Intermediation Reduces Punishment (and Reward)*

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Abstract

This paper investigates how punishment changes when a transgressor does not directly interact with the injured party. In a laboratory experiment, third party punishment for keeping money at the expense of a poorer player is shown to decrease when an intermediary actor is included in the transaction. Follow-up treatments provide evidence that intermediation reduces punishment predominately because the selfish player does not directly interact with the poorer player when an intermediary is used. Additionally, the experiments are designed such that when the selfish player chooses to include the intermediary, the poorer player can only be made worse off. Thus many current theories of fairness would incorrectly predict intermediation increases punishment. The results support the hypothesis that judgments of fairness are consistent with current theories but that judgments narrowly focus on direct interactions. Narrow judgment can have grim equilibrium consequences. When an intermediary is made available, punishment is almost entirely ineffective in moderating self-interest, and the poorest players are far worse off than when no intermediary is allowed. Primarily as a test of generalizability, this paper also investigates moral decision-making and indirectness in a charity-reward domain. Consistent with the laboratory results, a framed field experiment shows rewards of a charitable behavior (donating mosquito nets) to decrease when the saliency of an intermediary (a charity) is increased. Together, the results show that moral decision-making is not always well predicted by the *overall* fairness of an act but rather by the fairness of the consequences that *follow directly* from an act. The implications of these results are that allowing indirect actions may lead to increased anti-social behavior.

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1 Introduction

> *"Each society learns to live with a certain amount of... mis-behavior; but... society must be able to marshal from within itself forces which will make as many of the faltering actors as possible revert to the behavior required for its proper functioning."*
>
> - Albert O. Hirschman (1970)

In August 2005, Merck sold the rights to a cancer drug to a small pharmaceutical company, Ovation, that immediately raised the price charged to patients by a factor of 7 and later by a factor of 10.[1] Firms often worry about negative reactions to price-gouging (e.g. Kahneman, Knetsch & Thaler 1986), but was Merck not equally concerned about consumer outrage in response to selling to a firm that might raise prices so dramatically?[2] This paper does not aim to fully explain behavior in this scenario but rather to answer two provocative question that it raises. First, is Merck avoiding blame simply by not doing the deed itself? That is, what if Merck was known to be solely responsible for and solely benefitting from the price increase? Could Merck possibly avoid some loss of goodwill merely because the price increase is not perceived to be a direct consequence of its actions? Second, if this is true, what would it teach us more generally about how we punish?

This paper investigates these two questions of moral decision-making in the laboratory, where we have direct measures of punishment as well as increased control over objective measures of culpability. In the game, the first mover (Merck) has the option of being selfish at the expense of a poorer player (the patients) and whether to do this directly or through an intermediary agent (Ovation). An unaffected third party (i.e. the regulator, other customers) then has the opportunity to punish the first mover. The data show that the first mover is punished significantly less for keeping money when she "intermediates", that is, when she intentionally adds another actor between her action and the outcome. Several experimental controls show that this decrease in punishment is not due to diffusion of responsibility, punisher confusion, lack of thought by the punisher, or merely the inclusion of a third party. Rather, we find that, across treatments, punishment decreases if and only if including the intermediary allows the first mover to avoid "directly interacting" with the poorest player; that is, the selfish player is not punished as harshly if she does not take an action that changes the status quo of the poorest player before another action is taken.

[1] Berenson, Alex. 2006. "A Cancer Drug's Price Rise is Cause for Concern." *New York Times.* March 12, Business section.

[2] The small company, Ovation, raised prices on three different drugs in 2005 by at least a factor of 10 shortly after purchasing them.

The experiment is also designed to pit the hypothesis that intermediation reduces punishment against predictions offered by economic models of fairness. In our experiment, using the intermediary can only harm equity and decrease the payout of the poorest player. Further, subjects are shown to believe intermediation will greatly hurt the poorest player and decrease equity. They are also shown to believe that others share this belief. Thus, one might predict that subjects would actually punish intermediation more. In other studies, subjects have been shown to have a preference for both equating outcomes (Fehr & Schmidt 1999) and maximizing the minimum payoff (Charness & Rabin 2002) and are willing to punish deviations from these norms (Fehr & Fischbacher 2004, Carpenter & Matthews 2009). Since intermediation is a deviation from both norms in our game, the punishment decisions run counter to these findings. We do not suggest the norms are absent in our experiment. Instead, since the existence of these norms seem to be so robust, but directness drives our results, we conclude that directness may be a first order concern in moral decision-making. Furthermore, the data are consistent with the hypothesis that fairness judgments use these previously identified norms, but that judgments merely narrowly focus on, or overweight, direct interactions.

Moreover, since pro-social norms are not as strongly enforced with intermediation, socially preferable outcomes occur less frequently; the poorest player is dramatically worse off, making 36% as much as her counterpart in the same game without intermediaries. When the mere availability of indirectness is introduced to the environment, punishment becomes almost entirely futile, failing to sustain pro-social behavior. Additionally, when intermediaries are not only available but used, the poorest player makes less than 5% as much as the recipient does in the game without intermediaries. Though this comparison is subject to selection, it helps illustrate the potential anti-social equilibrium consequences of intermediation reducing punishment.

In a follow-up study, subjects' behavior in a charity and reward context is consistent with overweighting direct consequences. The experiment varies the saliency of the fact that charitable donations pass through an intermediary before reaching the recipient. Increasing the saliency of the intermediary in descriptions given to unaffected third parties considerably decreased rewards they gave to donors. Together, the results suggest that indirectness of behavior attenuates both punishment or reward, and we hypothesize that this is perhaps so because indirectness decreases the total perceived "badness" or "goodness" of an act.

There exists a large literature in both Economics and Psychology pertaining to fairness generally as well as fairness with multiple parties. Some papers will be mentioned in the behavioral predictions section, but we will review this literature more extensively after the results, in Section

5. This structure will allow for a discussion of the literature in relation to the current results.

The study of intermediation is important because of its ubiquity. Intermediations such as Merck's sale of its cancer drug are common. Many companies may face less backlash for outsourcing production to other firms who cut costs through questionable labor practices or by allowing more pollution than they would face if they took the same actions themselves. For example, many companies have their products produced in factories in China that use a dirty coal technology.[3] One could imagine a far greater public reaction if these companies themselves owned the factories that were emitting the CO_2 into the atmosphere. Similarly, firms can avoid being labeled a "patent troll" if they sell a patent to another firm willing to sue the infringing party.[4] Intermediation can be, and often is, less obvious than supply chains, selling, or outsourcing. Fehr, Hart & Zehnder (2008) show that experimental "suppliers" shirk less often when paid a low price if the "buyer" defers price determination to a market process rather than choosing a low price directly. This is true even though the market has a known, and experienced, low outcome. Charness (2004) similarly shows reciprocity is lower when wage is determined by a third party. There are also many real world examples of firms having a staffing company or an industrial psychologist lay off workers, hoping perhaps that the employees might harbor less ill will towards the firm if employers do not deliver the pink slip themselves.

Intermediation is not restricted to firm and worker relationships. In divorce proceedings, it would be inappropriate if one called his soon-to-be ex wife a terrible mother (especially in front of one's children), but maybe this impropriety is attenuated if one has his lawyer say it on his behalf. Like all of these examples, the experiment within will consider human intermediaries, though perhaps the concept can be more generalizable. Giving a friend cash for her wedding might be considered uncouth, but if the money is given through an online gift registry (by purchasing the item she has already selected), this might be viewed as more palatable.

The paper proceeds as follows. Section 2 details the laboratory experimental designs. Section 3 discusses predictions for the experiment from leading models of fairness. Section 4 describes the results from the laboratory experiments. Section 5 explains the design as well as the results of the framed field study involving charitable actions and rewards. Section 6 reviews related literature and discusses how our results relate. Finally, Section 7 provides concluding remarks.

[3]Spencer, Jane. 2007. "Why China Could Blame its CO_2 on the West." *Wall Street Journal*. November 12, The Outlook section.

[4]Elinson, Zusha. 2009. "Intellectual Ventures Takes an Indirect Route to Court." *The Recorder*. September 1.

2 Experimental Design - *The Intermediation Game*

The Intermediation Game has four players - first mover, intermediary, receiver, and punisher (See Appendix, "Experiment Instructions" for exact instructions as given to participants). The first mover owns a dictator game (henceforth "DG") worth \$10 to be played with the receiver. She has two options of what to do with the DG. She can play the game herself, or she can "sell" it to the intermediary. If she chooses to sell, she also chooses the price the intermediary must pay her for the DG. The first mover's strategy space is the same whether she is choosing how much to keep in the DG or the price for which she will sell it: {\$5, \$6, \$7, \$8, \$9, \$10}. If the DG is sold to the intermediary, she plays it with the same receiver, and she must keep at least as much as she was forced to pay; she cannot lose money. The intermediary has a \$5 endowment, equal to half the size of the DG. Thus, holding fixed the rents extracted by the first mover, using the intermediary weakly reduces equity and weakly makes the poorest player (the receiver) worse off.

Finally, the punisher can then reduce the payoff of the first mover (and only the first mover). The punisher may base her punishment decision on (i) the amount sent in the DG, (ii) whether the DG was sold, and (iii) for how much the DG was sold. The punisher can costlessly reduce the first mover's payout to any non-negative amount. Punishment decisions are elicited via the strategy method: The punisher gives a decision for all possible outcomes of the game the other three are playing, and her decision for the scenario that is realized is enacted[5]. The punisher is endowed with \$5. Thus, the intermediary and punisher both begin with \$5, and the first mover is deciding how to split \$10, so a 4-way, \$5 equal split is clearly feasible and can be guaranteed by the first mover if she does not sell the DG.

The intermediary has no say in the selling or pricing decisions, so the experiment avoids any complicity of the intermediary in the first mover's rent extraction. Punishment was costless in order to increase power. The task of the punisher is akin to the task of a judge. Judges are not assessed costs for penalties they mete out; the effect their decisions have on others are incentive for truth-telling and thoughtful decision-making. The data do not reveal any patterns of careless decision-making, so this assumption seems valid.[6] The strategy method was utilized, so to minimize the number of scenarios posed to the punisher, all transactions in the game were in multiples of

[5]The strategy method may induce "cold" decision-making on the part of the punisher. In Paharia et al (2009), differences in moral judgments of direct versus indirect acts disappeared when the two scenarios were judged side-by-side; the similarity in the wrongness was too salient. Thus, we expect this design decision should attenuate any punishment differences we may observe. There is no reason to suspect it would drive or increase the main results.

[6]In pilot experiments, punishment was costly. There was less punishment overall, but the punishment dynamics described in section 4 were the same.

$1 (Sell prices and DG allocations). This reduced the number of scenarios to 27. The punisher was thus asked for 27 punishment decisions, one at a time, in a random order. The order was reversed each subsequent session. The same random order was used in other treatments (as well as the reversal if there were two sessions). In each session, the game was repeated four times, with each subject playing each role once. They were informed that they were re-matched into new groups in each period (To see an analysis of order effects and within-subject consistency, see Appendix Section 8).[7] They were paid for their earnings in one randomly chosen period. This is to discourage thinking of the game as a four period meta-game but rather focus on maximizing each period. To minimize learning about others' behavior, no feedback was given until all four periods were completed.

After the four periods, but before feedback was given, subjects were asked for their beliefs of how much the intermediary kept for each transaction price. They were paid $0.50 if they were within $0.50 of the average of how intermediaries actually behaved in that session (and $0.50 if this scenario did not occur in this session)[8]. In the final session of the *Intermediation Game*, they were then asked to guess the beliefs of others. They were paid $0.50 if they were within $0.50 of the average guess of everyone in the room from the questions previously answered. After these third party, second order beliefs were obtained, feedback was given.

