivalently assumes preferences are *individualistic*:

Assumption 4. $u_i(z) = u_i[x_i(z)]$, for all $i \in I$.

Note that formal game theory invokes only Assumption 1. However, virtually all applications of game theory (to the extent that testable predictions are required) invoke Assumptions 2-3 and typically Assumption 4 as well.³ While we know from experimental evidence that Assumption 4 does not hold for some games (e.g. Ultimatum and Dictator), free-riding behavior could make Assumption 4 a reasonable approximation for many-recipient games, as we show in the following subsections.

2.1. Many-Recipient Dictator Games.

M dollars are to be distributed among $n+1$ participants (including the dictator). One dictator (player 1) decides to keep x_1 dollars, and the balance $(M-x_1)$ is multiplied by $m > 0$ and equally divided among the other n participants. Given Assumptions 1-3, the dictator solves:

$$\max u_1[x_1, m(M-x_1)/n, \dots, m(M-x_1)/n]$$

For notational convenience we let $\beta(n)$ denote the average marginal rate of substitution of income of another participant ($i \neq 1$) for your own income (x_1):

$$\beta(n) \equiv \frac{1}{n} \sum_{i \neq 1} MRS_{i1}, \quad (1)$$

where $MRS_{i1} = (\partial u_i / \partial x_i) / (\partial u_1 / \partial x_1)$. Then, an interior solution implies that

$$\beta(n) = 1/m, \quad (2)$$

If the dictator has other-regarding preferences, then $MRS_{i1} > 0$ (at $x_1 = M$) for some other participant, but this is not necessarily sufficient for positive giving (i.e. $x_1 < M$). If $\beta(n) > 0$, then there will be positive giving for sufficiently large m . While we could have $MRS_{i1} > 1$ (at $x_1 = M$) for some others (say relatives and close friends), we can safely assume this is not true for all people on the planet; hence (1) will eventually fail for large enough n . If $\beta(n)$ is decreasing in n , the amount kept by the dictator, x_1 , should increase with n (up to M). We want to know how

² If one did specify other-regarding preferences recursively as $v_i(x_i, v_{-i})$, then we could define $u_i(x)$ as a fixed point of $v(x, v)$. However, there is no guarantee that there would be a unique fixed point, nor that $u_i(\cdot)$ would be continuous; and these problems would be major obstacles to analysis of the game.

large n has to be in a typical laboratory experiment among unrelated students before $x_1 = M$? The effect of the multiplier m on x_1 is ambiguous. Because increasing m lowers the effective price of giving, the substitution effect is negative. On the other hand, assuming own-income is a normal good, the income effect would be positive.

3. The Experiments.

3.1. An Overview of the Experimental Designs.

We designed and conducted four experimental studies using many-recipient dictator games. The four studies, their features and their purpose are summarized in Table 1 below.

Table 1. The Four Studies.

Study	(n,m,M)	Dictator Selection	Purpose
Study 1	$n \in \{1, 3, 7, 11, 23\}$; $m \in \{1, 2, n\}$; $M \in \{80, 40, 20\}$.	Ex-post	Allows mapping n to $\beta(n)$
Study 2	$n=5$; $m=1$; $M=\$20$	Ex-ante	Eliminates empathy
Study 3	$n=2$; $m=1$; $M=\$10$	Ex-ante	Eliminates empathy
Study 4	$n=5$; $m=1$; $M=\$75$	Ex-post	Isolates empathy from warm glow

n = the number of recipients; m = multiplier of the amount not kept; M = Endowment to dictator,

3.2. Study 1: Many-Recipient Dictator Games and Group Size

3.2.1. The Design

The basic decision task is the above many-recipient dictator game. We ran a 5×3 treatment design in which $n \in \{1, 3, 7, 11, 23\}$, and $m \in \{1, 2, n\}$. The amount to be divided depended on the multiplier: M was \$80 when $m = 1$, \$40 when $m = 2$, and \$20 when $m = n$. We chose this within-subject design to test consistency of behavior with the theoretical requirement

³ Experiments to test specific theories of other-regarding preferences (e.g., Bolton and Ockenfels, 2000, Fehr and Schmidt, 1999; Charness and Rabin, 2002) obviously do not invoke Assumption 4.

that $\beta(n) = 1/m$. Each treatment was presented once and only once, and one and only one of the fifteen treatments was randomly chosen at the end to determine the actual distribution. In addition, each participant received \$10 "as compensation for participating". Our reason for using a substantial "show-up" fee was to make sure no one left the experiment worse off than when they came in, and to make this fact common knowledge so no one would feel obliged to give money away simply to compensate other participants for their lost time.

Each experiment session had 24 subjects from the University of Texas recruited by flyers from the upper division and graduate student population, excluding graduate students in Economics. When $n=1$, we had 12 groups of two; when $n=3$, we had 6 groups of four; etc. Participants made their decisions at a computer terminal. Figure 2 shows what they saw on the computer screen.

[Insert Figure 2 about here.]

All subjects were instructed to respond *as if* they were the dictator for each treatment. Thus, we have 24 observations per treatment per session. Two sessions were run, for a total of 48 observations per treatment. After all 15 decisions were made, one of the 15 treatments was randomly chosen (by roll of dice), and then for each group a dictator was randomly chosen with equal probability.⁴ This procedure is called the *random-payoff method*.⁵ The random assignment of participants to groups, the random selection of the dictators, and absence of individually signed payment receipts⁶ allowed us to maintain absolute anonymity; our instructions made this salient to the participants.

This design can test the following hypotheses.

1. For a fixed m and M , $\beta(n)$ is decreasing in n , so x_1 should be increasing in n .
2. For $m=1$, $x_1 = M$ for some $n \leq 23$.

