Contracting in the trust game

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Abstract

We present a simple mechanism that can be implemented in a simple experiment. In a modified trust game, the allocator can offer to pay the investor to cooperate. The mechanism is successful at implementing efficient outcomes: participants manage to achieve an efficient outcome, when this is possible, two–thirds of the time. While these results are encouraging, we find evidence that both concerns for fairness and motivation crowding out distort the incentives presented in the mechanism.

Keywords: compensation mechanism, side payment, trust game, signaling, crowding out, concerns for equity, taste for cooperation

JEL: C92, D62, H42

1 Introduction

There is a growing number of studies that examine mechanisms for implementing Pareto efficient outcomes in a subgame perfect Nash equilibrium.[1] Most previous results of experimental tests of these incentive schemes were discouraging. We find encouraging new results for the compensation mechanism. This mechanism has been tested in somewhat complex games, in which the best decisions are not independent of the beliefs about what strategy might be selected by the other player.[2] We look at a simple game, identify tensions between individual incentives and other motivations that distort incentives, and pin down what makes the compensation successful or unsuccessful.

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[1] See Moore and Repullo (1988) for a theoretical examination of implementability in subgame perfect equilibria.

[2] See Andreoni and Varian (1999) and Charness et al (2007) for tests in Prisoner's Dilemmas, Bracht et al. (2008) for a test in a public goods environment, and Chen and Gazzale (2004) for a game from the literature on solutions to externalities. These papers analyze games with multiple stages with the players choosing simultaneously in the final stage. These types of games make drawing inferences from observed actions very difficult. See Section 5.3. for a discussion of the related literature on the compensation mechanism.

In the trust game, one player, the investor, has the choice of investing or not investing in a project that is administered by the other player, the allocator. The investment is always successful: the amount invested multiplies in value. However, the allocator controls the proceeds of investment: he may keep the total amount for himself or split it evenly with the investor (Berg et al. (1995)). The economist's prediction for this game is that a selfish allocator will always keep all proceeds from an investment rather than share with the investor. A selfish investor who understands the allocator's situation knows that any investment will be lost, and so will choose not to invest. However, both investor and allocator are better off if the investor sends the money and the allocator returns the fair share.[3]

Social scientists across many disciplines have found trust games helpful in thinking about phenomena ranging from the strength of political institutions to the effectiveness and sustainability of development projects.

We look for an institution to change the game from a social dilemma to a game in which cooperation is sustainable as an equilibrium. We add another stage, a contracting stage, to the trust game in which the allocator can make a binding commitment to pay the investor some amount if she chooses to invest.[4] The idea is straightforward: in the mechanism, subgame perfect equilibria are Pareto efficient. The equilibria imply a transfer from the allocator to the investor. The amount of the transfer is at least as large as the amount the investor would receive if she were not to invest.

Coase (1960) presents an example similar to our game: a rancher's cattle stray onto the farmer's property and damage the crops. Coase argues that the efficient outcome will result because the farmer would have an incentive to pay the rancher to reduce the number of straying cattle.

We show that the mechanism was largely successful at increasing efficiency, and that the participants played reasonably close to the subgame perfect equilibrium, with qualifications. On the other hand, many experimental researchers have found that behavioral theories can characterize aspects of decisions that standard theory cannot. So, we also examine a few behavioral sources of hypotheses. According to taste for cooperation (Palfrey and Rosenthal (1988)), individuals have a natural tendency to be nice. We also consider fairness theory (Prasnikar and Roth (1992)), according to which individuals dislike unequal payoffs. According to crowding out (Deci (1971)), some individuals have an inherent tendency toward cooperative behavior. This tendency may be damaged by mechanisms providing financial incentives for such behavior because the introduction of an incentive scheme can repress the expression of altruism. Hence, mechanisms that provide weak financial incentives (too small to change monetary best responses) lead to less cooperation than if there had been no mechanism at all—in contrast to the equilibrium prediction of no effect. At the same time, mechanisms that provide strong financial incentives (strong enough to change monetary best responses) lead to more cooperation than if there had been no mechanism at all—in accordance with the equilibrium prediction of an effect. We also consider a signaling theory (Bracht and Feltovich (2008)), according to which a choice by the allocator of the largest possible offer amount is a signal

[3] We use terms such as theoretical prediction, equilibrium prediction, or prediction of game theory to mean the combination of appropriate equilibrium concepts (usually subgame perfect equilibrium) and the assumption that preferences concern only players' own monetary payoffs. We acknowledge that this is an abuse of terminology, as game theory itself makes no assumptions about what form preferences take; different preferences may lead to different theoretical predictions.

