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Real and hypothetical rewards in self-control and social discounting

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Abstract

Laboratory studies of choice and decision making among real monetary rewards typically use smaller real rewards than those common in real life. When laboratory rewards are large, they are almost always hypothetical. In applying laboratory results meaningfully to real-life situations, it is important to know the extent to which choices among hypothetical rewards correspond to choices among real rewards and whether variation of the magnitude of hypothetical rewards affects behavior in meaningful ways. The present study compared real and hypothetical monetary rewards in two experiments. In Experiment 1, participants played a temporal discounting game that incorporates the logic of a repeated prisoner's-dilemma (PD) game versus tit-for-tat; choice of one alternative ("defection" in PD terminology) resulted in a small-immediate reward; choice of the other alternative ("cooperation" in PD terminology) resulted in a larger reward delayed until the following trial. The larger-delayed reward was greater for half of the groups than for the other half. Rewards also differed in type across groups: multiples of real nickels, hypothetical nickels, or hypothetical hundred-dollar bills. All groups significantly increased choice of the larger delayed reward over the 40 trials of the experiment. Over the last 10 trials, cooperation was significantly higher when the difference between larger and smaller hypothetical rewards was greater. Reward type (real or hypothetical) made no significant difference in cooperation on most measures. In Experiment 2, real and hypothetical rewards were compared in social discounting—the decrease in value to the giver of a reward as social distance increases to the receiver of the reward. Social discount rates were well described by a hyperbolic function. Discounting rates for real and hypothetical rewards did not significantly differ. These results add to the evidence that results of experiments with hypothetical rewards validly apply in everyday life.

Keywords: delay discounting, hypothetical rewards, prisoner's dilemma, real rewards, social discounting, humans.

1 Introduction

In research on choice using hypothetical rewards, participants are asked to imagine the alternatives—to choose as they would if the alternative rewards were real. Such research assumes that participants are successful in this imaginative task. One potential problem with using hypothetical questions in psychological research is that participants may not be capable of the required imaginary act. People often cannot predict what they will do in certain situations; an alcoholic may believe in the morning that he will not drink at a party to be held that evening. But then he may well drink (Connors, O'Farrell, & Pelcovits, 1988). Even if he is asked to reflect on his behavior at past parties, he may not remember how much he drank (White, 2003). Or, in a task involving a degree of altruistic behavior, such as a dictator game (Camerer, 2003), where participants are asked how much of an initial monetary endowment they are willing to give to another participant (with no explicit consequence for giving a small amount or even nothing), a participant might say she would give more money to the recipient when the gift is hypothetical than she would if the money were real. Participants want experimenters to believe that they are altruistic; with hypothetical rewards, it costs nothing to be altruistic. Because of such "demand characteristics", economists and some psychologists have been reluctant to recognize the validity of choices among hypothetical rewards. Reviews of game-theory and decision experiments have found that real monetary rewards are stronger incentives than nominally equivalent hypothetical rewards (Camerer & Hogarth, 1999; Hertwig & Ortmann, 2001; Smith & Walker, 1993). Kuhberger, Schulte-Mecklenbeck, & Perner (2002), unlike most prior studies (and unlike Experiment 1 of the present study) compared large real rewards directly with large hypothetical rewards. They studied double-or-nothing gambles with all combinations of a) positive and negative framing, b) small and large rewards, and c) hypothetical and real rewards. They found strong inter-group effects of framing and magnitude of reward, as expected, but choices by participants who imagined a hypothetical gamble did not differ from choices by participants gambling for real money.

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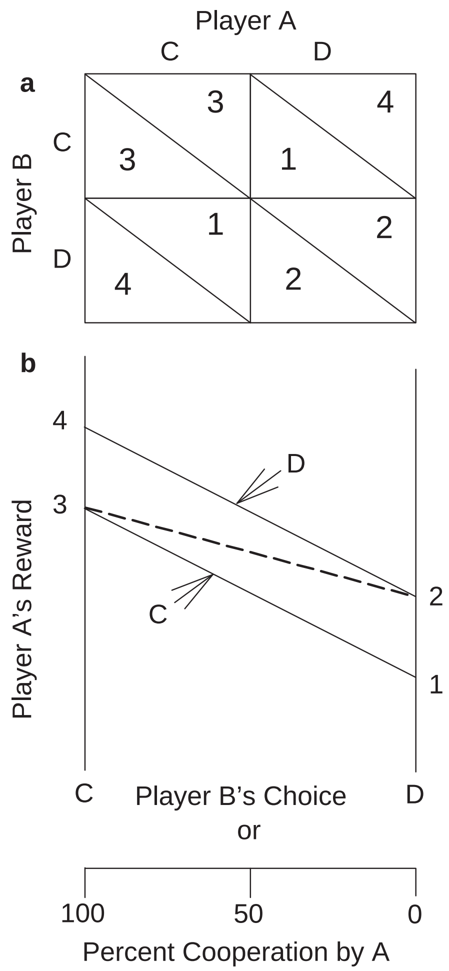
Moreover, studies comparing delay discounting with real and hypothetical rewards have found both reward types to yield hyperbolic functions and failed to find significant differences in steepness of discounting depending on type of reward (Madden, Begotka, Raiff, & Kastern, 2003). Although failure to find significance does not in itself mean that there are no differences, the existence of significant differences in steepness of delay discounting of hypothetical as well as real rewards when *other* variables have been varied (such as reward amount, IQ, GPA, and age of participants, stability of the monetary unit, whether participants are compulsive gamblers or use drugs including cigarettes, alcohol, heroin or cocaine; reviewed in Odum, 2011) increases confidence that addicts and non-addicts alike successfully imagined real-world choices. For example, Johnson and Bickel (2002) found a strong correspondence (r = .83) between degree of discounting with hypothetical choices and "real rewards" (one randomly selected outcome actually given).

Whether or not hypothetical rewards have effects similar to those of real rewards is an important issue in interpreting laboratory studies of decision and choice. If the effects of real and hypothetical rewards are similar then rewards in the laboratory may be varied over wide ranges, and limitations on number of participants will not be constrained by monetary considerations. For example, steepness of discounting (decrease in reward value) varies inversely with amount of a delayed reward but directly with amount of a probabilistic reward (Green, Myerson, & Ostaszewski, 1999). The hypothetical delayed or probabilistic rewards in the Green et al. study varied from $100 to $10,000; it would have been practically impossible to have done this study with real rewards. But, the more evidence that experimental participants choose similarly between lesser amounts of real and hypothetical rewards, the more confidence there will be that participants choosing among larger hypothetical rewards are successfully imagining how they would choose if the rewards were real.