⁴ Potential assignments to groups and selection of the dictator was predetermined by the ID of the computer terminal, but because of the random assignment of participants to terminals, and the random assignment of computer ID numbers by our java program, each participant was *ex ante* equally likely to be selected as the dictator in whatever group they ended up in.

⁵ Note that in the extensive form, each participant has a single information set, so it would be a misuse of terms to call this the "strategy method", which is widely used for games in which some players have multiple information sets and are asked to make a contingent choice for each information set before the game is actually played.

⁶ As a further step to ensure privacy, the assistant who passed out sealed payment envelopes was not the assistant who put money in those envelopes.

Figure 2. The computer screen

Participant 1

DECISION 6

Assume you have ended up in a group of size 2, and that you have been randomly selected to be the decision maker for a bonus of \$40.0.

Specifically, you will decide how much of \$40.0 to keep for yourself and the amount you do not keep will be multiplied by 2 and divided equally among the others in your group.

These amounts plus the \$10 show up payment will determine the total amount of money everyone in your group goes home with.

Group size = 2

Bonus = \$40.0

Multiplier = 2.0

You choose to keep \$ leaving \$ which will be multiplied by giving each of the others in your group \$

CONFIRM YOUR CHOICE

CLOCK: **RECORD**

	A	B	C
2	Decision 1	Decision 6	Decision 11
4	Decision 2	Decision 7	Decision 12
8	Decision 3	Decision 8	Decision 13
12	Decision 4	Decision 9	Decision 14
24	Decision 5	Decision 10	Decision 15

3.2.2 The Data.

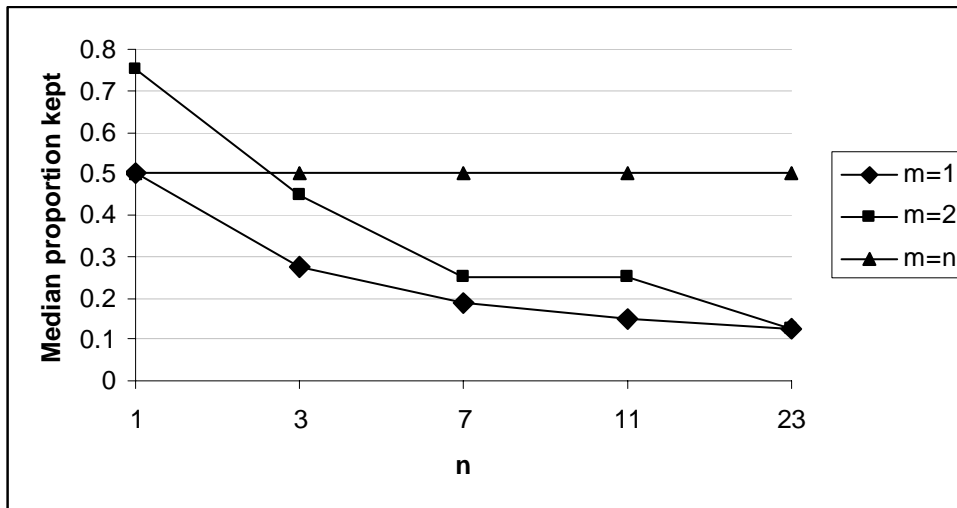
Figure 3 displays the median amount kept as a proportion of the pot. Clearly, our hypotheses are rejected. Contrary to the first hypothesis, x_1 is decreasing in n for $m=1$ and $m=2$. This finding suggests that $\beta(n)$ is not decreasing in n . Moreover, the median x_1/M is strictly less than 1 for all treatments, contrary to the second hypothesis.

For the $m = n$ treatment, the median fraction of M kept is exactly $1/2$ for all group sizes. This is, of course, the egalitarian division in which all members of the group receive the same amount. Indeed, the median amount kept in all treatments conforms well with the egalitarian solution:

$$x_1/M = m/(n+m) .$$

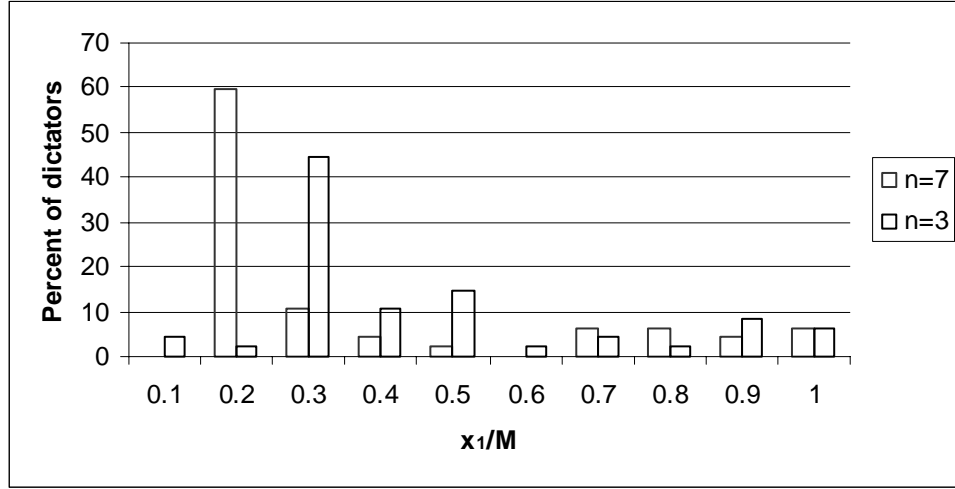
Finally, Figure 3 reveals that x_1/M consistently increases with m for all treatments. Thus, the income effect of m dominates the substitution effect.

Figure 3. Median x_1/M kept



For comparison with data from the next two experiments, Figure 4 displays the distribution of the proportion kept (x_1/M) for the treatments with $M = \$80$, $m = 1$ and $n=3$ and 7.

Figure 4. Distribution of Proportion Kept by Dictators in Study 1, $m=1$.