All experimental sessions were performed at the Computer Lab for Experimental Research (CLER) at Harvard Business School. All sessions consisted of 12, 16, 20, or 24 study participants recruited via email from the Boston area. All subjects were under the age of 30.[9] They were paid a $10 show up fee plus their earnings from the experiment, which averaged $4.57 for the game and $3.37 for the belief elicitations. The experiment was conducted at computer terminals using z-Tree 2.1.4 (Fischbacher, 1999). No session took longer than one hour. 64 subjects participated in August and September of 2008.

A framed field study in a charity-reward domain was also employed. Its design and results follow in section 5.

[7] Given sessions as small as 12 subjects, we could not eliminate being in the same group as another subject twice. Subjects were never in the same group as another subject in consecutive periods. When they were in the same group twice, the subject did not know with whom or when.

[8] This incentivizes subjects to report the midpoint of the $1 interval they believe to be most likely for their session. Beliefs were elicited in this way in order to make it simple and understandable for the subject.

[9] In one session, the recruitment software experienced a bug and allowed subjects over 30 to sign up. These data are not included in the analysis. The main results do not change with their inclusion. In sum, over-30's punished more, also punished intermediation less, and exhibited more outcome bias. The over-30's were confused by the instructions and were unfamiliar with computers, and consequently they did not finish in one hour and were let go.

Control - *No Intermediary Treatment*

To evaluate the welfare effects due to the presence of intermediaries, a control experiment, with no intermediary, was also run. Thus the control was an instantiation of the *Third Party Punishment Game* (TP-DG, Fehr & Fischbacher 2004). Otherwise, everything was consistent with earlier sessions: punishment was costless, the punisher made $5, and the DG was worth $10. Throughout, this will be referred to as the *No Intermediary* treatment. This treatment was run in October 2008 and had 24 subjects.

Two Punishments Treatment

To understand better how subjects allocate blame, we added a treatment that was identical to the *Intermediation Game* but allowed the punisher to punish *both* the first mover and the intermediary. The punishment mechanism does not change: Punishment is costless, and the punisher cannot reduce either player's payoff to negative amounts. She can punish both, one or neither. The amount she chooses to punish one does not affect her strategy space for the other punishment decision. The punishments are again elicited using the strategy method, and though the scenarios are presented one at a time, the punishment decision for the first mover and the intermediary are elicited simultaneously. This treatment was run over two sessions in March 2009 with 20 and 16 subjects in the two sessions.

The Reflection Treatment

To encourage subjects to think about and see if they understood the game and particularly the strategies of other players, they were asked in this treatment to think about the game and write down their thoughts beforehand. This treatment was exactly the same as the *Intermediation Game* except subjects spent four minutes before the game writing on four blank sheets of paper for a minute apiece reflecting on four questions: (1) Why might the first mover sell the game? (2) Why might the first mover not sell the game? (3) What will happen if the first mover does not sell the game? (4) What will happen if the first mover sells the game? The questions are intentionally neutral and balanced across scenarios of selling and not selling. This treatment was to encourage the subjects to reflect about what is going on in the game and to test if the results hold for subjects who are clearly aware of the potentially dubious motives of other players. As Paharia et al (2008) suggest, moral judgments seem to be intuitive, and when subjects are forced to reflect on motives, judgments can change significantly. This session was run in October 2008 and had 24 subjects.

The Allow-Taking Game

This game reframes *The Intermediation Game* to test the hypothesis that intermediation reduces punishment precisely because when the intermediary is used, the first mover is not directly interacting with the receiver. In this treatment, the first mover first plays a $10 DG with the receiver, keeping $5, $6, $7, $8, $9, or $10 for herself. She then decides whether to allow the intermediary to take from the money the first mover sent the receiver in the DG.[10] If she allows the intermediary to take, the intermediary can take any non-negative integer amount up to the amount that the receiver was sent in the DG. The punishment technology and elicitation is identical to those in *The Intermediation Game*. Thus, *The Allow-Taking Game* is a re-framing of *The Intermediation Game*, except the first mover directly interacts with the receiver whether or not the intermediary is included in the game. This session was run in October 2009 and had 24 subjects.

3 Behavioral Predictions

There is a class of models that make clear predictions for the *Intermediation Game*. None will predict less punishment for rents extracted through the intermediary. Moreover, the most recent models will predict that punishment will be *greater* for money made via intermediation. That is, the public might punish Merck *more* for selling the drug to an un-punishable intermediary.

The simplest way to separate the competing theories is to ask what punishment levels they predict when the payoff outcome is the same, but the path taken is different. For example, the outcome is the same if the first mover keeps $X in the DG or if she sells the DG for $X and the intermediary then keeps $X, making zero profit. In both cases, the resulting payout is ($X, $5, $10-$X, $5) for the first mover, the intermediary, the receiver, and the punisher respectively (Recall the intermediary and the punisher are endowed with $5 each). We will consider what punishment dynamics each class of models may predict for each path. We will use "Direct" to refer to the path where the first mover does not sell the DG and plays the DG herself, and we will use "Indirect" to refer to the path where the first mover sells the DG.

[10] To see if either switching from a giving frame to a taking frame or switching from a market transaction frame - selling the DG - to a transfer frame drives the results of this game, see the design and results for *The Who's-The-Dictator Game*, Appendix Sections A and B.

Outcome-based Models of Fairness: If the punishers have preferences based solely on the outcome, as in Fehr & Schmidt (1999) or Bolton & Ockenfels (2000), then, punishments will be independent of whether the intermediary was used; punishment will be identical in the two cases raised above.

Intentions-based Models of Fairness: Rabin (1993) and Dufwenberg & Kirchsteiger (2004) define fairness through intent, and the relevant piece of intent for *The Intermediation Game* is beliefs:[11] If I believe you believe I will play a_i, and you play b_j, then I judge your fairness based on what profits would result from a_i and b_j regardless of what I actually play. Extending this framework for the *Intermediation Game*, beliefs take the form, "I believe the first mover believes the intermediary will keep \$Y when the DG is sold for \$X (with Y≥X), so when the first mover sells the game for \$X, I will judge her for directly choosing the allocation (\$X, \$5+(\$Y-\$X), \$10-\$Y, \$5) regardless of the actual actions of the intermediary." Thus, in comparing this action (selling the DG for \$X) to not selling the DG and keeping \$X directly, the punisher judges the fairness of choosing (\$X, \$5, \$10-\$X, \$5) (no intermediation) versus (\$X, \$5+(\$Y-\$X), \$(10-Y), \$5) (intermediation). Notice that the two allocations differ only by a \$(Y-X) transfer from the receiver to the intermediary, a transfer from the poorest player to a player already endowed with the average pre-punishment payout (\$5). Rabin (1993) and Dufwenberg & Kirchsteiger (2004) do not define preferences over others' payouts (but rather focus on refining our understanding of intent). However, if a punisher in their setup is indifferent over who receives the \$(Y-X), punishment is expected to be identical whether the first mover keeps \$X in the DG or via intermediation. If punishers are not indifferent about others' payoffs, these intentions-based models are also outcome-based and make the same predictions as the models discussed in the following subsection.

Intentions and Outcome-based Models of Fairness: Their experimental evidence leads Falk, Fehr & Fischbacher (2008) to conclude our best models of retributive behavior in standard laboratory games assume fairness is based on both intentions and outcomes. Falk & Fischbacher (2004) provide the first such unifying framework. However, agents in their model only have preferences over outcomes relative to their own. We adapt this model to include preferences over third party outcomes using the general model of Charness & Rabin (2002). They claim, "[subjects] like to maximize the minimum payoff among players."[12] This seems a reasonable extension and is experimentally demonstrated by Charness & Rabin as well as Engelmann & Strobel (2004) to be a salient fac-

[11] The other piece of intent is the strategy space from which the judged action was chosen, but the strategy space is held constant in our game, so this will not be a part of the discussion.

[12] The prediction that will follow does not change if instead agents have a preference for equity over others' payoffs since intermediation also weakly damages equity. Other than indifference, equity and maximin are the leading candidates in the literature of preferences over third party payouts. Though efficiency is also an important parameter (See Charness & Rabin (2002), Engelmann & Strobel (2004)), all moves in the pre-punishment stage are zero-sum.

tor in determining preference over others' payouts.[13] Recall from the discussion in the *Intentions* subsection that intermediation weakly decreases the minimum payout, and more importantly, we will show subjects *believe* that the first mover *believes* that intermediation weakly hurts the poorest player. Thus, in this hybrid model, punishment is predicted to be weakly greater when the first mover uses the intermediary. More interestingly, this inequality will be strict if the punisher believes the first mover believes $Y>$X.

Diffusion of Responsibility: Charness (2000) and Bartling & Fischbacher (2008) both experimentally demonstrate that including a second, human agent who is partly responsible for the outcome will partially absolves the initial agent. In Bartling & Fischbacher's model of this concept, an agent's responsibility is determined by her share of the total increase in the probability of the bad outcome occurring due to human agents; thus, if a second party increases the likelihood of the outcome, the principal's responsibility decreases for the same bad outcome, holding fixed her actions. However, in our game, even when the first mover sells the game for $X, she has, at the very least, guaranteed the receiver cannot be paid more than $10-X; thus, she is at least as responsible as she is when not selling the game and keeping $X. Further, by putting the DG in the hands of an unpunishable party, the probability of a worse outcome for the receiver weakly increases, so doing so, in fact, may make the first mover weakly *more* responsible than she would be had she not sold the game, and we would expect weakly more punishment. In addition, this model makes an even clearer prediction when the intermediary is perfectly altruistic. When the second agent does not transgress, she is not responsible for the outcome; thus, responsibility is not diffused, and punishment should not decrease. It thus seems reasonable that in any case, we should expect weakly more punishment for intermediating.

4 Results

The results in this section confirm the main hypothesis: Intermediation decreases punishment. This is not due to diffusion of responsibility, punisher confusion, lack of thought by the punisher, or merely the inclusion of a third party. Rather, the results suggest that intermediation reduces punishment because it allows the first mover to avoid directly interacting with the poorest player. Moreover, first movers frequently use the intermediary. This shifts money from the poorest player to the endowed intermediary. As a result, the poorest player is considerably worse off due to the presence of the intermediary.

[13] Johansson and Svedsater (2009) support this claim, showing third parties have a preference for equity among other players so long as they are not at a disadvantaged payout, which is true in *The Intermediation Game*.

In *The Intermediation Game*, each punisher was asked to make a punishment decision for the same 27 scenarios (see Appendix, Table 8 for complete list of scenarios & average punishments) facilitating matched, nonparametric analysis of the data, which will be employed alongside OLS regression analyses. Unless otherwise noted in this section, reported results are from *The Intermediation Game* treatment.

Table 9, in the Appendix, provides summary statistics for a general understanding of how the game was played. 81% of subjects punish at least once, and when they do punish, it is economically substantial, almost equal to average profit and fully 58% of the first mover's pre-punishment payout (See Table 10 in the Appendix for more details on what may have determined the size of punishments). Figure 1 also shows just how large average punishments were in the experiment, with an average punishment over $5 when the first mover keeps $10 in the DG herself. Also note that 52% of subjects use the intermediary, so it is not an irrelevant alternative. Result 1 may help explain why.

Result 1 - Punishment significantly decreases when money is made through intermediation rather than directly.