[4] The game we look at is modeled after work by Varian, regarding preplay contracting. The term compensation mechanism was introduced by Varian (1994a,b).

that the allocator intends to split the proceeds of investment. Think of signaling as a good will gesture by allocators. Consequently, behavior following a given offer amount should depend to some extent on which other amounts were permitted; specifically, investment (if investors interpret this behavior as signals) and splitting (if allocators actually are signaling) will be higher when the offer amount is the highest possible. We see some systematic deviations from our predicted equilibrium, but this behavior is consistent with participants trying to signal cooperative intentions similar to those observed in other experiments.

The rest of the paper is organized as follows. Section 2 describes the new game. Section 3 presents the experimental design. Section 4 discusses the hypotheses. Section 5 describes the results and discusses findings of the related literature. Section 6 concludes.

2 Pay for Invest

We begin by describing the exact form of our three-stage game, which we call *Pay for Invest*. Figure 1 shows a standard trust game with two players (investor, allocator). We introduce a prior stage to this game where the allocator can announce a number, indicating the amount that he will pay the investor if she chooses *Invest*. This announcement is binding: once the allocator offers a contract, he is obligated to carry it out. Figure 2 shows the subgame of the *Pay for Invest* after a side payment of s is chosen.

Figure 1: The basic trust game.

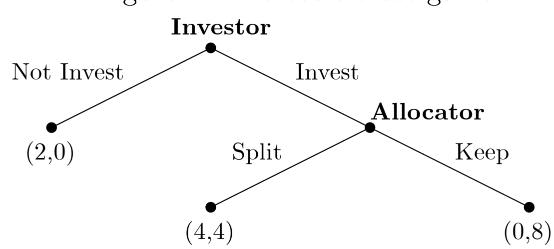
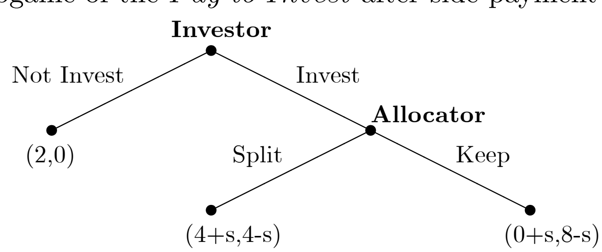


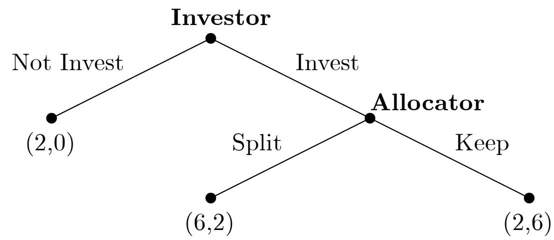
Figure 2: Subgame of the *Pay to Invest* after side payment s is chosen.



Let us calculate the subgame perfect equilibrium of such a game. The minimal payment that would induce the investor to cooperate is 2. If this payment is announced, i.e., $s = 2$, then the subgame is transformed (Figure 3). It is clear that $s = 2$, followed by *Invest* and *Keep*, is the unique subgame perfect equilibrium of the *Pay for Invest*.[5]

[5] Indeed, the allocator will always choose Keep after investment as $8 - s > 4 - s \Leftrightarrow 8 > 4$, the investor will invest if and only if she is offered a good side payment, i.e., $s \geq 2$, and the allocator will choose $s = 2$.

Figure 3: Subgame of the *Pay to Invest* after side payment $s = 2$ is chosen.



In the experimental game that we consider, the side payments are restricted to being integers. This restriction adds a new subgame perfect equilibrium to the game, namely, one where the allocator pays one unit more than the break-even announcement, i.e., $s = 3$. In the subgame following this offer of a side payment, it is still a dominant strategy for the allocator to choose *Keep* after investment. This equilibrium is supported by the pessimistic expectations that if the investor is indifferent between her two strategies she will choose not to cooperate. The other equilibrium is supported by the optimistic expectations that if the investor is indifferent between *Invest* and *Not Invest*, she will choose *Invest*.