3.2.3 Analysis.

What are possible explanations for the falsification of our hypotheses? Is it really the case that $\beta(n)$ is increasing in n over this range, or are our results an artifact of the experimental procedure, in particular the random-payoff method? In a similar setting, Sefton (1992) found that the random-payoff method produced significantly different results for the standard two-person dictator game. Using the random-payoff method, he found the egalitarian solution to be the modal response, whereas selecting the dictator and informing that participant that s/he is the dictator before the choice yielded dramatically more individualistic dictator behavior.

We suggest a theoretical path-dependency explanation (in violation of Assumption 3) of why the random-payoff method yields significantly different results, which we will call *warm-glow egalitarianism*. In contrast to the standard notion of altruism as a preference that puts some weight on the payoffs of others (termed “pure altruism” by Andreoni, 1990; Dawes and Thaler, 1988), the notion of “impure altruism,” or warm glow (also investigated by Andreoni, 1990; Dawes and Thaler, 1988) suggests a warm glow that comes with “doing the right thing”. Impure altruism, or warm glow, is generally described as “satisfaction of conscience, or of noninstrumental ethical mandates” (Dawes and Thaler, 1988). Anderoni (1990) modeled it as increasing in the level of contribution, without regard to final outcomes.

Suppose a participant in a dictator game receives a warm-glow utility benefit, w , from being egalitarian. This benefit is compared to the expected cost. In a single n -recipient dictator game, this expected cost is $Mn/(m+n)$. However, when the random-payoff method is used, the

expected cost is reduced by the probability, p , of any given decision being selected for actual payoffs but the warm glow is unattenuated. If $w > pMn/(m+n)$, then the egalitarian solution is optimal; otherwise, the individualistic solution is optimal. In Sefton's experiment with $M = \$5$, when p was $1/4$, the expected cost of being egalitarian was only $\$0.625$, and 75% of the dictators were egalitarian. In contrast, when p was 1, the expected cost of being egalitarian was $\$2.50$, and only about 20% were egalitarian. Thus, an egalitarian warm glow between $\$0.65$ and $\$2.50$ would be consistent with Sefton's data. In our experiment, $p = 1/(15(n+1))$, so the expected cost of egalitarianism was $Mn/(15(n+1)(m+n))$, which never exceeded $\$1.33$; thus, an egalitarian warm glow of $\$1.33$ or more would be consistent with our data.

A second alternative explanation (in violation of Assumption 4) is *empathy*. That is, since each participant is most likely a *recipient* rather than a dictator, the participant identifies with the recipient, invoking other-regarding preferences which otherwise might have been latent. However, Bolton and Katok (1998) do not find evidence for this empathy explanation in a modified dictator game. In their experiments, there was an initial allocation of $\$15$ to the dictator and $\$5$ to the recipient). In most sessions, all participants were asked to make choices as if they were the dictator with the actual selection of the dictator determined by a coin flip after the participants made their choices, but in one session, the dictator role was selected before choices were made. There were no significant differences in these treatments: about 45% chose the initial allocation, while about 33% chose the egalitarian split. Evidently, having a 50% chance of being a recipient did not invoke more empathy, whereas, if empathy accounts for Sefton's data, then having a 75% chance of being a recipient does invoke more empathy.

In Bolton and Katok's treatments, the expected cost of being egalitarian was $\$2.50$ and $\$5.00$ respectively; hence, a warm-glow utility benefit less than $\$2.50$ would be compatible with no treatment effect (consistent with our warm-glow explanation of Sefton's data). This higher expected cost could also explain why, despite Bolton and Katok's framing of the game as a "distribution task" with "provisional distributions", the median amount kept was substantially larger than in Sefton's random treatment and in our treatment with $n=1$ and $m=1$.

3.3. Study 2: Five-Recipient Dictator Game with *Ex Ante* Selection.

3.3.1. The Design.

Section 4.3 discussed path dependence and empathy as possible reasons for the finding of declined generosity with larger n . To test whether our results for the many-recipient dictator games are strongly affected by path dependence, empathy or other artifacts of the design, we eliminated both the within subject design and the random-payoff method by asking each dictator for a single choice and by designating the dictator before the participants made their choices. This is essentially the standard dictator game design, extended to multiple recipients. This will neutralize the potential empathy effect. Without the random-payoff method, the within-subject design must also be abandoned; otherwise, other-regarding preferences can be satisfied through portfolios of individualistic decisions.⁷ Thus, we had to choose one set of treatment parameters (m , n , M). Since the first experiment had significant egalitarianism even with a multiplier of 1, and since the standard dictator game has $m = 1$, we chose $m = 1$. The obvious candidates for n were $\{1, 3, 7, 11 \text{ and } 23\}$ from the first experiment. We eliminated $n = 1$ because others have already conducted that experiment. We eliminated $n \geq 11$ because those group sizes are already too large to be practical for experiments that require several groups. Noting the monotonic effect of n revealed by Figures 3 and 4, we felt free to pick intermediate values for n between 3 and 7; specifically 5. To find a single M that would be comparable to the first experiment, we computed the average pot size in the first experiment weighted by the probability of being the dictator; it was \$16. We rounded this up to \$20. The cost of egalitarianism is \$16.66, which makes the warm-glow effect much less plausible. In summary, we chose a single treatment with $m = 1$, $n = 5$, and $M = \$20$.