Figure 1: **Average Punishments**

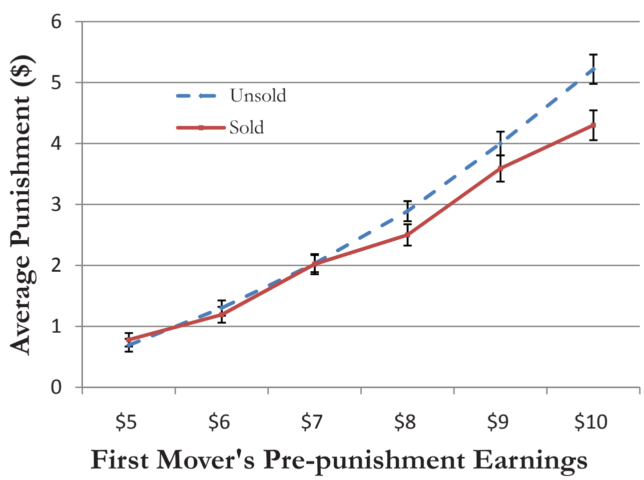

Figure 1 and Table 1 show that when the first mover keeps more than $7, directness is punished more harshly: Average punishment is greater when the first mover extracts rents directly, and many subjects punish in this direction. At these profit levels, at least 2.5 times as many subjects punish direct actions more harshly than subjects who punish indirect actions more harshly. Using nonparametric tests - both a Wilcoxon signed rank test and a Fisher Pitman matched-pair permutation test - the punishment distributions are shown to be significantly different for each of these high profit levels of the first mover (See the last two columns of Table 1). In other words, when punishment (and misbehavior) is high, direct actions are frequently and significantly punished more.

Table 1: Directness is Punished More Harshly[a]

	Avg. Punishment		# Subjects Who Punish		p-values	
Profit of First Mover	DG Unsold	DG Sold	Unsold More	Sold More	Matched Pair, Signed Rank[b]	Permutation Test[c]
$10	$5.22	$4.30	24	7	<0.01	0.01
$9	$4.00	$3.52	15	6	0.05	0.07
$8	$2.89	$2.48	20	8	0.02	0.07
$7	$2.03	$1.98	16	12	0.58	1.00
$6	$1.30	$1.41	11	6	0.22	0.27
$5	$0.69	$0.78	2	1	0.55	0.50

[a]To hold outcomes constant, only scenarios where intermediary makes no money included
[b]Matched Pair Signed Rank test based on null hypothesis that punishment distributions are identical for direct and indirect action. Pairs matched by subject.
[c]Permutation p-values based on 200,000 simulations per test.
Pairs matched by subject. P-values are from two-tailed tests.

The strongest case in support of Result 1 is in the first row of Table 1: When the first mover keeps *all $10* for herself, she is punished less when she does so indirectly. When sold the DG for $10, the intermediary is not even making a decision; she had to pay $10, so she has to keep $10 in the DG. The first mover has effectively chosen the final outcome - ($10, $5, $0). All that is different is that the first mover is no longer interacting directly with the recipient; now, in between, the intermediary is pressing a button, having no choice other than to click on "Keep $10". More than any other pair of scenarios, the intention and responsibility of the first mover is transparently equivalent, yet punishment drops 18%.[14]

Intermediation also reduces the frequency of punishment. Comparing scenarios with identical outcomes (when the intermediary makes zero profit on top of her $5 endowment), the subjects are 9% less likely to punish overall when the intermediary is used (See Table 15 in the Appendix). This difference is significant at the 1% level using either a Wilcoxon signed rank test or a Fisher Pitman matched-pair permutation test. Moreover, there is a decrease in the frequency of nonzero punishments for every pre-punishment profit level of the first mover other than $5, and the decrease is statistically significant at at least the 5% level for three of these five profit levels (Also see Table 15 in the Appendix).

[14]This comparison also answers the hypothesis that punishment is decreasing in response to average overall behavior by the group - Player A is being selfish, but perhaps if Player B is acting nice, the average "niceness" of the group has increased, so I, as a punisher, may act nice as well. See Keser & Van Winden 2000, Fischbacher, Gächter & Fehr 2001, Frey & Meier 2004 for evidence of subjects' behavior being well-predicted by group behavior.

The drop in the frequency of punishment seems to account for most, but not all, of the decrease in the level of punishment. Table 16 in the Appendix runs the same non-parametric analysis as Table 1 but only includes matched pairs where both data are nonzero. The table shows little evidence of a substantial decrease in punishment due to intermediation, though the decrease remains when the first mover keeps $10 for herself (Also see Model I of Table 17 in the Appendix for an OLS regression suggesting the decrease remains after controlling for punishment frequency). Carpenter & Matthews (2009) suggest subjects often use a different set of norms or rules when making the decision whether to punish and how much to punish. In this game, intermediation affects both decisions.

Various regression specifications confirm result 1. Table 2 regresses punishment on monetary outcomes and paths taken ("Sold" is a dummy for whether the DG was sold to the intermediary). Every regression is a subject-level random effects model. According to the point estimate in the first row in Model I of Table 2, for every dollar the first mover keeps over $5 playing the Dictator Game herself, punishment increases 90 cents. The coefficient on the interaction term in the second row is the linear estimate of how much this punishment changes when she sells the DG to the intermediary: She is punished 26 cents less for every dollar she keeps by "selling" the DG, a 29% reduction.

Though many of the models in Table 2 impose functional forms, they have many nice features. First, they give an idea of the magnitudes. Second, they allow for inclusion of scenarios where the intermediary makes positive profits in the *Intermediation Game*, which is more difficult in pairwise nonparametric tests. This is important since punishers exhibit "outcome bias":[15] Punishment increases slightly, the more the intermediary keeps in the DG. This can be seen on the coefficient on "Intermediary's Profit" in Table 2. Though only a point estimate, the reported magnitude of the outcome bias, however, is quite small ($0.05), less than one fifth the expected decrease in punishment for each dollar made when intermediating ($0.26). Moreover, if the game is sold for a price near $10, there is little profit for the intermediary to make, and thus little opportunity for outcome bias to affect punishment.

These tests also speak to the behavioral predictions from Section 3. In section 3, we made predictions from leading classes of models for what punishments would be for scenarios which differ only in the path taken to the outcome. Table 1 makes just such a comparison. Recall, none of the predictions produced by the fairness models in Section 3 are consistent with punishment being greater for direct actions. However, Table 1 shows that subjects on average punish direct

[15]See Gino et al (2008) and Cushman et al (2009) for clean demonstrations of outcome bias

Table 2: *Intermediation Game*: Subject-Level Random Effects Regressions

	OLS	OLS	Ordered Probit[a,b]	Tobit[c]	Tobit[d]	Probit[b]
Dependent Variable	Punishment ($)	Punishment ($)	Punishment ($)	Punishment ($)	Punishment % of Total	Punish {0,1}
Scenarios Included	All	Intermediary Makes $0	All	All	All	All
	(I)	(II)	(III)	(IV)	(V)	(VI)
First Mover's Profit ($)	0.90***	0.90***	0.82***	1.51***	0.19***	0.87***
over $5	(0.04)	(0.05)	(0.04)	(0.07)	(0.01)	(0.08)
First Mover's Profit	-0.26***	-0.18***	-0.17***	-0.30***	-0.05***	-0.20**
over $5 *Sold	(0.05)	(0.07)	(0.05)	(0.08)	(0.02)	(0.03)
Intermediary's Profit	0.05*	–	0.07**	0.14***	0.03***	0.10**
	(0.03)		(0.03)	(0.05)	(0.01)	(0.04)
Sold Dummy	0.31**	0.16	0.15	0.21	0.03	-0.14
	(0.15)	(0.20)	(0.16)	(0.27)	(0.05)	(0.23)
Period	-0.28	-0.29	-0.10***	-0.36	-0.09	-0.23
	(0.24)	(0.26)	(0.03)	(0.52)	(0.12)	(0.34)
Constant	1.12**	1.14	–	-1.99	-0.13	-1.00
	(0.21)	(0.72)		(1.44)	(0.34)	(0.98)

64 subjects made punishment decisions for 27 scenarios. Model II includes only 12 of these scenarios.
***, **, * indicate p<0.01, 0.05, 0.1 respectively
[a]Only integer punishments (11 values) were chosen by subjects. [b]Marginal effects reported.
[c]Data censored below at $0. [d]Data censored below at 0% and above at 100%

actions more harshly (Table 14 in the appendix furthers this point through frequencies). Further, Model II in Table 2 re-runs the OLS specification in Model I, this time only including scenarios where the intermediary made $0 profit. Though these scenarios are included in Model I, imposing linearity on the relationship between the intermediary's profit and punishment when it is not a linear relationship may bias the other estimates in the model. Thus, Model II is a robustness check that the punishment decrease holds specifically for the scenarios highlighted in the predictions section. Note that the coefficient on the interaction term in the second row of Model II is also significantly less than 0; thus, even when the intermediary is *completely blameless*, her inclusion reduces punishment. No model discussed in Section 3 is consistent with this result.

Models III through V in Table 2 also estimate that intermediation significantly reduces punishment. Model III is an ordered probit regression of the data. Though punishment could be any whole cent amount, only whole dollar amounts were reported by subjects, so the data only take on eleven values. This makes linear fitting less plausible and suggests an ordered probit. Additionally, punishment was censored in the game. Subjects were not allowed to reward, so punishment was censored below at zero. Also recall, subjects could not reduce the first mover's payout to negative amounts, so it was censored above by the first mover's pre-punishment payout. These censoring issues are addressed in models III through V, using ordered probit and tobit regressions. Model IV is a tobit regression assuming the data are censored below at $0. In Model V, the dependent variable is now the ratio of punishment to the first mover's pre-punishment wealth. This allows for a constant upper limit to the data, 100%. Model V assumes a lower limit of 0% and an upper limit of 100%. The interaction term in every one of these regressions is significantly less than zero, consistent with the OLS regressions and the non-parametric analysis. Model VI is consistent with the non-parametric result that the probability of punishing is attenuated by intermediation. This model is a probit regression with a dependent variable that is an indicator of whether there was any punishment or not. The interaction term in this model, as well, is significantly less than zero (See Appendix Table 17 for alternative specifications including session random effects, subject-level and session-level clustering, and subject-level and session-level fixed effects models).

Result 2 - Blame is not just being shifted or diffused; it is merely decreasing.

In addition to considering scenarios where the intermediaries are *completely blameless*, we can further our case by looking at these scenarios when zero blame is *actually* assigned to the intermediaries. Recall in the *Two Punishments* treatment, both the first mover and the intermediary could be punished. Thus we can re-run our analyses controlling for whether punishment was shifted or diffused to the intermediary. Models II and III in Table 3 show OLS regression output for such scenarios. All specifications only include data from scenarios where the intermediary made zero profit. Model II controls for punishment-shifting at the subject level; it only includes data from punishers who *never* punished the intermediary when the intermediary made zero profit. Model III controls for punishment-shifting at the punishment level; it only includes punishments of the first mover if the punishment of the intermediary was $0. For both of these restricted samples, punishment still decreases when the intermediary is used: The coefficient on the first mover's profit interacted with a dummy for selling the game is significantly negative in both cases (Moreover, this result holds if we restrict the analysis to the first mover keeping $9 or $10; see section 8 in the Appendix).

This table simultaneously responds to concerns that perhaps subjects were confused, particularly by the framing of the game. In general, the term "sell" implies mutual consent for the transaction from the buyer and seller. It is very reasonable then to wonder if the subjects are thus assigning some blame to the intermediary simply because the word "sell" tells them she too was responsible. Table 2 shows that subjects who did not diffuse punishment, and thus were not displaying any such confusion, show similar punishment dynamics in the dimensions of interest (For a discussion of whether subjects' failure to include Player B's $5 endowment in their fairness judgments drives

Table 3: OLS: Two Punishments

Dep. Var. = Punishment ($)			
	Treatment = Two Punishments		
	All	Never Diffuse	No Diffusion
	(I)	(II)	(III)
	N=36	N=24	N=36
First Mover's Profit ($)	0.68***	0.67***	0.68***
over $5	(0.14)	(0.16)	(0.30)
First Mover's Profit	-0.19**	-0.21**	-0.30***
over $5 *Sold	(0.09)	(0.08)	(0.08)
Intermediary's Profit	0.01	0.00	-0.07
	(0.01)	(0.00)	(0.05)
Sold Dummy	0.32	0.26***	0.20
	(0.20)	(0.08)	(0.12)
Period	0.32**	0.26	0.17
	(0.15)	(0.19)	(0.16)
Constant	-1.00**	-1.05*	-0.67
	(0.39)	(0.52)	(0.40)

Standard Errors Clustered at Subject Level
***, **, * indicates p<0.01, 0.05, 0.1 respectively

the results, please see Appendix subsection 8).