Experiment 1 compared real and hypothetical rewards in a prisoner's dilemma (PD) type game versus tit-for-tat. Figure 1a shows a PD contingency matrix used in Experiment 1. In a 2-person PD game, Player A and Player B each choose between cooperating (C) and defecting (D). Each player receives a reward determined by the combination of both players' choices. In the case of the 1–2-3–4 reward matrix of Figure 1, a player will earn 1, 2, 3, or 4 reward units, depending on which of the 4 possible choice combinations are chosen. If both cooperate, each receives a moderately high reward (3 units); if both defect, each receives a moderately low reward (2 units) but if one cooperates and the other defects the cooperator receives a very low reward (1 unit) while the defector receives a very high reward (4 units). A strategy, called tit-for-tat, has been found to increase cooperation in repeated PD games (Rapoport, 1974). Suppose Player-B rigidly plays tit-for-tat: If A cooperates on trial n, B will cooperate on trial n+1; if A defects on trial n, B will defect on trial n+1. When B cooperates, A receives 3 units (for cooperating) or 4 units (for defecting); when B defects, A receives only 1 unit (for cooperating) or 2 units (for defecting). Thus, regardless of A's choice, A gets 2 more units when B cooperates than when B defects. But, if B is rigidly playing tit-for-tat, the only way that B will

Figure 1: Upper panel (a): The Prisoner's Dilemma ("1–2-3–4") reward matrix. Cooperating or defecting produced the units indicated in the top row (C) or bottom row (D), respectively. Values above each "\" would be given to Player A and values below would be given to Player B. Lower panel (b): The average number of reward units earned per trial by Player A as a function of defection rate by Player B. The solid "D" line indicates exclusive defection by Player A. The solid "C" line indicates exclusive Cooperation by Player A. The dashed line indicates perfect reciprocation (tit-for-tat) by Player B as a function of defection rate by Player A.



cooperate is if A previously cooperated. By cooperating on the present trial, A gives up 1 unit but will gain 2 units on the next trial when B reciprocates. B's tit-for-tat strategy thus creates a self-control problem for A: a smaller, immediate gain for defecting (1 unit on this trial) versus a larger, delayed gain for cooperating (2 units on the next trial). Essentially, if B rigidly plays tit-for-tat, A must choose the lower of two rewards on the present trial (cooperate) in order to get B to cooperate; B's cooperation in turn allows A to choose between higher rewards on the next trial. The dashed line of Figure 1b shows A's average reward as a function of A's percent cooperation with B playing tit-for-tat. The optimum strategy for A (except on the very last trial) is to always cooperate, averaging 3 units per trial. Since B's tit-for-tat behavior is rigidly determined, B's place may be taken by the experimental apparatus—as it was in Experiment 1.

In prior experiments, with a 1–2-3–4 matrix versus tit-for-tat, participants learned to cooperate over repeated trials (Rachlin, Brown & Baker, 2000). With a 1–2-5–6 matrix, where the delayed reward was 4 units, players learned to cooperate faster and to a higher asymptote than with the 1–2-3–4 matrix, where the delayed reward was 2 units. As the delay of B's reciprocation (time between trials) increases or its probability decreases, A's cooperation decreases proportionally (Locey & Rachlin, in press; Rachlin et al., 2000).

The computer game played in Experiment 1 (originally used in a board-game version by Brown & Rachlin, 1999) replicated the PD versus tit-for-tat contingencies but did away with the pretense that there was another player (Player B) making independent choices. As in the typical PD versus tit-for-tat game, participants had to choose the lower of two available rewards on the present trial (1 rather than 2 or 3 rather than 4) in order to be able to choose between a pair of higher rewards (3 or 4 rather than 1 or 2) on the next trial.

Given that (1) the PD versus tit-for-tat is a self-control game, (2) self-control has been found to vary inversely with the steepness of an individual's delay discount function (Odum, 2011), and (3) the steepness of delay discount functions has been found not to be significantly different with real or hypothetical rewards (Johnson & Bickel, 2002), we expected that type of reward (real versus hypothetical nickels) would have a weak effect or no effect on cooperation in the present experiment. Whether cooperation would be affected by increasing all hypothetical rewards by a factor of 2,000 ($100.00:$0.05) would depend on whether self-control is a function of the ratio or the difference in amounts of immediate and delayed rewards. Choice experiments with humans (for instance, Logue & Forzano, 1992) indicate that reward ratios rather than differences count in these situations. Thus we expected that cooperation would not be significantly affected by increasing all rewards proportionally. However, we did expect that increasing only the (delayed) reward for cooperation while leaving the (immediate) reward for defection constant (changing the matrix from 1–2-3–4 to 1–2-5–6) would increase cooperation significantly.

The PD game of Experiment 1 is normally a social game where the alternatives are maximization of benefit to oneself versus maximization of benefit to another player. The tit-for-tat contingency substitutes the participant's own future self for the other player and changes the game to a self-control situation. Experiment 2 studied a true social choice. Participants chose between larger rewards for another person and smaller rewards for themselves. Both experiments tested the efficacy of hypothetical versus real monetary rewards.

2 Experiment 1

2.1 Method

Participants: 150 undergraduate students (70 female, 80 male), recruited through the psychology subject pool at Stony Brook University, were randomly assigned to one of two reward matrices: 1–2-3–4 (n = 75, 35 female) or 1–2-5–6 (n = 75, 35 female). Within each reward-matrix condition, participants were randomly assigned to one of three groups: Hypothetical $100 Bills, Hypothetical Nickels, or Real Nickels.