We ran four sessions with 36, 36, 36 and 30 participants respectively in the Experimental Laboratory at the Harvard Business School. Participants were paid \$5 just for participating and an additional \$5 for showing up early. Due to Harvard regulations, each participant had to sign an individual payment receipt. Since such receipts can reveal to us, the experimenters, the identity of the dictators and their choices, and therefore potentially introduce experimenter bias, we preceded the dictator game with a simple lottery for each participant with a prize of \$20 and a 1/10 chance of winning. The outcomes of these lotteries were not revealed until the end of the

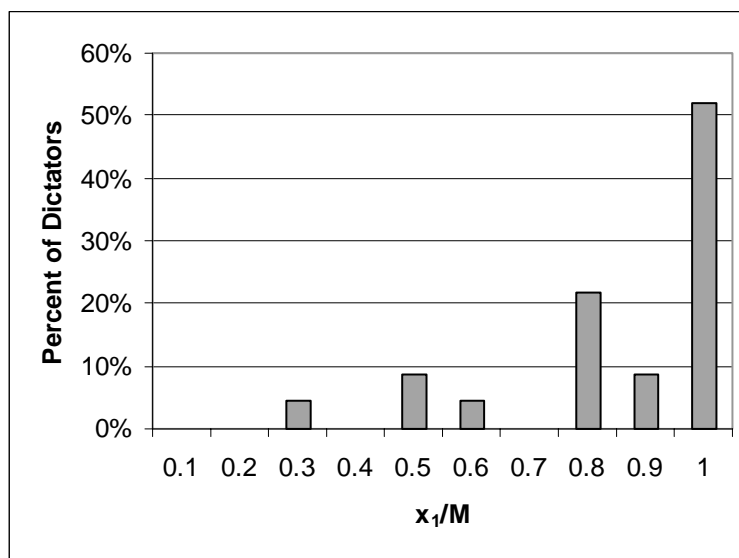
⁷ E.g. if each subject makes a dictator decision, then 100% selfish behavior by everyone will result in *ex post* equal payoffs which is consistent with egalitarian preferences.

experiment, at which time, counting the show-up payments and the lottery, but not the dictator game, participants could have earned \$5, \$10, \$25 or \$30. Thus, when adding the results of the dictator game to these payments it would be impossible to identify which participants were the dictators and what choices they made. This anonymity protection was explained to the participants.

3.3.2. The Data.

Figure 5 shows the distribution of the proportion kept by the 23 dictators in this five-recipient dictator game. The contrast with our first experiment (Figure 4) is dramatic. Over 83% keep \$15 (0.75) or more. Recalling that keeping \$15 means giving only \$1 to each other member of the group, we are comfortable in interpreting this as essentially individualistic behavior. While the egalitarian solution was predominant in the first experiment, now no dictator chose to keep that amount or less; the smallest amount kept was \$5 (0.25) by only one dictator.

Figure 5. Distribution of Proportion Kept by Dictators in Study 2



3.3.3. Analysis.

First, the dramatic difference in the proportion of players choosing egalitarian behavior between the two-recipient and five-recipient treatments strongly suggests that group size matters and that it reduces egalitarian preferences significantly; in other words, $\beta(5) < 1$.

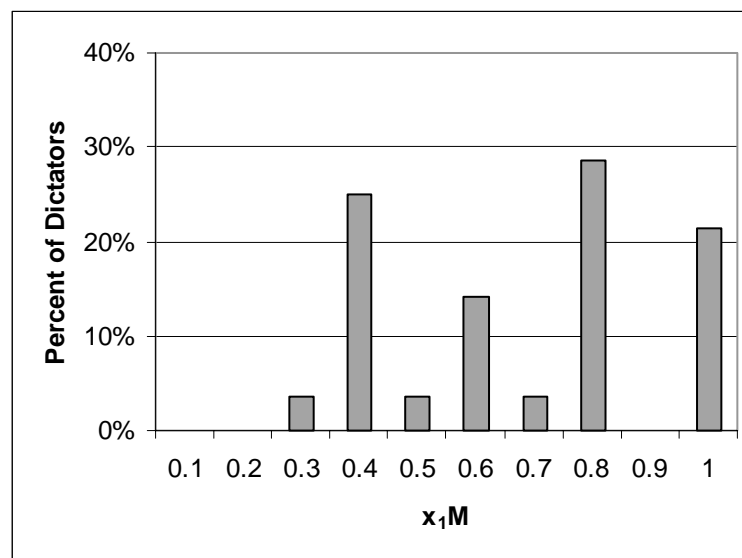
The stark contrast in the distribution of the proportion kept between the first experiment—the random payoff design-- and the second experiment—the five recipient treatment-- shows that the experiment procedure matters. When the random-payoff method was used and the probability that any one decision would matter was only $1/(15(n+1))$, the participants were predominantly egalitarian; but when the dictator was selected prior to choices and the dictator had only one decision task, the dictators were predominantly individualistic in this five-recipient design.

The warm-glow effect is not plausible in this experiment because the expected cost of egalitarianism was \$16.66, substantially larger than any previous estimates (between \$1 and \$2.50). Also, the empathy effect is not plausible because each dictator was selected before they had any opportunity to think about the decision, so it is unlikely that they thought about the possibility of being a recipient.

3.4. Study 3: Three-Recipient Dictator Game with *Ex Ante* Selection.

The design was the same as for the second experiment, except $n = 2$ and $M = \$10$ (m is 1). Two sessions with 18 participants each and two session with 24 participants each were run at UT Austin. Each participant received \$5 "as compensation for participating." Figure 6 shows the distribution of the proportion kept by the 28 dictators in this two-recipient dictator game.

Figure 6. Distribution of Proportion Kept by Dictators in Study 3.



In contrast to the five-recipient dictator game reported in the previous section, the two-recipient dictator game has a strong mode close to the egalitarian outcome (0.33). In other words, with a group size of three there is a significant amount of other-regarding behavior.

The warm-glow effect is not plausible in this experiment because the expected cost of egalitarianism was \$6.67, substantially larger than any previous estimates (between \$1 and \$2.50). Also, the empathy effect is not plausible because each dictator was selected before they had any opportunity to think about the decision, so it is unlikely that they thought about the possibility of being a recipient. We can infer that $\beta(2) > 1$. It remains to determine which effect by itself (if any) is responsible for the egalitarian behavior of our first experiment.