Result 3 - The availability of intermediation makes the poorest player dramatically worse off and reduces total surplus.

When the first mover sells the DG, she is putting the DG in the hands of an unpunishable agent, who, on average, sends very little to the receiver. This shifts money from the receiver to the intermediary, from the poorest player to a player with the average wealth level. As Table 4 indicates, the receiver earns $1.63 on average in the *Intermediation Game* versus $4.50 in the *No Intermediary* treatment, 2.8 times more.[16] In the Third Party Punishment Game (similar to the *No Intermediary Treatment*), Fehr and Fischbacher (2004) conclude that punishment is successful in encouraging significantly more equitable outcomes. When an intermediary is added to the same game, this is far from being the case.

Not only is the availability of an intermediary bad for the poorest player, in this particular setting, it is Pareto-damaging. Even though punishment is less when the intermediary is used, it is still greater than zero (The sum of the first two coefficients in Table 2, $0.64, which is the amount of punishment per dollar kept when selling the DG, is significantly greater than $0). Since punishment is inefficient, this creates a loss in overall social welfare, assuming the researcher's budget is not a

[16] Average profits reported are for all periods, not just the randomly chosen period for which the subjects were paid.

part of the reference group for the social welfare calculation. Table 4 also shows that average profits decrease by 54 cents per subject compared to the *No Intermediary* profits. The *No Intermediary* treatment only has three players, so to make an overall earnings comparison, assume there is a fourth player in the *No Intermediary* treatment who earns $5 (akin to an unused intermediary). This brings the average earnings up only slightly, to $4.89, and total earnings to $19.55. Overall group earnings in the *Intermediation Game* was $17.24, over $2 less. This result is particularly surprising since $10 of group earnings is fixed (the endowments of the punisher and intermediary), so 24% of what *can* be lost is lost when an intermediary is present (which amounts to 12% of total possible surplus).

If first movers have rational expectations of punishments, the results suggest that, when an intermediary is present, the first movers are willing to inefficiently transfer money from the receiver to herself. In the *No Intermediary* treatment, the median first mover sends $5 to the receiver. In the *Intermediation Game*, the median first mover sells the game and sells it for $8. Keeping $8 instead of $5 transfers $3 from the receiver to the first mover, but it does so inefficiently since punishment increases.

Table 4: Welfare and Equality

	Intermediation Game	**No Intermediary Treatment**	**Rank Sum p-value**
Receiver's Avg. Payoff	$1.63	$4.50	<0.001
% Who Receive $5	22%	79%	
% Who Receive $0	55%	0%	
Avg. Profit (All Roles)	$4.31	$4.85	0.01

Result 4 - Even when the DG is unsold in the Intermediation Game, *the receiver is worse off than had there not been an intermediary.*

Dictators who do not to sell the DG keep more than dictators in the *No Intermediation* treatment; they only send $3.13, significantly less than the $4.50 sent in the *No Intermediary* treatment (Wilcoxon two-tailed rank sum p-value<0.01). This could be due to selection: Those who send the *most* in the *No Intermediary* treatment might be now selling the DG to the intermediary.[17] It could also be a changing norm: The same person will now choose to keep more because there is an intermediary present even if she does not use the intermediary. We cannot separate these stories

[17] This would imply that the subjects who would choose the most generous allocation directly - those most motivated by factors such as guilt aversion, altruism, or blame-avoidance - are now choosing to intermediate; thus, they believe intermediating attenuates these factors.

in this experiment though either is an implication of how intermediation changes the game that is worthy of future investigation.

Result 5 - Subjects believe intermediation makes the poorest player substantially poorer, and they believe others share this belief.

Subjects correctly predict the extreme inequity that intermediation produces. Recall beliefs of how much the intermediary kept in the DG were elicited in an incentivized manner after the game was played but before feedback was given. Beliefs of how much the first mover keeps in the DG were elicited in the *No Intermediary* treatment since this eliminates selection of who sells the game.

Table 5 shows that subjects expect the intermediary to keep at least 67% of the remaining value of the DG, leaving as little as \$0.05 and up to \$1.70 for the receiver. In stark contrast, subjects believe the first mover will send the receiver about \$4 when she is not allowed to sell the game (recall these beliefs, in the first column of Table 5, were elicited in the *No Intermediary* treatment to control for potential selection effects. For consistency, the empirical averages in the first column are also from the *No Intermediary* treatment). Moreover, 46 of 64 subjects report that the intermediary will keep more than she was made to pay for the DG for *every* price, *and* that others believe this as well (See Appendix, Table 19 for OLS regressions of these 46 subjects). Wilcoxon rank sum tests confirm subjects believe less will be sent to the receiver when the game is sold regardless of the price ($p<0.01$, two-tailed test comparing the unsold belief distribution to the of the distribution of beliefs for any given price). Moreover, as the second row of Table 5 shows, they believe other subjects hold similar beliefs ($p<0.01$, two-tailed test comparing the unsold belief distribution to the of the distribution of beliefs, also for any given price). Thus punishers believe using the intermediary reduces equity and makes the poorest player much worse off, they believe the first mover shares this belief, and yet they punish her less when she uses the intermediary. This is particularly striking because experimental subjects have been shown to have maximin preferences over payoffs (Charness & Rabin 2002). Punishing intermediation less runs opposite this preference.

Subjects believe the first mover believes intermediation reduces the minimum payoff (and equity). Thus, according to fairness models based on beliefs, intermediating should be deemed an antisocial action and punished *strictly* more harshly. Thus Result 3 makes the predictions of existing models fit even less well in this context.

If not beliefs, what is driving subjects' behavior? One hypothesis is that some subjects exhibit limited reasoning: Pinning responsibility on the first mover in the case of intermediation takes more reasoning ("The receiver is poor *because* the intermediary kept this much *because* the first

Table 5: Beliefs of How Much Will Be Kept in the DG

	Unsold[a]	Purchase Price				
		$5	**$6**	**$7**	**$8**	**$9**
Expected Amount Kept in $10 DG	$5.90	$8.37	$8.97	$9.30	$9.67	$9.85
Beliefs of Others' Expectations of Amount Kept	$6.10	$8.30	$8.90	$9.45	$9.75	$9.95
Average Actual Amount Kept	$5.50 N=24	NA N=0	$10 N=2	$9.43 N=7	$9.75 N=8	$9.83 N=6

[a]Beliefs and averages for Unsold column taken from *No Intermediary* treatment.

mover sold the game for this much," versus, "The receiver is poor *because* the first mover kept this much."). If some subjects do not reason through this one extra step of logic, this could drive Result 1. *The Reflection Treatment* was designed to test this hypothesis. Recall, subjects were given four neutral questions as primes for reflection before the game was played. 22 of the 24 subjects wrote something for all four questions. Interestingly, 15 also explicitly mentioned in at least one of their answers that the first mover will sell the game to avoid punishment and that selling the DG would likely lead to worse outcomes for the receiver (The tables refer to these 15 as "wise" subjects).

Result 6 - Limited Reasoning is not driving Result 1.

Table 20 in the Appendix shows average punishments for the "wise" subjects. Comparing scenarios with identical outcomes, the data indicate even for these 15 wise subjects, intermediation decreases punishment. Both a matched pair signed rank test and a Fisher Pitman matched-pair permutation test for these scenarios (including all profit levels of the first mover, which are balanced across selling) return p-values less than 0.01. The wise subjects show a decrease in punishment when the DG is sold in a linear approximation as well. Table 6 shows the OLS regressions for all subjects in *The Reflection Treatment* (Model I) using the same specification as in Table 2. Model II is the same specification ran only on the 15 subjects who explicitly mentioned the first mover's punishment-skirting intention. In both models, rents kept over $5 are punished less when the intermediary is used.

Thus even subjects who wrote down that first movers sell the game to try to avoid punishment and that selling is bad for the poorest player, punish first movers less when they sell the game. They exhibit an awareness of the dubious intentions of a first mover who sells as well as the consequences of their behavior, but this does not predict their punishment. Coupling this with Result 5 (subjects believe intermediation makes the poorest player substantially poorer), it has been shown that subjects who fully understand the poor outcomes and dubious intentions involved

Table 6: OLS Regressions: Reflection Treatment

Dep. Var. = Punishment ($)		
	All Subjects (I)	15 "Wise" Subjects (II)
First Mover's Profit ($) over $5	1.14***	1.09***
	(0.16)	(0.21)
First Mover's Profit over $5 *Sold	-0.33***	-0.41***
	(0.10)	(0.12)
Intermediary's Profit	0.09***	0.04
	(0.03)	(0.03)
Sold Dummy	0.10	0.39*
	(0.29)	(0.20)
Period	-0.42	-0.29
	(0.31)	(0.38)
Constant	1.31	1.25
	(0.97)	(1.31)

Standard Errors Clustered at Subject Level
***, **, * indicates p<0.01, 0.05, 0.1 respectively

in intermediation still punish it less; thus, limited rationality does not seem to explain Result 1.

Result 7 - The inclusion of a third, endowed player alone does not reduce punishment; this is not the mechanism through which intermediation reduces punishment.

Table 7 shows how subjects punished in *The Allow-Taking Game* based on whether the first mover decided to allow the intermediary to take any of the money sent to the receiver or not. If intermediation reduces punishment because intermediation includes a third party with an endowment, which, among other reasons, may change the reference group for fairness calculations, then we should note a reduction in punishment in *The Allow-Taking Game* when the intermediary is included.

Merely including an endowed third party does not reduce punishment. Table 7 shows that most subjects punish the first mover fairly equally whether she decides to allow taking or not. The data only show one possible significant difference in punishment, and the difference is in the opposite direction: The first mover is punished slightly more for allowing taking after she has kept $5 (in the last row of Table 7) (To see if we obtained different results between this and the *Intermediation Game* because either we switched from a giving frame to a taking frame or from a market transaction frame - selling the DG - to a transfer frame, see the design and results for *The Who's-The-Dictator Game*, Appendix Section 8.2). We conclude from this result that the mechanism driving the reduction in punishment by intermediation is not that intermediation includes an endowed third party. It must be another feature of intermediation. Potential reconciliations of all these results

Table 7: The Taking Game: Using a Third Party Does Not Reduce Punishment[a]

	Avg. Punishment		No. Subjects Who Punish		p-values	
Profit of First Mover	Taking Not Allowed	Taking Allowed	Not Allowing Taking More	Allow Taking More	Matched Pair, Signed Rank[b]	Permutation Test[c]
\$10	\$4.08	\$3.67	4	3	0.62	0.40
\$9	\$2.83	\$3.04	3	5	0.43	0.44
\$8	\$2.17	\$2.35	2	6	0.18	0.60
\$7	\$2.13	\$1.63	5	3	0.42	0.31
\$6	\$1.33	\$1.21	4	6	0.72	0.87
\$5	\$0.50	\$0.79	0	4	0.05	0.12

[a] To hold outcomes constant, only scenarios where intermediary makes no money included
[b] Matched Pair Signed Rank test based on null hypothesis that punishment distributions are identical for including and not including intermediary. Pairs matched by subject.
[c] Permutation p-values based on 200,000 simulations per test. P-values do not change at the hundredths level when re-run. Pairs matched by subject. P-values are from two-tailed tests.

are discussed in Section 7.