Apparatus & Instructions: Participants were seated at a cubicle measuring 2.5 m high, 2.1 m wide, and 2.2 m deep. They faced a personal computer with a 17-inch monitor atop a 1.2 m-wide square table. Experimental events were controlled by a custom program—designed using Microsoft Visual Basic® 6.0—that ran on the Microsoft Windows XP® operating system. A one-button mouse was used to interact with the objects on the computer screen. When participants were first seated at the computer, they were asked to read the following instructions on the screen:

> In this game, you should try to collect as much money as you can from the treasure chests. Unfortunately, the money you earn is not real, but please make your choices as you would if the money were real. The contents of each chest are labeled at the bottom of the chest. Click on a chest of the same color as your key to unlock it. Once it is unlocked, click on the lid to open it. Click on the new key so that you can continue collecting money. Tip: There is no time limit for this game, so take as much time as you want with each choice. Tip: You never need to hold down the mouse button in this game—just click and release.

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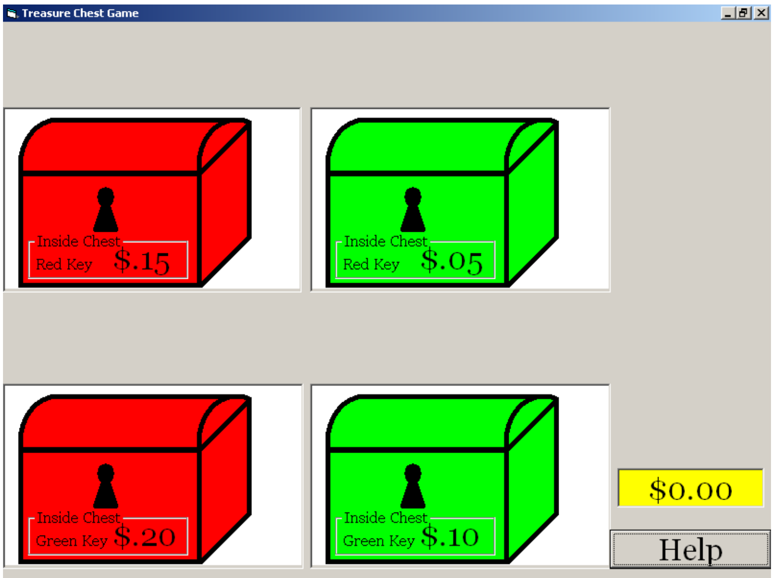
Figure 2: The starting screen for the Nickels groups in the 1–2-3–4 reward matrix.



For participants in the Real Nickels group, the second sentence in these instructions was replaced with: "The money you earn is REAL: at the end of this session, you will be paid in CASH whatever amount has accumulated in the yellow box on the right."

A start button (labeled "Start") was located in the bottom right corner of the screen. Participants were verbally instructed to click on the Start button once they had finished reading the instructions. A single mouse click on the Start button revealed the screen shown in Figure 2. The monetary values shown in Figure 2 were those used in the Real Nickels and Hypothetical Nickels groups of the 1–2-3–4 reward matrix. For the Hypothetical Bills group of the 1–2-3–4 reward matrix, each 5 cents was replaced by $100 ($100, $200, $300, and $400). For the 1–2-5–6 reward matrix groups, the chest values were $.05, $.10, $.25, and $.30 for the Hypothetical and Real Nickels groups and $100, $200, $500, and $600 for the Hypothetical Bills group.

A single click on the Help button in the bottom right corner of the screen revealed the instructions—which could be removed with another click of the same button (labeled "Continue" while the instructions were on the screen). The Money Box above the Help button showed the total amount of money that had been earned throughout the course of the game.

Procedure: Participants chose between immediately larger and immediately smaller rewards. Choosing the immediately larger reward corresponded to defection in a PD game versus tit-for-tat. Although choice of the larger reward (defection) maximized present-trial reward, it resulted in a smaller pair of alternatives on the next trial and an overall lower rate of reward (as if another player had defected in turn). The color of the key obtained by the participant determined which pair of rewards would be available. A red key gained access to the larger pair of alternatives on the next trial, but the red key could only be obtained by choosing the lesser reward on the current trial. Overall rate of reward could be maximized by always choosing the smaller current reward (just as overall rate of reward in a PD game versus tit-for-tat is maximized by always cooperating). The main difference between the current procedure and a PD game versus tit-for-tat is that current participants knew that there was no other player.[1]

The procedure was exactly as described in the instructions. At the start of the game, the mouse cursor was a red key. A single click on either of the two red chests changed the cursor to a white hand and created a slight opening of the chosen chest's lid. Clicking on the chest again opened the lid and revealed the contents of the chest—as labeled on the chest. The contents appeared directly above the chest. For the Nickels groups, the contents were a number (1–6) of U.S. nickels and a red or green key. For the Bills groups, the contents were a number (1–6) of $100 bills (USD) and a red or green key. Clicking on the newly revealed key caused the money to slide to the right side of the screen at a constant speed for 0.5 seconds. It then

[1] It makes a difference whether PD players versus tit-for-tat believe that they are playing against another player or a machine. When they know that they are playing against a machine they learn to cooperate faster than they do when they believe they are playing against another participant. Baker and Rachlin (2002) attribute this to attempts by participants who believe there is another player to punish her defection by defecting themselves.

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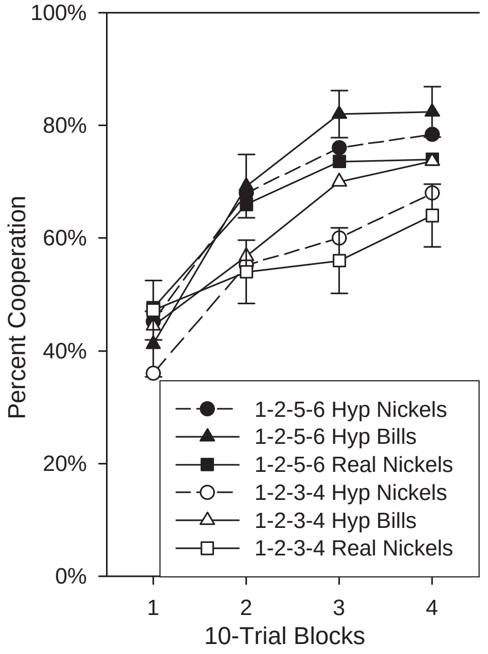
Figure 3: Percent cooperation for each of the six groups as a function of 10-trial blocks. Error bars indicate standard error of the mean for the most and least cooperative groups (1–2-5–6 Hypothetical Bills and 1–2-3–4 Real Nickels); for the sake of clarity, error bars are shown only for these 2 groups. Variance was about the same for all groups.



slid down to the Money Box at a constant speed for 0.5 seconds, at which point the money disappeared and its value was added to the total amount shown in the Money Box. In addition to causing the deposit of any revealed money, clicking on the newly revealed key also caused the mouse cursor to assume the appearance of that key. If red, the key could be used to open one of the red chests. If green, it could be used to open one of the green chests. The game ended after 40 trials of obtaining money (and keys) from chests.