3.5. Study 4: Five-Recipient Dictator Game with *Ex Post* Selection.

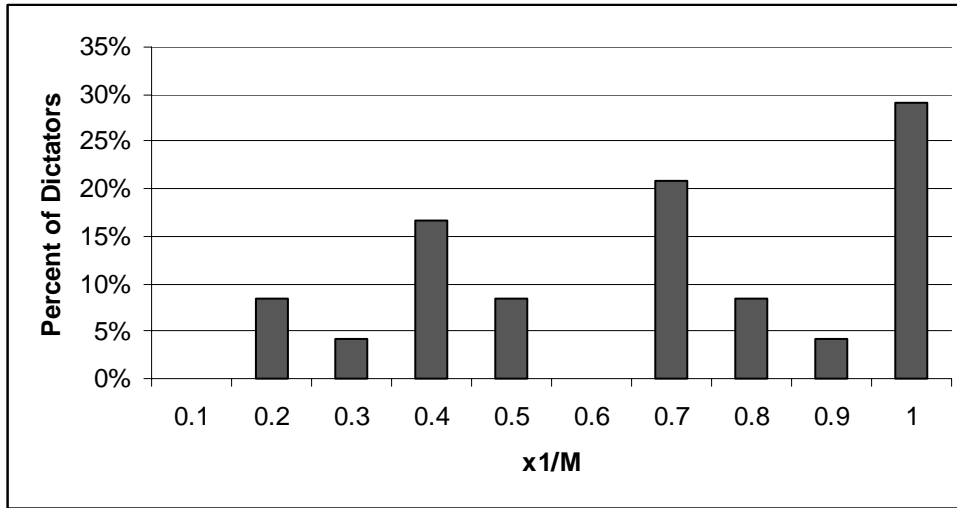
3.5.1. The Design.

We wanted a design for which only the warm-glow effect or the empathy effect is plausible, but not both. We decided to eliminate the warm glow effect while preserving empathy. For that purpose, we needed a treatment with an ex-post selection of the dictator (to create empathy) but with large enough incentives to make warm glow effects insignificant. We conducted a five-recipient dictator experiment consisting of 24 participants from the University of Texas, recruited as for the first experiment, and randomly assigned to 4 groups of 6 participants each. Each participant received \$10 "as compensation for participating." Every participant was asked to report what they would keep if they were randomly selected as the dictator. The actual dictator for each group was randomly selected after all decisions were made. Absolute anonymity was maintained. The multiplier was $m = 1$, and the pot to be divided was \$75, making the expected cost of egalitarianism equal to $1/6 (\$75 - \$12.50) \approx \$10.42$. With this high expected cost, the warm-glow explanation is not plausible; however, with each participant most likely being a recipient, the empathy explanation could apply. Also given our results for experiment 2, we expect $\beta(5) < 1$.

3.5.2. The Data.

Figure 7 shows distribution of the proportion kept by the each participant (as the dictator). The modal response was to keep the whole \$75, while no participant chose the egalitarian amount or less. The least proportion kept (by only two participants) was 0.20.

Figure 7. Distribution of Proportion Kept in Study 4



The majority (62.5%) kept \$50 (0.67) or more, giving \$5 or less to each of the other members of their group. The median proportion kept was also 0.67.

3.5.3. Analysis.

Compared with the first experiment (Figure 4), there was far less other-regarding behavior; in particular far less egalitarianism. Thus, it would seem that the empathy explanation cannot account for the predominance of egalitarian behavior in the first experiment.

Compared with the second experiment (Figure 5), there was insignificantly more near-egalitarian behavior (8% *vs.* 4%), but substantially less individualistic behavior (29% *vs.* 83%) when defining "individualistic" as giving no more than \$1 to each other member of the group. Even if we use a relativistic standard of giving no more than 5% to each other member, only 42% were relatively individualistic, half that observed in the second experiment. The other-regarding participants typically gave 10% to 15% per other member. Empathy is the likely explanation for these differences. This is because, with an expected cost of \$10.42 for being egalitarian, the warm-glow explanation cannot account for these differences.

4. Discussion.

Though other-regarding preferences are clearly important to understand and study, they are not always a desired feature in experiments. Rigorous tests of game theory predictions conditional on individual expected utility payoffs require experimental designs that marginalize

or eliminate other-regarding preferences. One idea that experimenters implicitly subscribe to is that as the group size increases, the marginal rate of substitution of income for another participant declines. To separate this explanation from other possibilities (like diffusion of responsibility), several variations on the multi-recipient dictator game were run. In accordance with the hypothesis, we observe much less giving in a five-recipient dictator game relative to a two-recipient dictator game when the dictator is announced ahead of time instead of drawn randomly after decisions are already submitted. The results reported here support that larger group sizes may minimize, and possibly eliminate, other-regarding preferences. This conclusion is useful in the design of future experiments and in dissipating doubt on the findings of past experiments.

However, empathy and warm glow can more than offset the group size effect when the subject's role is determined following the choices. In such cases, the larger group size actually reduces the probability that the decision maker's choice will determine the outcome. In that case, the decision maker can receive the warm glow from choosing a generous action with a low probability of incurring the associated cost. In terms of the assumptions needed to give game theory empirical context, our experiments provide clear evidence that the *path* can matter (in particular through warm-glow effects). Selecting one decision out of many to determine monetary payoffs can drastically reduce the importance of terminal payoffs relative to *path-dependent effects*. Since the "random-payoff method" for extensive-form games is open to path-dependency effects, many experimental conclusions should be re-evaluated.

We investigated two kinds of path-dependent effects: warm-glow egalitarianism and empathy. The former hypothesizes that making an egalitarian decision produces a latent increment in utility whether or not the decision is actually realized. The empathy hypothesis assumes that the weight given to others' monetary payoffs increases with the probability that you may be a passive recipient. While empathy can explain the giving behavior in our third experiment, it is not sufficient to explain the predominance of egalitarianism in our first experiment, where we infer that the warm-glow effects were predominant.