5 Study 2 - Intermediation Reduces Reward

A follow-up study was conducted with two main objectives - First, primarily to test the generalizability of Result 1 and second, to attempt to understand the mechanism driving Result 1. The second experiment was a modified *Intermediation Game* with charitable behavior (real donations to a real charity) and rewards rather than antisocial behavior and punishment.

Subjects were undergraduates at Harvard University. All were recruited via email in November 2008. Each dormitory was randomized into one of two experiments, either the *Donation Experiment* or the *Reward Experiment*, and sent a link for that experiment. The website they visited then randomized them into a treatment, so randomization within each experiment was at the individual level.[18]

Donation Experiment Design

Subjects in the first experiment were told that 3 names would be drawn for every 100 survey respondents. The first name drawn would win \$70 and have the opportunity to donate money to purchase mosquito nets for pregnant mothers in Busia, Kenya[19]. This opportunity was framed as either a

[18] Experiment was conducted using Qualtrics survey platform; see www.qualtrics.com
[19] Mosquito nets are very important in this region to protect against malaria.

direct gift to the pregnant mothers or as a donation to a charity (TamTam, www.tamtamafrica.org) who would then purchase nets for pregnant mothers (See Appendix, "First Person Donation Framing" for exact wording). It was made clear that in both cases, a donation of $3.50 would result in a pregnant mother in Busia receiving a mosquito net and that the money would be sent to TamTam, who would deliver the net. The framing manipulated the saliency of passing the donation through the intermediary. Subjects reported the number of nets they would like to donate, up to 20. Subjects were assured they could receive a forwarded email from TamTam if they chose to donate. This could ease any suspicions the money would not be sent while maintaining the feature that giving in this experiment is not to receive approbation from the receiver. The sender is anonymous, so her only motivations are altruism and self-signaling (e.g. Benabou & Tirole 2006 and Grossman 2009).

The second and third names drawn would play the two roles of a Dictator Game worth $48 with efficient giving. The amount sent was scaled up by a factor of three (to encourage more nonzero gifts). Everyone made both a mosquito net donation decision and a dictator decision. The dictator decision always followed their donation decision. Each question was on a separate screen, and at any point in the experiment, they did not know what would be on the next screen. They were not made aware of the second experiment.

Donation Experiment **Results**

See Table 21 for subject characteristics and evidence that the randomization provided two comparable samples. Subjects were 97% non-seniors, 69% women, and 59% participants from the student charity organization (PBHA). This is not representative of the undergraduate population, so there was sorting; however, even though this may be expected to affect the amount of giving, ex ante, we have no reason to expect this may interact with the treatment.

The framing manipulation had little to no effect on donations. The 94 subjects in the direct frame on average donated 10.7 nets while the 108 in the indirect frame donated 10.2. While their generosity is noteworthy (donating more than half of the $70 prize), the manipulation is not (Wlicoxon ranksum testing equal distributions: p=0.61). Though there was no ex ante hypothesis for this result, it is in consistent with the self-serving bias literature. Performing an act with an intermediary may result in ambiguity of who is responsible.[20] Subjects will resolve this ambiguity in a self-serving way; thus, if responsibility is good, as it is here, they will take full credit, and we should not see any difference between the two conditions.

[20] Hamman et al 2008 show that, indeed, subjects are more likely to undertake *unethical* endeavors with an intermediary, in such cases resolving the ambiguity by convincing themselves the intermediary is responsible.

Subjects who donated at least one net directly gave directionally, but not significantly, more in the subsequent DG than subjects who donated at least one net indirectly: \$9.70 vs \$7.85 (ranksum p-value=0.22). This question was added primarily to elicit beliefs in an incentivized way in the second survey; there was no ex ante hypothesis of a difference in altruism between who may give in the two conditions.

***Reward Experiment* Design**

Subjects in this experiment were randomized into either the *reward* treatment or the *DG Guess* treatment. In either experiment, they were then told they would be randomly matched with one subject from Survey 1. They were told one piece of information about this person. They were (correctly) told either:

> *Direct Framing:* "This respondent was asked if he/she won \$70, would he/she like to purchase mosquito nets (at \$3.50 each) for pregnant women in Busia, Kenya (through TamTam `www.tamtamafrica.org`). This respondent said yes - Had his/her name been drawn he/she would have purchased mosquito nets for pregnant women in Busia, Kenya."
>
> OR
>
> *Indirect Framing:* "This respondent was asked if he/she won \$70, would he/she like to donate money to Tam Tam (www.tamtamafrica.org). Money donated to TamTam enables them to purchase mosquito nets (at \$3.50 each) for pregnant women in Busia, Kenya. This respondent said yes - Had his/her name been drawn he/she would have donated money to TamTam."

Subjects in the *reward* experiment then played a dictator game worth \$84, with efficient giving - so that for each \$1 sent, the other participant received \$3. In the DG, the recipient is the anonymous Survey 1 respondent about whom they just learned. They were told the Survey 1 respondent would receive a letter, along with any money sent, explaining that another respondent was given the opportunity to send them money based on their decision to donate nets. Thus, this was framed as an unexpected reward. They received the letter even in the case of zero money sent.

Subjects in the *DG Guess* experiment did not reward but rather were asked to guess how much the anonymous Survey 1 respondent sent in the DG she played following her donation decision.

They were paid $50 if they were within $5 of how much the respondent actually sent[21].

Reward Experiment Results

Subjects rewarded direct-framed giving more than indirect-framed giving. Subjects sent $36.4 to "direct" donors (thus they received $109.2), while subjects sent "indirect" donors an average of $29 (thus they received $87). A Wilcoxon rank sum test of a hypothesis that these reward distributions are equal gives marginal significance (p=0.099). The cumulative distributions of rewards shown in figure 2 further show the difference in rewards. Rewards to the direct donor appear to first order stochastically dominate rewards to the indirect donor, and a Kolmogorov-Smirnov test of minimum distance marginally rejects equal probability distributions with a p=0.09.

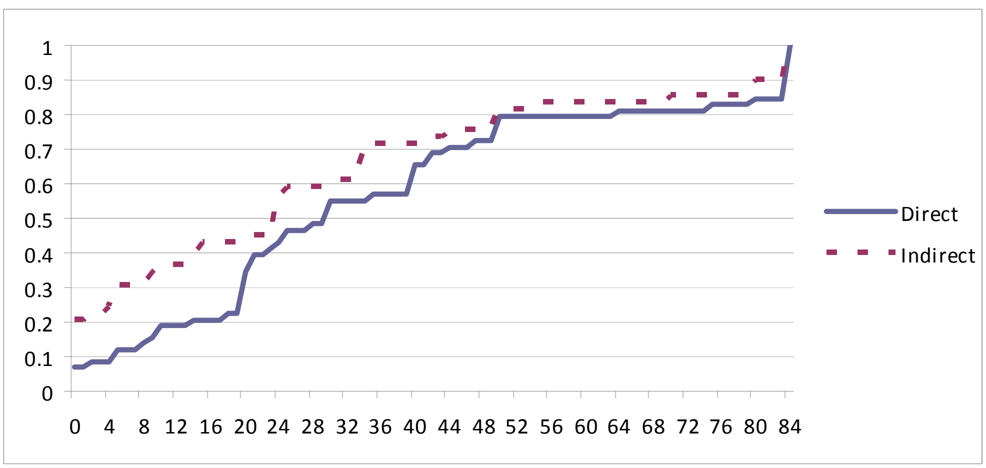

Figure 2: CDF's of Rewards to Donors, by Framing Manipulation.

This is preliminary evidence that the decision process driving Result 1 from *The Intermediation Game* may be generalizable. We have changed the behavior from selfish to charitable, the first mover's motivation from intrinsic and extrinsic to intrinsic (the first mover knew of the punisher in *The Intermediation Game*, but not of the rewarder here), the environment from the lab to online, from anonymous, undergraduate subject receivers to real charities and charitable cases, and from endogenous, chosen intermediation to exogenously imposed intermediation. In this very different environment, a similar result develops: Intermediation reduces reward as it did punishment.

[21]This elicits their belief of the midpoint of the $10 interval containing the most Survey 1 donors in their distribution of beliefs. Beliefs were elicited this way in order to keep it simple and understandable for the subject.

Finally, subjects believe subjects who donated nets *indirectly* donated *more* in the subsequent DG. They guessed indirect donors sent $14.1 compared to $8.5 by direct donors. A Wilcoxon rank sum reports these distributions are significantly different, p=0.04. If the DG decision in Survey 1 is believed by subjects in Survey 2 to be independent of the decision to donate (it always came after), then this is a measure of baseline altruism. This question was added to test a hypothesis that Result 1 was being driven not by "punishing bad behavior" but rather "punishing bad people". As Levine (1998) argues, we might have more preference for the payouts of altruistic agents. This hypothesis is not supported in Survey 2; rewards are higher for the group with less expected altruism (direct donors).

Admittedly, the DG decision in Survey 1 may very well not be independent. There might be wealth effects, though wealth is only in expectation. Additionally, often after performing a good act, we feel licensed to be less moral in the next period (e.g. Cain, Loewenstein & Moore 2005). Whether subjects predict such inter-question dependencies, or others, is unknown to the author. In any case, this result should only be regarded with these considerations.

6 Related Literature

6.1 The Economics Literature: Punishment and Fairness

The literature has shown punishment can effectively regulate individuals (Fehr & Fischbacher 2004, Fehr & Gächter 2000) as well as companies (Dyck & Zingales 2004, Martin & Thomas 2005). Theoretically, punishment increases pro-social behavior, which means that punishment can lead to greater social welfare for the group (Axelrod 1986).[22] These encouraging results are supported by models of punishment. Theories of *how* we punish are typically a reflection of theories of *why* we punish: If we punish to reinforce pro-social behavior, punishment should be a response to, and only to, anti-social behavior. Hence most models of punishment and fairness are models identifying pro-sociality of behavior. They do this through factors such as outcomes, intentions, and procedures. Fehr & Schmidt (1999) and Bolton & Ockenfels (2000) both provide models of outcome-based fairness. Fehr & Schmidt (1999) model this in a two-player framework, where both players have a preference for equitable outcomes, where advantageous inequity comes at less of a utility cost. Bolton & Ockenfels (2000) argue that players have a preference for their own absolute payoff as well as how their payoff compares to the group average. Even though punishment obeys the law of

[22]Similarly, punishment can be sufficient for collusion amongst oligopolists (Stigler 1964) even with imperfect monitoring of each others' price and quantity decisions (Green & Porter 1984; Abreu, Pearce & Stacchetti 1986).

demand (Anderson & Putterman 2006), subjects in Falk, Fehr & Fischbacher's (2005) experiment punish defectors more than cooperators even when it is more costly to do so. This suggests behavior, not just outcomes, drives reciprocity. Rabin (1993) and Dufwenberg & Kirchsteiger (2004) define fairness in terms of intentions. Intention is comprised both of strategy space - how good is your action relative to your alternatives? - and your beliefs - given what you think I am going to do, how nice are you being? Falk & Fischbacher (2004) combine intentions and fairness in a theoretical framework that Falk, Fehr & Fischbacher (2008) provide experimental evidence works best among these three classes of models in predicting behavior in simple, laboratory experiments. Charness & Rabin (2002) provide experimental evidence (and a general model in the appendix) that subjects' fairness judgments and reciprocity decisions are largely dependent on the welfare of the poorest player in the reference group. Bolton, Brandts and Ockenfels (2005) provide experimental evidence that subjects' perceptions of fairness also depend on ex ante equity, a concept they term "procedural fairness". Our experiment did not speak to procedural fairness, but it was designed to cleanly test outcomes, intentions and the combination. We found that when pitted directly against "directness", these concepts of fairness made poor predictions. The conclusion is not that outcomes and intentions do not matter, but rather that directness does, and it seems to be first order.