Note that regardless of the active key color, the participant had a choice between two chests: an upper chest and a lower chest. For both reward matrices, choosing the upper chest always resulted in 1 monetary unit (a nickel or a $100 bill) less than choosing the lower chest. However, choosing the upper chest also resulted in a red key which increased the value of both choice alternatives on the ensuing trial, relative to having a green key. This procedure duplicates the contingencies of a tit-for-tat prisoner's dilemma. To maintain consistency with standard prisoner's dilemma terminology, choices for either of the upper chests are referred to here as cooperations, whereas choices for a lower chest are referred to as defections.

2.2 Results

The 40-trial session was divided into 4 blocks of 10 trials. Figure 3 shows the percentage of cooperations for each of the six groups as a function of trial block. Standard errors for all groups were similar (between 4.0% and 6.5% across all blocks). All groups increased cooperation in each successive block of trials, from an average of 43.7% in the first block to an average of 73.0% in the final block of trials.

A mixed-design ANOVA with trial-block as the repeated-measure, within-subject factor indicated a significant effect on cooperation for both reward matrix ($F(1,144) = 7.41$, $p = .007$, $\eta^2 = .028$) and trial-block ($F(3,432) = 86.28$, $p < .001$, $\eta^2 = .152$) but no significant effect of reward type ($F(2,144) = 0.66$, $p = .521$, $\eta^2 = .005$) and no evidence of an interaction between reward matrix and reward type ($F(2,144) = 0.13$, $p = .874$, $\eta^2 = .001$). Given that cooperation consistently increased across trial blocks, linear trend analyses were also conducted (see Hale, 1977 or Tabachnick & Fidell, 2001). Those analyses yielded similar results—qualitatively and quantitatively—to the mixed-design ANOVA with the sole exception of a significant effect of reward type: $F(2,144) = 3.364$, $p = .037$, $\eta^2 = .024$. Linear trend comparisons of the three reward types indicated a significant difference between Hypothetical Bills and Real Nickels ($F(1,98) = 5.689$, $p = .019$, $\eta^2 = .030$), a nearly significant difference between Hypothetical Nickels and Real Nickels ($F(1,98) = 3.903$, $p = .051$, $\eta^2 = .020$), but no significant difference between Hypothetical Bills and Hypothetical Nickels ($F(1,98) = .512$, $p = .476$, $\eta^2 = .002$).

As can be seen in Figure 3, the rank order of group mean cooperation was identical across each of the final three trial blocks, with all 1–2-5–6 reward matrix groups cooperating more than all 1–2-3–4 reward matrix groups. Furthermore, the rank order within each reward matrix was identical: the Hypothetical Bills group showed the most cooperation and the Real Nickels group showed the least cooperation in each of those blocks, for each reward matrix. However, linear trend comparisons of each of the three reward types for the final three trial blocks found no significant differences.

Figure 4 shows percent cooperation (mean and standard error) during the final block of trials for each of the 6 groups, organized by reward type. Consistent with Figure 3, the 1–2-5–6 matrix generated more cooperation than did the 1–2-3–4 matrix for each of the three reward types. A two-way ANOVA indicated a significant effect of reward matrix on cooperation ($F(1,144) = 5.11$, $p = .025$, $\eta^2 = .028$) and a non-significant effect of reward type ($F(2,144) = 1.46$, $p = .236$, $\eta^2 = .016$), with no evidence of an interaction ($F(2,144) = .01$, $p = .988$, $\eta^2 < .001$).

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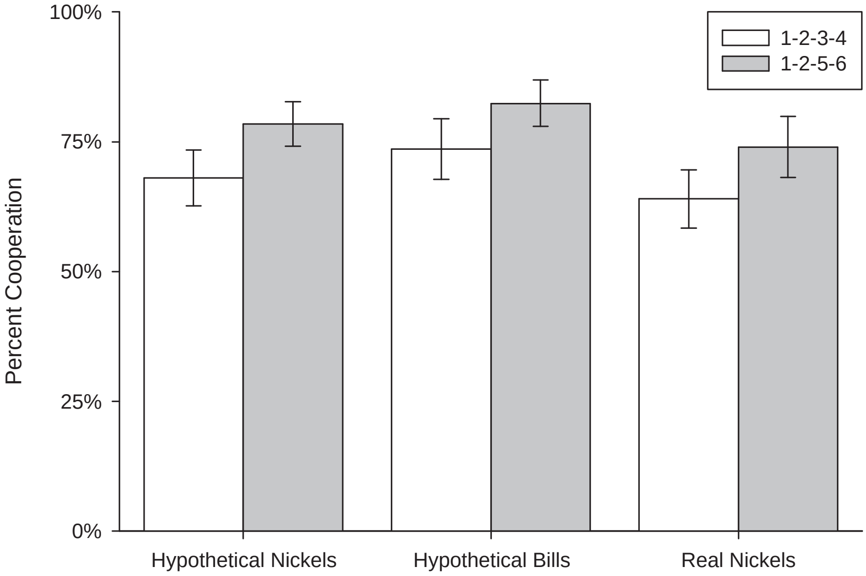
Figure 4: Percent cooperation for each of the six groups during the last 10 trials of the session. Error bars indicate standard error of the mean.



A mixed-design ANOVA with trial-block as the within-subject factor indicated a significant effect of gender on cooperation (F (1,138) = 5.81, p = .017, η^2 = .021), with males cooperating in 66.8% of the trials and females cooperating in 57.1%. Within each gender, overall cooperation was greater in the 1–2–5–6 reward matrix than in the 1–2-3–4 reward matrix. Similarly, within each gender, the most cooperation was found in the Hypothetical Bills group and the least cooperation was found in the Real Nickels group (consistent with the combined-gender data of Figures 3 and 4).