For a single, isolated five-recipient dictator game, when terminal payoffs are high enough to overwhelm potential path effects, and when the dictator is chosen before eliciting choices, dictator preferences appear to be essentially individualistic, equivalently, $\beta(5) < 1$. Moreover preferences appear increasingly individualistic as group size increases from 3 to 6. We believe

that this will hold for larger group sizes. It also follows that experiments using the mean-matching protocol, or other protocols involving population-statistics, with typical group sizes (six or more) can safely be interpreted as valid tests under the hypothesis of individualistic preferences.

Our results do not contradict those of Bolton, Katok and Zwick (1998), since their experiment really entailed ten two-person dictator games of \$1 each with the same dictator but ten different passive participants, instead of one eleven-person dictator game with a \$10 pot; hence, the cost of an equal split was only \$0.50, which is below our estimate of the warm-glow benefit of egalitarianism.

We should be cautious in our interpretations of dictator game results when the property rights and entitlement issues are ambiguous. How the participants view these issues becomes an unobserved nuisance parameter that could contribute to widely diverse behavior, because when rights are more definite, behavior is significantly affected (Hoffman and Spitzer, 1985; Hoffman, McCabe, Shachat and Smith, 1994).

Recognizing these effects as general path-dependency effects, their prevalence in most applications means that participants do not in general have stable preferences over the distribution of monetary payoffs: $u_i(z) \neq u_i(x(z))$. An alternative, and perhaps more fruitful approach, may be to look for archetypal *behavioral rules* (such as "be individualistic," or "be egalitarian"), that map from situations into available actions, and then to invert this map to categorize situations.

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Instructions to the Participants for Experiment 1

[PLEASE DO NOT TOUCH THE MOUSE AND KEYBOARD UNTIL TOLD TO DO SO!]

WELCOME [Introduce self and helpers. Say how everyone was recruited.]

This is an experiment about economic decision making. If you follow the instructions carefully you might receive a considerable amount of money. This money will be paid at the end of the experiment in private and in cash. [Wave cash.]

Each participant will receive \$10 as compensation participating in this experiment, which will last no more than one hour. Additional bonus payments will depend on the decisions that you and other participants make in the experiment; how these additional payments will be determined will be explained in detail momentarily.

It is important that during the experiment you remain SILENT. If you have any questions, or need assistance of any kind, RAISE YOUR HAND but DO NOT SPEAK. One of the experiment administrators will come to you and you may whisper your question to him. If you talk, laugh, or exclaim out loud, you will be asked to leave and will not be paid. We expect and appreciate your cooperation.

During the experiment, you will make 15 decisions. After making all 15 decisions, one of those decisions will be selected at random by the roll of dice, and your bonus payments will be determined by that decision alone. Since each decision could be the one that counts, you should treat each of the 15 decisions as if it were the one that counts.

You will be making choices using the computer mouse and keyboard. You may reposition the mouse pad so it is comfortable for you. Your mouse cursor should move when you slide the mouse on the pad. If not, please raise your hand.

Click on "Page Down" now.

You are now looking at the main screen.

Each of the 15 decisions is represented by one of the 15 yellow buttons on the lower right side of the screen. They are arranged in three columns labeled A, B and C, and 5 rows labeled 2, 4, 8, 12 and 24. The row labels indicate the group size for the three decisions in that row. For example, for the first row, the group size is 2, so if one of these three decisions is the one that counts, then all 24 participants in this experiment will be divided into 12 pairs (randomly assigned by the computer); in other words, you will be randomly paired with one other participant for this decision. For the second row, the group size is 4, so 6 groups of 4 participants each will be randomly created by the computer; you and three other randomly selected participants will be in one of those groups. For the third row, the group size is 8, so 3 groups of 8 participants will be formed; for the fourth row, 2 groups of 12 participants will be formed; and finally for the last row, everyone will be in one all inclusive group. [Show slide and describe the possible groupings.] The assignment to groups will be random and anonymous - no one will know who else is in their group.

The text boxes will be blank until you click on the first decision button. When you click on one of the yellow buttons, it will turn orange, and text will appear in the boxes.

In the first text box will be a description of one decision. On the overhead I will now show you what your computer screen will look like when the text boxes are filled in. Please look up front. You may stand up to see better. [Slide]

*Your **participant number** is on the top line. All money payments will be made in a sealed envelope marked only with your participant number. There is no way to associate your participant number with you as an individual (e.g. name or student ID). Hence, your decisions and the amount you go home with will be absolutely private.*

The second line shows the Decision number (4 in this example), and the text reads:

*"Assume that you have ended up in a group of size **n**, [$n = 2, 4, 8, 12$ or 24 , randomly and anonymously assigned as explained a moment ago]*

*and that you have been randomly selected to be the decision maker for a bonus of **\$Z**. [The computer will randomly and anonymously select a member of each group to be the decision maker; each member is equally likely to be selected. Show slide again.]*

*"Specifically you will decide now much of **\$Z** to keep for yourself, and the amount you do not keep will be multiplied by **m** [$m \geq 1$] and divided equally among the others in your group.*

"These bonus amounts plus the \$10 participation payment will determine the total amount of money everyone in your group goes home with." [Participants in the other groups will not be affected by your decision.]

The second text box will display the current group size, bonus amount, and multiplier. Below this text box is where you enter your decision as to how much of the bonus to keep. Click in the light blue box that appears after the words "You choose to keep \$___," [point]

*In this box you may enter any dollar and cents amount from 0.00 up to and including **\$Z**. If you enter more than two digits to the right of the decimal point, the computer will ignore them. If by mistake you enter a negative number or a number exceeding **\$Z**, your choice will not be recorded. After typing in a number in the blue box, numbers will appear in the subsequent boxes indicating the total amount of the bonus left for the other members of your group, and the amount each other member will receive (not counting their \$10 participation payment). To record this decision, click on the **CONFIRM YOUR CHOICE** button.*

Once you confirm your choice, the corresponding decision button will turn green. Thus, at the end of the allotted time, all 15 decision buttons should be green, indicating that you have made a decision for each one.