Many experiments have investigated how subjects perceive fairness when multiple agents have acted. The main finding is that responsibility can be diffused when a second party is present[23]. Charness (2000), Fershtman & Gneezy (2001), and Bartling & Fischbacher (2008) all demonstrate experimentally that the addition of a second responsible party may alleviate perceived responsibility of the first party. Fershtman & Gneezy show this is true even though the first mover is incentivizing the second party to act selfishly on her behalf and even when she has the choice of not using the second party at all. The authors provide the insight that perhaps the behavior they observed is "because it is not [done] directly by the proposer but by a third party," though they do not provide experimental support for this specific hypothesis. Rather, they show the subjects care about the second party's payout; the second party is a 'hostage' in the negotiation. Bartling & Fischbacher show similar abated reciprocity even though the first mover is delegating the choice to a second party who has incentive to act selfishly on behalf of both of them. Moreover, Hamman, Loewenstein & Weber (2008) show guilt might also decrease in such situations. Experimental subjects demand an agent take more for them in a DG than they are willing to take for themselves (Since assignment

[23]This paper follows the custom of assuming punishment, blame, responsibility and unfairness are positively correlated. As Falk, Fehr & Fischbacher (2005) conclude, "the desire to harm those who committed unfair acts, seems to be the most important motive behind fairness-driven sanctions." It must be noted, however, that though these are very similar, they are not identical. Cushman (2008) shows punishment is better predicted by outcomes while judgments without consequence are better predicted by the intentions of the actor.

is random, this also means subjects are willing to take more as an agent for another than they are for themselves). The experiment presented here is designed to allow for analysis of scenarios where the intermediary is completely blameless, and completely unpunished, for the outcome. Even in such scenarios, punishments decrease for actions done through an intermediary. This result does not refute responsibility diffusion. Instead, it identifies a separate force acting in tandem, strengthening the claims that the frequency of bad actions may increase when more people are involved, and that one may want to involve more people when taking a bad action.

A few recent experiments have shown that from a first person perspective, direct interaction may be important in fairness calculations. Dana et al (2006) show a significant portion of subjects would prefer to take $9 than have their decision from a $10 dictator game implemented. When they take the $9, the recipient does not receive anything, so this decision is at least weakly dominated by two DG allocations, ($9, $1) and ($10, $0). The subjects seem to not to want to feel responsible for making the decision. Similarly, Lazear et al (2009) show that most subjects would prefer to avoid the opportunity to share. Dana et al (2007) show that many dictators do not costlessly reveal the payout of their recipient, perhaps avoiding having to make a difficult or selfish decision. DellaVigna et al (2009) show similar avoidance behavior in the field. When a charity announces they will be coming around the next day for donations, 10-25% fewer households answer the door compared to unannounced visits. The results in our paper are psychologically similar, though from a third person perspective. In all of the studies as well as ours, an agent is faced with the decision {Play, Don't Play} and a sharing decision if she chooses "Play". Subjects in these experiments behave as if choosing "Don't Play" is okay even if that means not sharing anything. In our study, selling the DG is similar to choosing "Don't Play", and they are judged less harshly for doing so.

6.2 Moral Psychology

The Moral Psychology literature has shown a much less rational side of punishment. Much of these results have roots found in the workhorse of the Moral Psychology literature - the "trolley problem". This was originally a thought experiment constructed by Philippa Foot (1978), but has since been widely used in lab experiments. A script of the trolley scenario usually reads something like this:

> *Suppose a runaway trolley is about to run over and kill five people. Suppose further that you can hit a switch that will divert the trolley onto a different set of tracks where it will kill only one person instead of five. Is it okay to hit the switch? Now, what if the only way to save the five people were to push a large person (larger than yourself)*

in front of the trolley, killing him but saving the others? Would that be okay? (Green & Haidt 2002)

Study participants typically say that pulling the lever is okay but pushing the large person is not. Less the size of one man, the tradeoffs are identical, but in the eyes of a moral judge, the two situations are quite different.

Using this scenario and others, the Moral Psychology literature has identified many ways how and why moral judgment differs from utilitarian standards. Frederick & Kahneman 2002 provide evidence that punishment and moral judgment are not merely responses to the objective wrongness of an action. Moral preferences, rather, are at least partially derived from an intuitive or emotional reaction (See Greene & Haidt 2002 for a neuroimaging survey, Haidt 2001 on moral dumbfounding, Pillutla & Murnighan 1996 on anger in the ultimatum game, Valdesolo & DeSteno 2006 for exogenous variation in emotion and moral judgments, and Wheatley & Haidt 2005 on hypnotic disgust and moral judgment); in fact, subjects often are unable to explain why they made the moral judgment that they did (Cushman et al 2006). This has largely been explained by a dual process model of the brain, the emotional and the reasoning (Greene 2007). Greene et al (2001) show regions of the brain associated with emotion are more active when the personalness of the proposed scenario is increased, and Ciamarelli et al (2007) show experimental subjects with a damaged ventromedial prefrontal cortex, a brain region associated with emotion, report much more utilitarian moral judgments than control participants.

The Moral Psychology literature also speaks more specifically to the question of directness and morality. Much research has provided evidence suggesting that blame increases, the more directly the consequence follows from the action. Going back to the trolley scenario, it has since been shown that pulling a trap door from underneath the large man may be okay, but pushing him with a pole over the edge is not (Greene et al 2008a). Using your "personal force" to directly push him over is wrong, but setting about a chain of events that brings about his demise seems okay. Subjects also claim they would be more willing to undertake actions whose bad outcomes are side effects rather than means to an end (Royzman & Baron 2002). Paharia et al (2009) also show that subjects who read hypothetical scenarios rate firms as less immoral if the firms act selfishly when outsourcing. Interestingly, they find the effect diminishes if they read the two scenarios - when the firm outsources and when they do not - side by side. We add to this literature in three ways. First, we are able to investigate equlibrium behavior. Since the subjects are playing a game, they are responding to the beliefs and strategies of others. Second, in this environment, we can design clean predictions for existing theories of fairness and understand how they may be improved.

Third, subjects are incentivized. This does not always have an effect in experiments, but Camerer & Hogarth (1999) suggest they may affect subject's incentive to "present" themselves flatteringly. In a context of selfishness and punishment, these effects may be a concern.

Another interesting finding related to directness has been dubbed "omission bias". Spranca et al (1991) show that subjects judge an act of omission to be less reprehensible than an act of omission with the same intentions, motives and consequences. Like the subjects in economic experiments who choose to avoid making a sharing decision, these participants seem to judge a person for the actions she takes, not the ones she does not. This is consistent with our finding that subjects are punished less when they choose not to directly interact with the poorest player.

7 Concluding Remarks

This paper investigates how an action is punished when performed through an intermediary. We employ a simple game, which allows one player (the first mover) the option of undertaking an anti-social action (keeping money at the expense of the poorest player) and the option of whether to do this directly or through an intermediary. An unaffected party is told what happened and has the opportunity to punish the first mover. Using the intermediary is much worse for the poorest player (and equity), and subjects expect this. Nevertheless, subjects punish the first mover less when she elects to keep money via intermediation. This is true even if the intermediary is unambiguously free of responsibility. We then perform a treatment that encourages subjects to think and write about the game and the strategies of the other players before they play. 15 of 24 write down that the first mover will use the intermediary to avoid punishment and that intermediation is worse for the poorest player. When these 15 subjects subsequently play the game, they punish intermediation less. Thus even when subjects understand the poor outcomes intermediation produces or the punishment-skirting intentions of the first mover, they punish in a manner suggesting they believe using the intermediary actually is less punishable. We further show the punishment reduction is not due to the mere inclusion of a third player. Across treatments, punishment decreases if and only if including the intermediary allows the first mover to avoid directly interacting with the poorest player. In a framed field study, we show a similar effect with rewards and charitable action. These reductions in punishment and reward due to directness is interesting for two reasons: First, they cannot be explained by any current model or literature in Economics. Second, intermediation is widespread, and perhaps our moral intuitions and judgments in the case of individual or corporate partnerships should be questioned.

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8 Appendix

Table 8: Average Punishment for Each Scenario

Scenario	Sold?	Price	Amt. Kept	Avg. Punishment
1	No	–	$5	0.69
				(0.21)
2	No	–	$6	1.30
				(0.25)
3	No	–	$7	2.03
				(0.28)
4	No	–	$8	2.89
				(0.33)
5	No	–	$9	4.00
				(0.39)
6	No	–	$10	5.22
				(0.48)
7	Yes	$5	$5	0.78
				(0.22)
8	Yes	$5	$6	0.89
				(0.22)
9	Yes	$5	$7	1.06
				(0.24)
10	Yes	$5	$8	1.03
				(0.23)
11	Yes	$5	$9	1.03
				(0.23)
12	Yes	$5	$10	1.27
				(0.26)
13	Yes	$6	$6	1.19
				(0.26)
14	Yes	$6	$7	1.36
				(0.26)
15	Yes	$6	$8	1.45
				(0.27)
16	Yes	$6	$9	1.59
				(0.29)
17	Yes	$6	$10	1.44

Continued on next page

Table 8 – continued from previous page

Scenario	Sold?	Price	Amt. Kept	Avg. Punishment
				(0.28)
18	Yes	$7	$7	2.02
				(0.33)
19	Yes	$7	$8	2.05
				(0.31)
20	Yes	$7	$9	1.94
				(0.32)
21	Yes	$7	$10	1.91
				(0.32)
22	Yes	$8	$8	2.50
				(0.35)
23	Yes	$8	$9	2.38
				(0.36)
24	Yes	$8	$10	2.56
				(0.36)
25	Yes	$9	$9	3.59
				(0.43)
26	Yes	$9	$10	3.45
				(0.42)
27	Yes	$10	$10	4.30

Standard errors reported, clustered at subject level

Every average punishment statistically greater than zero, with p<0.01

Table 9: Summary Statistics

	Intermediation Game	**No Intermediary Treatment**
No. Subjects	64	24
No. Who Use Intermediary	33	-
Avg. Sell Price	$7.17	-
Avg. Amount Kept in $10 DG	$8.37	$5.50
(when sold)	$9.79	-
(when unsold)	$6.87	-
% Who Punish At Least Once	81%	100%
Avg. Punishment	$2.07	$3.65
Avg. Nonzero Punishment	$4.24	$4.70
Avg. Punishment as % of first mover's pre-punishment wealth	28%	44%
Avg. Nonzero Punishment as % of first mover's pre-punishment wealth	58%	56%
Avg. Proft (All Subjects)	$4.31	$4.83

Table 10: What Punishment Amounts Were Common?

	All	Direct	Indirect
No. Reported Punishments	1,728	384	1,344
Punishment = $0	884	157	727
Punish Everything (to $0)	253	57	196
Equate w/ Punisher (to $5)	188	64	124
Equate w/ Receiver	129	44	85
Equate w/ Intermediary	92	NA	92

When a punishment qualifies for more than one row, it is only included in the first row for which it qualifies.

A - *The Who's-the-Dictator Game* - Design

This game is a reframing of *The Intermediation Game* (and hence also *The Allow-Taking Game*) designed to test two potential explanations why intermediation may reduce punishment in *The Intermediation Game*. First is the hypothesis that selling the DG to the intermediary is less reprehensible because it is framed as a market transaction (since the term "sell" is used). Second is the hypothesis that *The Allow-Taking Game* finds no difference in punishment because the act of taking is more outrageous than keeping, and changing this frame may change the punishment imposed. In this game, the first mover first decides who starts with the $10, herself or the receiver. If she starts with the $10, then she plays a $10 DG with the receiver. If the receiver starts with the $10, then the intermediary decides how much of the $10 to take. First though, the first mover also decides how much the intermediary must take and pass to her. The intermediary must take at least this much from the receiver's $10. Anything she takes on top of this amount is the intermediary's profit for the game. After either process, the punisher can reduce the first mover's payout. The punishment technology and elicitation is identical to those in *The Intermediation Game*.