2.3 Discussion

As expected, increasing the amount of the (delayed) reward for cooperation increased cooperation. Also, as expected, reward type had little impact on cooperation. According to Cohen's (1988) guidelines, the effect size of reward type was small (.059 > η^2 > .01) for the across-session linear trend analysis and the comparison of cooperation during the final block of trials. As can be seen in Figure 4, these differences were also small with respect to the absolute differences in cooperation across groups—less than a 10% difference across all reward types within each matrix. Although the differences between reward types were small, the rank order of the three reward types was consistent across the final 30 (of 40) trials of the experiment (Figure 3) and across the two reward matrices (Figure 4); cooperation was highest with hundred-dollar bills, and lowest with real nickels. Although the differences were small and only significant with respect to the across-session linear trend comparison of hypothetical $100 bills and real nickels, it is worth noting that the direction of the reward-type differences would indicate that hypothetical rewards provided at least as great an incentive as real rewards, and possibly an even greater incentive. Perhaps the hypothetical nickels increased participants' sensitivity to the actual contingencies of the experiment while the small real rewards were distracting. It is reassuring, in any case, that $100 bills, practical only as hypothetical rewards, were most effective of all in reinforcing cooperation. In sum, decision-making about rewards in the context of this experiment does not appear to change based on whether rewards are real or hypothetical.

Participants in delay-discounting and self-control experiments may be thought of as choosing between rewards for themselves now and rewards for themselves later: one selfish alternative versus another selfish alternative. But many everyday-life and laboratory choices involve selfish versus unselfish alternatives. One may ask whether a person will be more generous with hypothetical than with real money when the recipient is another person than when the recipient is themselves at a later time. Experiment 2 takes up this question.

3 Experiment 2

The choice between selfish and unselfish alternatives is perhaps nowhere more stark than in the frequently-studied dictator game. In the dictator game, Player-A is offered a sum of money and told simply to allocate it as she wishes between herself and Player-B. Obviously, Player-A would maximize money earned by keeping it all. Perhaps surprisingly, the modal behavior in this game is often to split the money evenly with Player-B. Fantino, Gaitan, Kennelly and Stolarz-Fantino (2007) studied dictator-game behavior with both real probabilistic money (1/3 chance of winning $50) and hypothetical money ($50); they found that about 75% of participants in both groups split their endowment equally between themselves and the other player. They report no statistical tests but state (p. 112): "Resource allocation was comparable for … all … resource types." If hypothetical reinforcement were weak, or if participants were more inclined to impress experimenters with their generosity with hypothetical than with real reinforcers, they would have been more generous with hypothetical reinforcers. But they were not.

The present experiment follows-up the Fantino et al. (2007) results with social discounting, a systematic procedure similar to the dictator game, involving repeated choices between smaller amounts of money for oneself and larger amounts for another person (Jones & Rachlin, 2006, 2009; Rachlin & Jones, 2008a, 2008b, 2009). The social discounting procedure has been used to obtain social discount functions. A social discount function expresses the value to one person of a reward obtained by another person as a function of the social distance between them. The greater the social distance between Player-A and Player-B, the less a reward to Player-B is worth to Player-A.

Social discount functions have been obtained in the laboratory by: a) determining, on an ordinal scale, the social distance between the participant and a person potentially receiving a reward, then b) determining the amount of (hypothetical) money the participant is willing to forgo in order for the receiver to obtain that reward and c) plotting amount of money forgone as a function of social distance. In these experiments participants were asked to imagine making a list of the 100 people closest to them with #1 as the closest and #100 perhaps a person who they barely knew. The number on the list was used as a measure of social distance.

Then (in a booklet form) participants were asked a series of hypothetical questions as follows: "Which would you prefer, $75 for yourself or $75 for the N^{th} person on your list?" (The ordinal position, N, of the person potentially receiving the money varied from page to page of the booklet.) Most participants preferred the $75 for themselves for all but the very closest people to them. Then the participant was asked (for example, with the 10^{th} person on the list): "Which would you prefer, $65 for yourself or $75 for the 10^{th} person on your list?" Then, "Which would you prefer, $55 for yourself or $75 for the 10^{th} person on your list?" Then, "Which would you prefer, $45 for yourself or $75 for the 10^{th} person on your list?" And so forth, all the way down to, "Which would you prefer, $0 for yourself or $75 for the 10^{th} person on your list?" At some point, as their own reward decreased, all participants crossed over to prefer $75 for the other person. This procedure parallels the one developed to measure delay discounting with hypothetical rewards (Rachlin & Raineri, 1992). Figure 5 shows median crossover points as a function of social distance as obtained by Rachlin and Jones (2008a).

The solid line in Figure 5 fit to the points is the following hyperbolic discount function (Mazur, 1987):

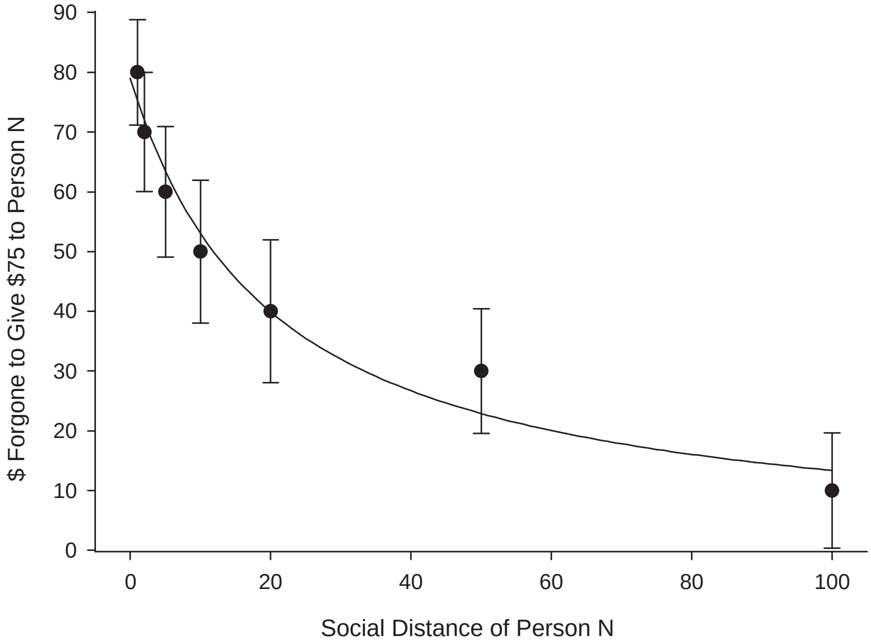
$$v = \frac{V}{1 + kN}, \tag{1}$$

where v = median crossover point; V = undiscounted value of the reward; N = social distance; k = a constant measuring steepness of discounting. For the data of Figure 5, $k = 0.05$ (Rachlin & Jones, 2008a).