If you want to change the amount you want to keep, you can click on the light blue box again, use the backspace and/or delete keys to erase the old number, and type in a new number. You must then click on the **CONFIRM YOUR CHOICE** button for this change to take effect.

After you go to another decision, you can come back to decisions you already looked at simply by clicking on the corresponding decision button. The screen will display the same text boxes and your previous confirmed choice (if any). You may then review your choice, change it, or leave it as is. **REMEMBER:** changes will not take effect unless you click on the **CONFIRM YOUR CHOICE** button.

You will have a total of 15 minutes to make all 15 decisions. You must first make the five decisions in column A. You may do these in any order you like, and you may revisit each decision as often as you like. However, when you proceed to column B, you will not be able to revisit decisions in column A. You must then make all five decisions in column B. You may do these in any order you like, and you may revisit each decision in column B as often as you like. However, when you proceed to column C, you will not be able to revisit decisions in column A or B. Finally, you will make all five decisions in column C. You may do these in any order you like, and you may revisit each decision in column C as often as you like.

When the 15 minutes expire (as indicated by the clock on the bottom of your screen), you must have made all 15 decisions, so all 15 decision buttons must be green. If not, you will not receive any part of the bonus, and \$5 will be subtracted from your participation payment.

I will give you a verbal warning when 10, 5 and 1 minute remains.

If there are any questions, please raise your hand, but do not speak out loud. I will come to you, and you may whisper your question to me.

If there are no further questions, we are ready to begin.

Click on the box to the right of the **BEGIN TIME** button, and type in 555. Do not click the **BEGIN TIME** button until I tell you to click. Put your mouse cursor on the **BEGIN TIME** button and wait ... ready, click now.

Remember that you must click on a yellow decision button before text appears in the boxes. The clock is ticking down from 15 minutes, 0 seconds. At the end, all 15 decision buttons must be green.

[After the time runs out, select a decision.

Roll a six-sided dice twice and add to determine group size.

Sum of dice = 2, {3,4}, {5,6,7,8,9}, {10,11} 12
 n = 2, 4, 8, 12, 24
row = 1, 2, 3, 4, 5

Roll once again to determine column.

	A	B	C
2	1, 1	6, 2	11, 3
4	2, 4	7, 5	12, 6
8	3, 7	8, 8	13, 9
12	4, 10	9, 11	14, 12
24	5, 13	10, 14	15, 15

$die = \{1,2\}, \{3,4\}, \{5,6\}$
 $column = \begin{matrix} A & B & C \end{matrix}$

$Decision\ number = (column - 1)*5 + row;$ announce this.

However, enter the following number at the server prompt: $(row - 1)*3 + column.]$

Instructions to the Participants for Experiment 2

[PLEASE DO NOT TOUCH THE MOUSE AND KEYBOARD UNTIL TOLD TO DO SO!]

WELCOME

*[Introduce self and helpers. Say how everyone was recruited.
Explain: "This session will be tape-recorded for scientific purposes."]*

This is an experiment about economic decision making. If you follow the instructions carefully you might earn a considerable amount of money. This money will be paid at the end of the experiment in private and in cash. [Wave cash.]

Any participant that showed up at least 5 minutes early will receive a \$5 bonus. In addition, each participant will receive \$5 as compensation for participating in this experiment, which will last about one hour. Additional bonus payments will depend on the decisions that you and other participants make in the experiment; how these additional payments will be determined will be explained in detail momentarily.

It is important that during the experiment you remain SILENT. If you have any questions, or need assistance of any kind, RAISE YOUR HAND but DO NOT SPEAK. One of the experiment administrators will come to you and you may whisper your question to him. If you talk, laugh, or exclaim out loud, you will be asked to leave and will not be paid. We expect and appreciate your cooperation.

You will be making choices using the computer mouse and keyboard. You may reposition the mouse pad so it is comfortable for you. Your mouse cursor should move when you slide the mouse on the pad. If not, please raise your hand. Do NOT click the mouse buttons or use the keypad until told to do so.

[The screen will be blank until everybody clicks on the begin button - but not yet.]

You will be making a number of decisions, but the outcomes of these decisions will not be revealed to anyone until the end of the experiment after all decisions have been made. Each decision will count in determining your total earnings.

Please click on the Begin button on your screen. Once you have done this, the screen will display the first decision. Please listen to my instructions.

***We will now describe the first decision task.** You will click on the box on the right side of Decision 1 and enter an integer from 1 to 10. At the end of the experiment, an integer from 1 to 10 will be randomly drawn, and if it is equal to your choice, you will win \$20.*

Click on the box for Decision 1 now and make your decision. [Pause 30 seconds.]

If you haven't made a decision yet, do it NOW and stop.

Following this decision, some of you will have earned \$30 (\$20 from Decision 1, \$5 for participating, plus \$5 for showing up early). Others will have earned \$25, \$10, or only \$5. The remaining decisions in this experiment will add further earnings. In the end, there will be no way to infer from your total earnings what choice you made on any particular decision. Also, your consent form, which by now should be signed by you, made it clear that your receipts will not be kept by the researcher after the experiment, and that your name and identifying information will never be linked with any of the data. Any attempt to link decision to a person would be deemed a serious violation of the Human Subjects Protocol and Harvard Protection of Human Subjects. Therefore, rest assured that your choices are completely private and anonymous.

Now please click on "WAIT FOR THE EXPERIMENTER'S SIGNAL". Your screen now says "to move onto the next decision click on the button below". Click on OK now. Please listen to my instructions.