One session of 24 subjects was run in October, 2009 at the Computer Lab for Experimental Research at Harvard Business School. The session last for one hour.

B - *The Who's-the-Dictator Game* - Results

The data are fewer and noisier than those provided by *The Intermediation Game*, but there is evidence that intermediation reduces punishment. As in *The Intermediation Game*, this holds when misbehavior, and punishment, is high. Table 11 shows that when the first mover makes $10, she is punished less when the intermediary passes her the money than if she took it herself. The results for $9 are slightly mixed: The p-value from a matched pair signed rank test is 0.2 while the p-value from a Fisher-Pitman permuatation test of the data is 0.07. This indicates that, if there is an effect, it may be driven by few, large differences rather than many, small differences. The effect intermediation has on punishment seems to be smaller, or at least noisier, in this game compared to *The Intermediation Game*. This could be due to, among other reasons, subject pool differences, different sample sizes, and one of the two hypotheses proposed in the design description of this game. Though speculative, framing the use of an intermediary as a market transaction, or switching from a giving frame to a taking frame, may reduce punishment as well.

Table 11: Directness is Punished More Harshly in *Who's-the-DG*[a]

	Avg. Punishment		No. Subjects Who Punish		p-values	
Profit of First Mover	DG Unsold	DG Sold	Unsold More	Sold More	Matched Pair, Signed Rank[b]	Permutation Test[c]
$10	$7.67	$6.38	6	1	0.05	0.03
$9	$6.63	$5.58	7	4	0.20	0.07
$8	$4.13	$4.17	5	5	0.91	1.00
$7	$3.08	$3.13	5	7	0.66	1.00
$6	$1.92	$2.00	5	6	0.83	0.89
$5	$1.25	$0.96	3	1	0.31	0.38

[a]To hold outcomes constant, only scenarios where intermediary makes no money included
[b]Matched Pair Signed Rank test based on null hypothesis that punishment distributions are identical for direct and indirect action. Pairs matched by subject.
[c]Permutation p-values based on 200,000 simulations per test. P-values do not change at the hundredths level when re-run. Pairs matched by subject. P-values are from two-tailed tests.

C - Order Effects and Within-Subject Consistency

The subjects played the game for four periods, in all four roles. This begs two questions. First, does the order of roles played affected decisions as a punisher? Second, do subjects show behavioral correlations across roles? We find no evidence of order effects with respect to the main result, but we do find within-subject correlations.

Role order does not affect how subjects punished indirectness. Table 12 includes three dummy variables (Played as A, B or C) which equal 1 if at the time the subject was a punisher, she had already played the game in the role specified[24]. This specification shows that how much punishment decreases when the game is sold is not affected by these order effects. The coefficients of the three-way interactions in the table identify how much punishment decreases when the DG is sold for punishers who played the game in each of the other roles first. None of these coefficients are statistically different than zero indicating that the punishment decrease is not impacted by the order of roles the subject experienced. Another way to read the table, is that in Period 1, when the punisher is untainted by experience, there is still a significant decrease in punishment for money made indirectly. This is evidenced in the statistically significant coefficient of the interaction of first mover's profit and whether she sold the DG.

Table 13 shows evidence of some behavioral consistency across roles. This table runs OLS regressions interacting the two main drivers of punishment - 'Player A's profit above $5' and 'Player A's profit when the game was sold' - with (i) whether the punisher sold the DG when she was player A and separately with (ii) how much profit above $5 she claimed for herself as Player A. Model I includes the data from all players. The first three rows show, respectively, punishment increases as Player A's profit increases, this is not different for punishers who sold the DG, but it is less for punishers who claimed (or will claim) more money when they were Player A. That is, they do not punishing selfishness as harshly if they were (or are going to be) selfish themselves. The next three rows, respectively, show that punishment of profit made decreases if made by selling (Result 1 in the paper), this decrease is much greater for punishers who sold the game themselves, and it is no different for punishers who made (or will make) a lot of money as Player A. In other words, subjects who take (or will take) the indirect action, punish indirectness less harshly.

[24]Subjects all played the game in the order $D \rightarrow C \rightarrow B \rightarrow A \rightarrow D$. Thus we do not have any data for subjects who only played as C or B before they punished; they always played as A as well.

Table 12: OLS Regressions: Order Effects

Dep. Var. = Punishment ($)	
First Mover's Profit ($) over $5	0.90*** (0.09)
First Mover's Profit over $5 *Sold	-0.21*** (0.03)
First Mover's Profit over $5 *Sold Played as A first	-0.07 (0.16)
First Mover's Profit over $5 *Sold Played as B first	0.01 (0.17)
First Mover's Profit over $5 *Sold Played as C first	0.00 (0.14)
Intermediary's Profit	0.05** (0.02)
Sold Dummy	0.31*** (0.08)
Played as A first	-0.77 (0.72)
Played as B first	-0.04 (0.57)
Played as C first	-0.02 (0.56)
Constant	1.73** (0.77)
Session F.E.	Included

Standard Errors Clustered at Subject Level
***, **, * indicates p<0.01, 0.05, 0.1 respectively

D - Do Subjects Not Account for the Intermediary's Endowment?

There is reasonable concern that the punishers do not account for the intermediary's $5 endowment when making their fairness judgments. If the punisher thinks both the intermediary and the receiver have $0, then "selling" the DG can be construed as a pro-social move, granting a poor player a chance to receive some wealth. This would drive similar punishment patterns as we observe. Thus let us consider only scenarios where the first mover does not leave enough for money to be shared amongst the other two players. If the first mover chooses to make $9 or $10, then there is $1 or $0 left over. Since dollars are indivisible in this game, selling the game for $9 or keeping $9 in the DG should be identical: The first mover has chosen a final allocation of ($9, $1, $0) or ($9, $0, $1). To keep the outcome constant, let us only consider scenarios where the intermediary passes the dollar to the receiver, so the final allocation is ($9, $0, $1). We shall also restrict the analysis to punishers who do not (mistakenly) shift any blame to the intermediary in any of these scenarios. That is, we only include punishers who do not punish the intermediary in the four scenarios we have isolated

Table 13: OLS Regressions: Behavioral Consistency

Dep. Var. = Punishment ($)			
	All (I)	Sold as A (II)	Didn't Sell as A (III)
First Mover's Profit ($) over $5	1.62*** (0.38)	1.71** (0.67)	1.64*** (0.49)
First Mover's Profit over $5 *Sold DG as Player A	0.05 (0.18)	–	–
First Mover's Profit over $5 *Profit made as A	-0.10* (0.05)	-0.11 (0.08)	-0.09 (0.07)
First Mover's Profit over $5 *Sold	-0.39* (0.23)	-0.57 (0.53)	-0.42* (0.24)
First Mover's Profit over $5 *Sold *Sold DG as Player A	-0.16* (0.09)	–	–
First Mover's Profit over $5 *Sold *Profit made as A	0.03 (0.03)	0.04 (0.07)	0.02 (0.03)
Intermediary's Profit	0.05** (0.02)	0.04** (0.02)	0.07 (0.05)
Sold Dummy	0.31*** (0.08)	0.15*** (0.06)	0.48*** (0.14)
Sold DG as Player A	-1.06 (0.49)	–	–
Profit made as A	0.37** (0.15)	0.41* (0.23)	0.29 (0.22)
Period	-0.20 (0.25)	-0.06 (0.31)	-0.30 (0.38)
Constant	-0.88 (1.46)	0.13 (3.31)	-3.07 (2.88)
Session F.E.	Included	Included	Included
Observations	64	33	31

Standard Errors Clustered at Subject Level
***, **, * indicates p<0.01, 0.05, 0.1 respectively

- Sell for $10, Don't Sell and Keep $10, Sell for $9 and intermediary passes the $1, and Don't Sell and Keep $9.

Restricting our analysis thusly does not change the punishment comparisons. A two-tailed, signed rank test rejects the hypothesis that the punishments are the same whether the first mover sells or not with a p=0.02. Similarly, a Fisher Pitman matched pair permutation test rejects the same hypothesis at the same level (p=0.02).

E - Charity Experiment Framing

First Person Donation Framing

Direct Framing: We would like to offer an opportunity to do something great with a portion of your winnings. You can purchase a mosquito net for a pregnant mother in Busia, Kenya (via TamTam Africa, www.tamtamafrica.org). We will match you dollar for dollar in purchasing nets. So even though they usually cost $7, for $3.50 you can purchase 1 net, for $7, two nets, etc. Since you have won $70, you may purchase anywhere between 0 and 20 nets to be given to pregnant women in Kenya (via TamTam Africa).

Indirect Framing: We would like to offer an opportunity to do something great with a portion of your winnings. You can donate money to a charity, TamTam Africa (www.tamtamafrica.org), enabling them to buy mosquito nets for pregnant mothers in Busia, Kenya. We will match you dollar for dollar in your donation. So even though it usually takes a donation of $7 for TamTam to be able to purchase a net, for a donation of $3.50 from you, then can purchase 1 net, for $7, two nets, etc. Since you have won $70, you may make a donation that helps them purchase anywhere between 0 and 20 nets to be given to pregnant women in Kenya.

F - Additional Tables and Figures

Table 14: How Many Subjects Punish Directness More Harshly†

No. Subjects Who...	
Punish Direct Action Harsher More Frequently	28
Punish Direct Action Harsher At Least Once	36
Never Punish Indirect Action Harsher	46
Total No. Subjects	64

Table 15: Directness is Punished More Frequently[a]

	% Who Punish		# Subjects Who Punish		p-values	
Profit of First Mover	DG Unsold	DG Sold	Unsold, But Not Sold	Sold, But Not Unsold	Matched Pair, Signed Rank[b]	Permutation Test[c]
All	59%	50%	41	8	<0.01	<0.01
$10	75%	67%	8	3	0.13	0.23
$9	77%	67%	6	0	0.01	0.03
$8	75%	63%	9	1	0.01	0.02
$7	66%	50%	11	1	<0.01	0.01
$6	45%	38%	6	1	0.06	0.12
$5	17%	19%	1	2	0.56	1.00

[a]To hold outcomes constant, only scenarios where intermediary makes no money included
[b]Matched Pair Signed Rank test based on null hypothesis that punishment distributions are identical for direct and indirect action. Pairs matched by subject.
[c]Permutation p-values based on 200,000 simulations per test.
Pairs matched by subject. P-values are from two-tailed tests.

Table 16: Is Directness Punished More Harshly? Controlling for Frequency[a,b]

	Avg. Punishment		# Subjects Who Punish		p-values	
Profit of First Mover	DG Unsold	DG Sold	Unsold More	Sold More	Matched Pair, Signed Rank[c]	Permutation Test[d]
$10	$7.20	$6.58	16	4	0.01	0.07
$9	$5.44	$5.35	9	6	0.46	0.76
$8	$4.21	$3.95	11	7	0.30	0.30
$7	$3.55	$4.10	5	11	0.09	0.06
$6	$3.22	$3.22	5	5	1.00	1.00
$5	$4.30	$4.30	0	0	1.00	1.00

[a]To hold outcomes constant, only scenarios where intermediary makes no money included
[b]To test if punishment is affected by intermediation beyond the frequency of punishing, only matched pairs with two nonzero punishments are included.
[c]Matched Pair Signed Rank test based on null hypothesis that punishment distributions are identical for direct and indirect action. Pairs matched by subject.
[d]Permutation p-values based on 200,000 simulations per test.
Pairs matched by subject. P-values are from two-tailed tests.