As with delay discounting, steepness of social discounting has been found to correlate with various other measures: participants who contributed more (hypothetical) money to a common good in a public goods game had shallower social discount functions than those who contributed less (Jones & Rachlin, 2009); social discount functions were steeper for non-relatives (genetic overlap, $r < 1/32$) than for relatives ($r > 1/32$) (Rachlin & Jones, 2008b); pregnant smokers who stopped smoking during pregnancy had shallower social discount functions than those who kept smoking (Bradstreet et al., in press); amount given in dictator and ultimatum games decreased hyperbolically, as in Figure 5, as a function of social distance and steepness was correlated across individual participants with that of social discount functions (Jones & Rachlin, 2009). Although social distance in these experiments was measured on an ordinal scale, participants were able to transform the ordinal scale to a ratio scale—to express social distance in terms of physical distance (Rachlin & Jones, 2009).

One reason why discounting has been so frequently measured with hypothetical rewards is the logistical difficulty of using real rewards. In typical discounting studies, dozens of questions are asked of each participant. If all rewards were actually given, each would have to be trivial in amount. With delayed rewards, this problem is usually avoided by randomly selecting only one of the answered questions and, if a delayed reward had been chosen, sending the reward to the participant by mail at the specified time (Madden et al., 2003). With social rewards, the difficulties are even greater because the reward may go to another person. In the present experiment, real monetary rewards were given by asking participants to come to the laboratory with a list of names and addresses of people at various positions on their lists of the 100 people closest to them (specifically, persons #1, #2, #5, #10, and #20), as well as their own name and address. As with delay

Figure 5: From Rachlin & Jones (2008a). The median amount of money forgone to give $75 to another person at each social distance. The error bars span one standard deviation. The solid line is the best-fitting version of Equation 1.



discounting, one answer was selected randomly. For the real-money group, the amount chosen, in $5 money orders, was put into a stamped envelope in the participant's presence, addressed to either the participant or the other person on the participant's list, and sealed. Then, the envelope was sent to the appropriate person (with a note indicating that the money was given by the participant as part of a psychology experiment) after the session was over. For the hypothetical-money group, the same procedure was followed except that the participant understood when making choices that no money would actually be sent. The purpose of the experiment was to compare the social discount functions obtained with the real and hypothetical rewards.

3.1 Method

3.1.1 Participants

Participants were 40 undergraduate students (16 male, 24 female) recruited through the psychology subject pool at the State University of New York at Stony Brook.

3.1.2 Procedure

The website on which participants signed up for the experiment included the following instructions:

> Imagine you made a list of the 100 people closest to you in the world, ranging from your dearest friend or relative at #1 to a mere acquaintance at #100. Now determine approximately who would be person #1, #2, #5, #10, and #20 and BRING A NAME AND PHYSICAL MAILING ADDRESS FOR EACH OF THOSE PEOPLE AND FOR YOURSELF. If you do not have an address for one of those 5 numbers, bring an address for someone very close to that number (for example, you could bring the name and address for person #18 if you do not have an address for person #20). If you do not bring physical copies of these addresses, you will not be permitted to participate! The whole experiment will last no longer than 1 hour.

Half of the participants were randomly assigned to the real money group (n = 20); the other half were assigned to the hypothetical money group (n = 20). Each participant met individually with the experimenter. The participant and experimenter sat at opposite ends of a small table. After giving consent, the participant was asked to produce the list of names and addresses as described in the website instructions above. Six participants failed to do so, and they were given the opportunity to reschedule the session for another day (five of the six rescheduled and an additional participant was recruited to replace the participant who failed to do so). After providing the list of names and addresses, the participant was told to which of the two groups they had been assigned. The

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meaning of this group assignment was explained as either, "None of your choices will be for actual money, but we ask that you still make choices as if real money were involved," or "One of the choices you make will be for real money." The participant was then handed a small booklet and asked to read the instructions on the first page of that booklet. The instructions were as follows:

> The following experiment requires that you have imagined making a list of the 100 people closest to you in the world ranging from your dearest friend or relative at position #1 to a mere acquaintance at #100. I have asked you to identify the name and physical address for persons #1, #2, #5, #10, and #20.
>
> On the following pages you will be asked to make choices between an amount of money for yourself versus an amount of money for each of the people you have identified from your list. Please make your choices as if real money was involved.
>
> Please look ahead at the choices you will be making. If you have any questions about the task, please ask them now.

For the real-money group, an additional paragraph was inserted prior to the last paragraph of instructions. The inserted paragraph read:

> **One of your choices will actually be for real money!** After you have completed all of the following pages, I will randomly select one of those pages. Then I will randomly select one of the choices you made on that page. If you chose "A", we will mail you the amount of money that you chose for yourself on that line. If you chose "B", we will mail the designated person the amount of money that you chose for him or her on that line (i.e., $30). We will send you or the person you have designated postal money orders for the amount you have chosen.

Five social distances [N's] were presented: #1, #2, #5, #10, #20, each on its own page. The amount of money to forgo in order to give $30 to another person was presented in 7 increments ranging from $30 to $0. For half of the participants in each group, the pages were organized in ascending order of social distance (person #1, #2, #5, #10, and #20); for the other half, in descending order. Each page contained the following instructions:

> Imagine you made a list of the 100 people closest to you in the world ranging from your dearest friend or relative at #1 to a mere acquaintance at #100.
>
> Write the name of the person who is #[N] on your list:_________________.
>
> Now imagine the following choices between an amount of money for you and an amount for the **#[N]** person on the list. Circle A or B to indicate which you would choose in EACH line.

A. $30 for you alone or	B. $30 for the **#[*N*]** person on the list.
B. $25 for you alone or	B. $30 for the **#[*N*]** person on the list.
. . .	. . .
B. $0 for you alone or	B. $30 for the **#[*N*]** person on the list.