***We will now describe the second decision task.** The computer has randomly assigned each of you to six groups of six participants each, as illustrated on the video monitors. One participant in each group has been randomly selected to be the Decision Maker for this decision task. Your screen will indicate in the text for Decision 2 whether or not you are the Decision Maker in your group.*

If you are the Decision Maker, then you decide how much of \$20 (0 - 20) to keep for yourself; the amount you do not keep will be divided equally among the other 5 members of your group. Let me repeat this ... [Pause 5 seconds]

If you are not the Decision Maker, then another participant in your group is the decision maker. Nonetheless we would like to know what decision you would have made had you been selected as the decision maker.

Everyone, please use the next minute to make your decision. [Pause 60 seconds.] If you haven't made your decision yet, do so NOW and stop.

Instructions for Participants for Experiment 3.

[PLEASE DO NOT TOUCH THE MOUSE OR KEYBOARD UNTIL TOLD TO DO SO!]

WELCOME

[Introduce self and helpers. Say how everyone was recruited.]

This is an experiment about economic decision making. If you follow the instructions carefully you might earn a considerable amount of money. This money will be paid at the end of the experiment in private and in cash. [Wave cash.]

Every participant will receive \$5 as compensation for participating in this experiment, which will last about one hour. Additional bonus payments will depend on the decisions that you and other participants make in the experiment; how these additional payments will be determined will be explained in detail momentarily.

It is important that during the experiment you remain SILENT. If you have any questions, or need assistance of any kind, RAISE YOUR HAND but DO NOT SPEAK. One of the experiment administrators will come to you and you may whisper your question to him. If you talk, laugh, or exclaim out loud, you will be asked to leave and will not be paid. We expect and appreciate your cooperation.

You will be making choices using the computer mouse and keyboard. You may reposition the mouse pad so it is comfortable for you. Your mouse cursor should move when you slide the mouse on the pad. If not, please raise your hand. Do NOT click the mouse buttons or use the keypad until told to do so.

[The screen will be blank until everybody clicks on the PG DN button - but not yet.]

You will be making a number of decisions, but the outcomes of these decisions will not be revealed to anyone until the end of the experiment after all decisions have been made. Each decision will count in determining your total earnings.

***We will now describe the first decision task.** The computer has randomly assigned each of you to 6 groups of three participants each (**show slide of groups**), as illustrated on the overhead. **One** participant in each group has been randomly selected to be the Decision Maker for this decision task. Each of you has an equal chance of being the Decision Maker. Your screen (when it appears) will indicate whether or not you are the Decision Maker in your group. No one will know which group they belong to, or who is in their group, and the identity of the Decision Makers will never be made public. You will be identified only by a randomly assigned Participant number. Your money earnings will be given to you in an envelope identified only by your Participant number. You will not be asked to sign a receipt or in any way associate your name, Social Security number or any other personal information with your choices or money earnings. Thus, absolute anonymity and privacy will be ensured.*

If you are the Decision Maker, then you will decide how much of \$10 (0 - 10) to keep for yourself; the amount you do not keep will be divided equally among the other 2 members of your group.

If you are not the Decision Maker, then another participant in your group is the Decision Maker. Nonetheless we would like to know what decision you would have made had you been selected as the decision maker.

Click on page down. *Everyone, please use the next minute to make your decision. Click on the box, enter a number. Use the backspace key to correct any typos, then click the SUBMIT button. [Pause 60 seconds.] If you haven't made your decision yet, do so NOW and STOP. Wait for further instructions.*

Instructions to Participants for Experiment 4

WELCOME *[Introduce self and helpers.]*

*This is an experiment about economic decision making. If you follow the instructions carefully you might receive a considerable amount of money. Each participant will receive \$10 as compensation for participating in this experiment, which will last no more than 1/2 hour. Additional bonus payments will depend on the decisions that you and other participants make in the experiment and partly on chance. This money will be paid at the end of the experiment in private and in cash. [Wave cash.] It is important that during the experiment you remain **SILENT**. We expect and appreciate your cooperation.*

*Each of you has been given an envelope with an ID number printed on the envelope. Inside are two pieces of paper: one thin white slip and a larger and stiffer green card. Please remove **ONLY** the small white slip. It also has your ID number printed on it. You will need this to claim your envelope (which will contain your money earnings) at the end of the experiment, so put this in a safe place such as your pocket. You will not be identified in any other way.*

*Everyone has been randomly assigned to groups of six participants each (**show slide of groups**), as illustrated on the overhead. No one will know which group they belong to or who is in their group. For each group, **one** participant will be randomly selected to be the Decision Maker. Each of you has an equal chance of being the Decision Maker, and you will be asked to make a decision as if you were the Decision Maker, record this on the green paper and put it back in your envelope.*

*After all envelopes have been collected, they will be randomly tossed into four boxes (one for each group of 6). You will not know which box your envelope is in. One envelope will be randomly drawn from each box; **the decision in that envelope** and only that decision will determine how much money we put in each envelope for that group. To guarantee that the identity of the Decision Makers remain anonymous, we will not announce the ID number of any Decision Maker.*

*Here is your decision problem [**show next slide.**] Assume you have been randomly selected to be the Decision Maker for a bonus of \$75. Specifically, decide how much of \$75 to keep for yourself [\$0.00 to \$75.00]; the amount you do not keep will be divided equally among the other 5 members of your group. No one, including us, will ever know the identity of the Decision Makers. Let me repeat this ... [Pause 5 seconds]*

*Everyone, please use the next minute to make your decision. Write your decision on the green slip of paper, and do not allow anyone to see what you write. Then put the paper inside the envelope. [Pause 60 seconds.] If you haven't made your decision yet, please do so **NOW**.*

We will now collect the envelopes, and follow the previously announced procedure to put money into these same envelopes. At the end of this session you can claim your envelope by presenting your Claim Check as you leave.