Table 17: Additional Regressions: *The Intermediation Game*

	Subject-Level Clustering OLS	Session-Level Clustering OLS	Session R.E. OLS	Subject F.E. OLS	Session F.E. OLS
Dependent Variable	Punishment ($)	Punishment ($)	Punishment ($)	Punishment ($)	Punishment ($)
	(I)	(II)	(III)	(IV)	(V)
First Mover's Profit ($)	0.90***	0.90***	0.90***	0.90***	0.90***
over $5	(0.09)	(0.09)	(0.07)	(0.04)	(0.07)
First Mover's Profit	-0.26***	-0.26**	-0.26***	-0.26**	-0.26***
over $5 *Sold	(0.06)	(0.04)	(0.09)	(0.05)	(0.09)
Intermediary's Profit	0.05	0.05	0.05	0.05*	0.05
	(0.02)	(0.03)	(0.05)	(0.03)	(0.05)
Sold Dummy	0.31***	0.31*	0.31	0.31**	0.31
	(0.08)	(0.11)	(0.28)	(0.15)	(0.28)
Period	-0.28	-0.28	-0.28***	–	-0.28***
	(0.25)	(0.23)	(0.05)		(0.05)
Constant	1.12**	1.12	1.08***	0.43***	1.12***
	(0.70)	(0.49)	(0.39)	(0.12)	(0.26)

***, **, * indicates p<0.01, 0.05, 0.1 respectively

Table 18: Expected Payouts in *The Intermediation Game*

Pre-punishment Wealth	Exp. Payout Unsold	Exp. Payout Sold	Ranksum p-value[a]	Permutation p-value[a]
$10	$4.78	$5.70	<0.01	0.01
$9	$5.00	$5.41	0.05	0.07
$8	$5.11	$5.44	0.07	0.06
$7	$4.97	$4.96	0.61	1.00
$6	$4.70	$4.41	0.48	0.13
$5	$4.31	$3.73	<0.01	<0.01

Expected Payouts in "sold" column are "worst case" scenarios -
They assume the intermediary will take the action that maximizes expected punishment.
[a]Both tests are two-tailed. Permutation p based on 200,000 permutations.

Table 19: OLS Regressions

	Subjects Who Expect Intermediation to Hurt Equity In *The Intermediation Game* N=46	
	Dep. Var. = Punishment ($)	
	All Scenarios (I)	Intermediary Makes $0 (II)
First Mover's Profit ($) over $5	0.91*** (0.11)	0.91*** (0.11)
First Mover's Profit over $5 *Sold	-0.24*** (0.05)	-0.14** (0.05)
Intermediary's Profit	0.03** (0.02)	–
Sold Dummy	0.26*** (0.09)	0.08 (0.09)
Period	-0.06 (0.30)	-0.05 (0.34)
Constant	1.02 (1.15)	1.02 (1.29)
Session F.E.	Included	Included

Standard Errors Clustered at Subject Level
***, **, * indicates p<0.01, 0.05, 0.1 respectively

Table 20: Punishment for "Wise" Subjects in *Reflection Treatment*[a]

	Avg. Punishment		# Subjects Who Punish		p-values	
Profit of First Mover	DG Unsold	DG Sold	Unsold More	Sold More	Matched Pair, Signed Rank[b]	Permutation Test[c]
All	$2.67	$2.08	23	6	<0.01	<0.01
$10	$5.80	$4.47	6	0	0.02	0.03
$9	$4.20	$3.40	5	2	0.21	0.25
$8	$2.93	$2.13	4	1	0.15	0.19
$7	$1.80	$1.33	5	2	0.24	0.31
$6	$1.00	$0.80	3	1	0.30	0.50
$5	$0.33	$0.33	0	0	0.16	1.00

24 Subjects participated in the *Reflection Treatment*; 15 were "wise".
[a]To hold outcomes constant, only scenarios where intermediary makes no money included
[b]Matched Pair Signed Rank test based on null hypothesis that punishment distributions are identical for direct and indirect action. Pairs matched by subject.
[c]Permutation p-values based on 200,000 simulations per test.
Pairs matched by subject. P-values are from two-tailed tests.

Table 21: Survey 1 Summary Stats & Randomization

	Pooled	Direct	Indirect
Observations	202	108	94
Gender (Male=1, Female=0)	0.31	0.33	0.35
Freshman	0.33	0.36	0.30
Sophomore	0.32	0.33	0.30
Junior	0.31	0.27	0.36
Senior	0.03	0.04	0.02
Member of Charity Group (PBHA)	0.59	0.60	0.59

Table 22: Survey 2 Summary Stats & Randomization

	Pooled	Direct, Reward	Indirect, Reward	Direct, Guess	Indirect, Guess
Observations	135	42	31	31	31
Gender (Male=1)	0.35	0.40	0.29	0.35	0.35
Freshman	0.30	0.21	0.39	0.26	0.39
Sophomore	0.27	0.36	0.29	0.23	0.16
Junior	0.39	0.31	0.32	0.52	0.42
Senior	0.02	0.05	0.00	0.00	0.03
PBHA	0.64	0.67	0.65	0.68	0.55

Experiment Instructions

Preliminaries

Thank you for participating in this study. If at any time you have questions, please raise your hand, and I will assist you. From now until the end of the session, communication of any kind between participants is not allowed. Please do not use the computer for any other purpose than participating in this study. Also, please turn off your cell phones.

Overview

The purpose of this session is to study decision making.
You will make a series of decisions that will affect your payoffs as well as the payoffs of others.
All studies run here in the Computer Lab for Experimental Research (CLER) do NOT use deception. Everything in these instructions is true, including the rules and the payoffs. Everyone received the same instructions.

What are my payoffs?

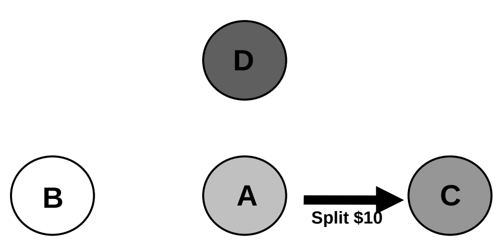
You will be paid your $10 show up fee plus the money made during the course of the following game.
As an example just for illustrative purposes: If you make $5 in the game, you will be paid $15 total.

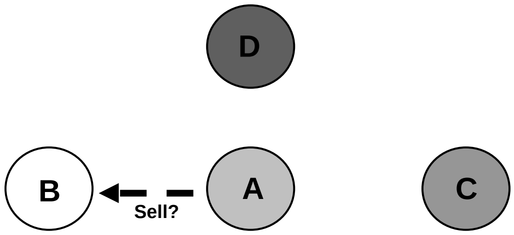
The Game

The game will be played in groups of four. We will play the game four times. Each time we play the game will be referred to as a "period".
In each period, everyone will be randomly assigned to one role and one group. You will play each role exactly once.
There are no computer players. The other three players in your group will be three other people in the room. You will be playing with three different people in each period.

Let's call the four roles A, B, C, and D. The game is completely anonymous, so players will only be referred to as A, B, C, or D for the duration of the experiment.

Here is how the game works:
Player A owns the rights to split $10 with C. That is, A decides how to split the $10, and the two players are paid accordingly. C has no say in the decision. C will simply be informed of A's anonymous decision.

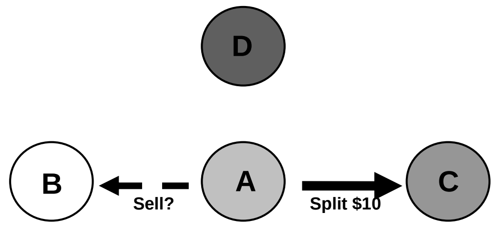


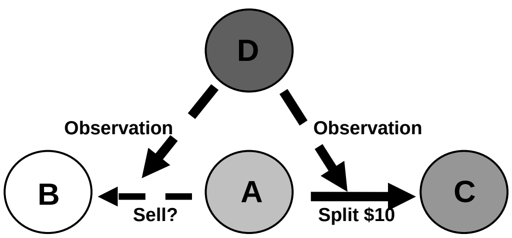


Before A splits the $10 however, A can "sell" the rights to play this game to player B.
A chooses whether to sell, and how much to sell for. B has no say in the decision.

If the game is "sold" to player B, then B splits $10 with C.
If the game is not "sold", then A splits $10 with C.
B's profit is whatever she keeps on top of the price she paid for the "Split $10" Game.

Also, Player B automatically has $5 in addition to any profit she earns in the game.

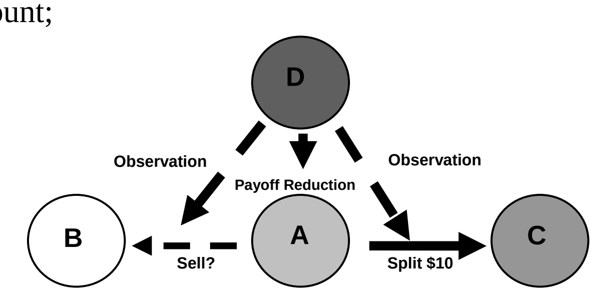




Player D then has the option of reducing Player A's payoff. They get to make this decision based on what happened in the game:
1. Whether the game was sold
2. If so, for how much
3. How was the $10 split with Player C

Player D can reduce Player A's payoff by any amount; However, she cannot reduce Player A to negative amounts.
So say Player A makes $7 (either by selling or playing the "Split $10" Game). Player D can punish any amount between $0 and $7.

Player D earns exactly $5 for the period no matter what punishment decisions she makes.



At the end of each period, you will not be told the decisions of the other players. We will learn what happened once we have played all four periods.
At that point, the computer will randomly choose one period. Your profits from that period, and that period only, will be the amount that you are paid when you leave, in addition to your show up fee.
Everyone will be paid for the same period. All four periods are equally likely to be the "payment period", so make thoughtful decisions in all four periods.

To recap, broken down by roles, the game is as follows:

Player A

- Owns the rights to anonymously split $10 with C, without any input from C
- Can "sell" this to B
- Can be punished by D
- If A sells the Split $10 game to B, Profit is equal to the price for which the game sells minus the punishment decided by D.
- If A does not sell the Split $10 game, Profit is equal to the amount A keeps in the "Split $10" game minus the punishment decided by D.

Player B

- Can purchase from A the rights to anonymously split $10 with C, without any input from C
- Cannot be punished by D
- If B is sold the Split $10 game by A, Profit is equal to the amount kept in the game minus the price paid for the game.
- If B is not sold the Split $10 game by A, Profit is 0.
- Will be paid $5 plus any profits made in this game.

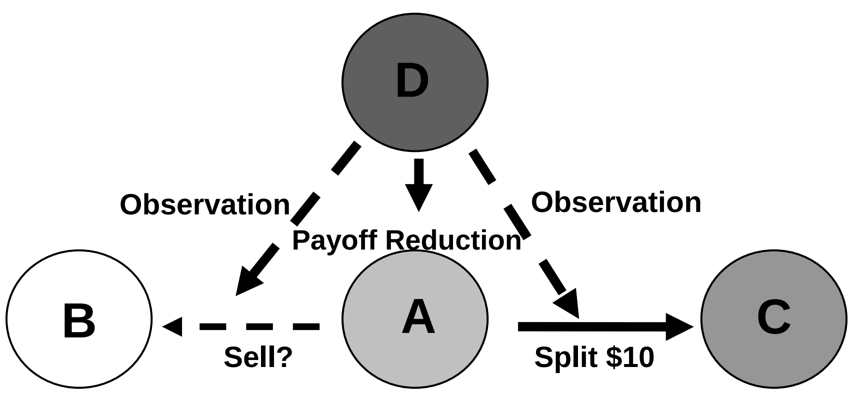
Player C

- Receives the amount that is decided by A or B. This is C's Profit.

Player D

- Based on the behavior of A and B, D can reduce A's payoff by any amount all the way to $0.
- Profit equals $5 no matter what.

Are there any questions about how this game works?



If at any time during the session you have questions, please raise your hand.

Good luck.