For half of the discount tests, the money amounts in Column A decreased as shown above; for the other half, the amounts in Column A increased from $0. The experimenter then took the booklet, turned to the first page and showed the participant how to interpret the instructions and choices on that page.

The experimenter wrote the name of the #[N] person from the participant's list. It was pointed out that all the choices on that page would be with respect to that named person. It was explained that the participant should then choose A or B on the first line and continue making such choices for all the lines on that page. The experimenter then flipped through the pages, showing the participant that he or she would be making similar choices for all of the people on the provided list.

For participants in the real money group, the experimenter further demonstrated what would happen upon completion of all the choices provided in the booklet. The participant was shown 2 sets of playing cards. One set (of 5 cards) represented the 5 choice pages. The other set (of 7 cards) represented the 7 choices on each page. The experimenter explained that he would first shuffle the set of 5 cards and allow the participant to draw one at random to select one of the potential recipients, then shuffle the set of 7 cards to select one of the choices. The experimenter showed the participant the actual money orders and envelope that would be used to send money to whomever had been selected to receive it. The participant was also shown a note that would be sent with any money order. The note indicated that the money was being sent as a result of choices made by the (named) participant in a psychology experiment. There were no other details in the note (e.g., about any of the specific choices the participant made).

For participants in both groups, the experimenter read each choice aloud and demonstrated each choice alternative with two fanned piles of imitation money (in denominations of $5, $10, and $20). Thirty dollars was placed atop the list provided by that participant to indicate

$30 for the designated individual. With each choice, the other pile, placed in front of the participant, was altered to signify how much money he or she would receive by choosing A. When reading the question, the experimenter would substitute the name of the listed individual for person #[N]. For example, he might have said, "Would you prefer $10 for yourself, or $30 for Fred?"

Upon completion of the last page of choices, participants were handed a 1-page questionnaire to fill out which included demographic information. Participants in the real money group then experienced the playing-card procedure described above. Such participants then wrote the name and address of the designated recipient on an envelope and a postal money order for the appropriate amount of money. The experimenter mailed the money order within 2 business days.

3.2 Results

A crossover point was determined for each page of the questionnaires. The crossover point was the average of the largest amount the participant was willing to forgo and the smallest amount the participant chose for him or herself. For example, if a participant preferred $30 for person #10 over $10 for herself, but preferred $15 for herself over $30 for person #10, the crossover point for person #10 would be $12.50. In other words, the crossover point was the point at which the participant switched from choosing A to choosing B, or vice versa. One participant in each group switched between A and B multiple times on individual pages; those participants' data have been excluded from all analyses. Exclusive preference for option A (money for oneself) was considered a $0 crossover point; exclusive preference for option B (money for another person) was considered a $32.50 crossover point.

Figure 6 shows the median crossover points for each of the 5 social distances tested for both the hypothetical (top panel) and real (bottom panel) money groups. The solid regression line is the best fit of Equation 1 for each panel. Prior social discounting experiments with hypothetical rewards used higher undiscounted amounts (usually *V* = $75). Many participants in prior experiments preferred $75 to be given to N = 1 and N = 2 than to receive $75 themselves. Allowing *V* (as well as *k*) to vary in fitting Equation 1 to median crossover points, obtained *V* varied from $80 to $90. With the present data, obtained *V*, when it was allowed to vary, was very close to the nominal value of $30 (within $5) for both groups; therefore, in fitting Equation 1 to the data, only *k* was allowed to vary. The resulting *k*-values were 0.052 and 0.065 for the hypothetical and real money groups, with variance accounted for (VAF) of 84% and 92% respectively.

Area under the curve (AUC) is a normalized measure not dependent on functional form (Myerson, Green, & Warusawitharana, 2001). It is calculated by first expressing crossover points as a percentage of V then connecting the crossover points by straight lines, then summing the areas of the trapezoids so formed, then dividing that sum by the rectangle, $V \cdot N_{MAX}$. AUC varies from 1.0 (no discounting) to 0.0 (complete discounting). In the present experiment, AUC was 0.68 and 0.62 for the medians of the hypothetical and real money groups respectively.

Equation 1 was fit to the crossover points of each participant individually. The median individual *k* was 0.056 (76.1% VAF) for the hypothetical money group and 0.068 (68.7% VAF) for the real money group. The median AUC was 0.69 for the hypothetical money group and 0.61 for the real money group. The interquartile range for individual *k*-values was 0.13 and 0.059 for the hypothetical and real money groups; the interquartile range for individual AUC-values was 0.29 and 0.16 for the hypothetical and real money groups.

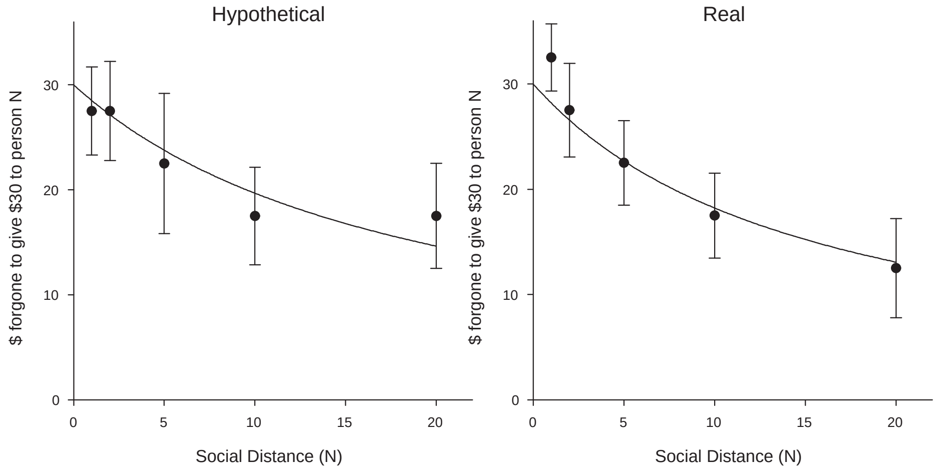
Unpaired t-tests found no significant difference in individual *k* between the two groups (t = 0.22, p = .827, d = .075). As is typical with social discounting (Rachlin & Jones, 2008a), the distribution of individual *k*-values was highly skewed; transforming *k* to log *k* significantly reduced the skew of the distribution but the difference in log *k* between the groups was still not significant (t = 0.56, p = .580, d = .181). The variance of the crossover points for the real-money group was significantly less than that for the hypothetical-money group for *k* (F = 2.89, $p < 0.05$) but not for log *k* (F = 1.58, p = 0.34) or AUC (F = 1.56, p = 0.35). Similarly, AUC comparisons also found no significant difference between individual AUCs of the two groups (t = 0.22, p = 0.824, d = .073).

Statistical extrapolations were performed to determine the approximate number of participants likely needed for each group to find a statistically significant difference in *k* and AUC measures. This was accomplished by simply duplicating the obtained results for each of the 19 participants in each group until the t-test yielded a p-value of .05 or less. These analyses indicated 1425 participants would be needed for each group for a significant difference in *k* and 1387 participants per group for a significant difference in AUC.

3.3 Discussion

The steepness of social discounting (as measured by both *k* and AUC) was slightly but not significantly greater with real than with hypothetical money. Of course, the null hypothesis cannot be proven true. But based on Cohen's (1988) guidelines, the effect size was less than small (d < 0.2) across all measures of social discounting (*k*, log *k*, or AUC). It is possible that, with a much larger sample size, the difference in social discounting would have reached significance. Our statistical extrapolations indi-

Figure 6: The median amount of hypothetical (left panel) and real (right panel) money forgone to give $30 to another person at each social distance. The error bars span one standard deviation. The solid line is the best-fitting version of Equation 1.



cated that over 2500 total participants would be needed to find such a difference. The validity of such analyses in determining the exact sample sizes needed depends upon the assumption that the obtained samples perfectly represent the population—an assumption that is almost certainly false. Still, these results do suggest that substantially larger samples would be needed to find statistically significant differences in either *k* or AUC. And even if the difference in social discounting was ultimately shown to be statistically significant, that difference is not great (about 9% in AUC). Furthermore, the obtained *k* for the real money group in the present study is within the range of median *k*-values obtained with hypothetical money in previous research (Jones & Rachlin, 2006, 2009; Rachlin & Jones, 2008a, 2008b, 2009).

In another hypothetical-money experiment, Rachlin & Jones (2008a) found that *k* increased with increases in money amount given to others (Column B). That experiment is the only one prior to the present study that has used amounts for others less than $75. For Column-B amounts of $7.50, $75, and $75,000, Equation 1 produced *k*-values of 0.06, 0.10 and 0.17, respectively (with correspondingly inflated *V*s). It is possible that what little (statistically insignificant) differences there are in *k* and AUC between the hypothetical and real money groups in this experiment are due to effective differences in reward magnitude. That is, choices for real money might produce steeper social discounting because such choices are functionally equivalent to choices for larger magnitudes of hypothetical money. For instance, a choice of $30 of real money might be equivalent to a choice of $40 of hypothetical money. Although such a conversion (from hypothetical to real) might be possible, the minimal and statistically insignificant difference between the two groups in the present study suggests that pursuit of such a conversion factor would be premature.

One potential problem with the present study is that the real rewards were not only potentially social but also delayed—by the time taken to mail and receive the letters—and probabilistic in the sense that any chosen alternative had a 1/35th chance of actually being received. However, since money is not generally a primary reinforcer, monetary rewards involve delay. Moreover, the money orders sent to the recipients were a step further removed from primary reinforcement than cash. But these problems are inherent in discounting; they occur frequently in the real-world choices to which the present research is intended to apply.

4 Conclusions

Although there are problems with hypothetical rewards in the laboratory, real rewards create their own set of problems. With hypothetical rewards, participants are asked to imagine a real-world context and to choose within that context. When real rewards are substituted for hypothetical ones, a context is provided in the laboratory, but it is much narrower than the real-world context. Where real laboratory rewards are strictly monetary, there is an implied instruction to maximize monetary amounts. In their classic review of probabilistic choice studies, Bruner, Goodnow, and Austin (1956) found that, with the possibility of winning or losing real money, people tended to maximize reward by exclusively choosing the reward with the higher expected value. But, with hypothetical or token rewards, people tended to distribute their choices proportionally to the probabilities. Questioning revealed that, with hypothetical or token rewards, the participants attempted to "win" or guess correctly on *all* trials; they believed (correctly) that the experimenters would balance the outcomes (i.e., not allow long strings or repeating patterns). If, as was conceivable, the implied goal was to "...aim for a perfect or unique solution of the problem..." (p. 193), such behavior would have been completely rational.

The Bruner et al. study raises the question: Is behavior in everyday-life situations approached more closely in the laboratory by asking people to imagine what they would do in those situations or by giving them real but lesser (or different) rewards than those situations entail? The current results do not definitively answer this question (there may be no definitive answer) but they provide some evidence for the validity of the former procedure. Real monetary rewards obtained in the laboratory impose a context of narrow maximization of monetary amount that may differ from the real-world context of primary interest to the experimenter. A participant in the laboratory, trying to imagine what she would do in a hypothetical real-world situation, may come closer to that situation than one simply trying to maximize monetary reward. This is especially the case in studies of altruistic behavior and cooperation. It would be self-defeating to study altruistic behavior in a context that encourages participants to maximize their own monetary rewards. In real-money laboratory experiments participants may tend to maximize their own monetary earnings and could conceivably act less altruistically than they would in more complex real-world situations. It is therefore not a foregone conclusion that results obtained with use of real money in the laboratory are more veridical than those obtained with use of hypothetical money in hypothetical real-world situations. Aside from the practical limitation of real-money amount, use of real money may create an implied requirement to maximize monetary reward and ignore other dimensions of reward that may be present in the real-life situations of ultimate application.

Both hypothetical and real money laboratory studies thus have their strengths and weaknesses. To some extent, the strengths of one method complement those of the other; corresponding results with the two methods each reinforce conclusions obtained with the other. It is therefore important to validate results obtained by either method by comparison with results obtained by the other—as was done in the present experiment.[2]

[2] In our opinion it would be least useful to combine a hypothetical context with a real reward grossly out of line with that context—for example, telling participants to imagine that they were employers or workers for a firm and then giving them "profits" or "salaries" in small amounts of real money.

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