3 Experiment

Our experiment was run on a computer network (Fischbacher (2007)). Instructions used in the experiment are presented in the Appendix. To give the mechanism the greatest challenge, we ran first 5 rounds of the basic trust game with each participant randomly reassigned a different counterpart each time. As is commonly observed, the participants started out cooperating but soon switched to defecting. By the final round, most participants were playing the noncooperative strategy. We then switched to the new game, *Pay for Invest*. In this game, each allocator names an amount that he will pay the investor if the investor chooses to invest. Once the allocator has committed to his payments, the amount is revealed to the investor. We repeated the *Pay for Invest* for 10 rounds. We consider four treatments, differing in S. In our control treatment *side*0, no side payment is possible ($S = \{0\}$). In our *side*3210, *side*210, and *side*10 treatments, $S = \{3, 2, 1, 0\}$, $S = \{2, 1, 0\}$, and $S = \{1, 0\}$, respectively.

Altogether, participants played a total of 15 rounds, 5 in the basic game and 10 in *Pay for Invest*. We conducted 13 sessions in all, using 240 participants, with 60 participants per treatment.[6] After each round, participants were reassigned to a different participant in the other group. Each session of the experiment was completed within 1 hour, and participants earned around £11 (including a £5 payment for showing up). We recruited participants with an online recruiting system.

[6] The data from the control treatment is from Bracht and Feltovich (2008). The control treatment *side*0 has 3 sessions with 20 participants in each session; the treatment *side*01 has 4 sessions with 14, 20, 14, and 12 participants; the treatment *side*210 has 3 sessions with 14, 14, and 28 participants; the treatment *side*3210 has 3 sessions with 20, 22, and 18 participants. The sessions for the new treatments were conducted at the economics laboratory at the University of Aberdeen Business School.

4 Hypotheses

The theoretical observations lead to the following hypotheses.

Hypothesis 1 *The frequency of Invest will be higher following an offer amount of 3 or 2 than following an offer amount of 1 or 0.*

Hypothesis 2 *The frequency of Invest will be the same following an offer amount of 1 and 0.*

Hypothesis 3 *The frequency of Split will be the same following any offer amount.*

While these predictions are clear, there is some reason to think the actual impact of the compensation mechanism might be different.

Individuals may have a natural desire to be nice,[7] a hypothesis that has received a fair amount of support in prior experiments.[8] If the investor transfers 2 units to the allocator, then the allocator gets 8 units. This implies a cost of cooperation of 1/4. However, if the allocator transfers 4 units, then the investor gets 4 units. This implies a cost of cooperation of 1. Hence, if there is a utility from seeing someone else's satisfaction increased, we may expect more cooperation from investors than from allocators. This motive gives rise to the conjecture that the propensity to cooperate differs across the two types of players.

Furthermore, if there is a utility component in cooperation above and beyond the monetary gain, then an announcement of an offer amount of 1 might suffice to tempt the investor to invest. Hence, the allocator may not need to buy out the investor entirely in order to induce such investment. This leads us to replace hypotheses 1 and 2 with hypothesis 4.

Hypothesis 4 *The frequency of Invest will be higher following an offer of a side payment of 3 or 2 or 1 than following no side payment.*

Individuals may have a natural taste for fairness,[9] a hypothesis that has received a fair degree of support in prior experiments.[10] If an offer of a side payment of 2 is announced, then the investor invests and the allocator keeps the return of investment. In such an equilibrium, the allocator's payoff is 6, and the investor's payoff is 2. Now, the investor might envy the allocator's fortune, and may prefer not to invest if only offered such a break-even amount (resulting in an investor's payoff of 2 and an allocator's

[7] We may reformulate this cooperation motive in the language of utility theory: the welfare of each individual depends not only on her utility but also on her contributions to the utility of others. She derives a utility from seeing someone else's satisfaction increased.

[8] In the economics literature, see Arrow (1972) for a discussion of effects of subtle forms of giving on the allocation of economic resources, Andreoni and Miller (2002) for general tastes for giving, Palfrey and Rosenthal (1988) for a discussion of social dilemmas, Andreoni and Miller (1993) on the Prisoner's Dilemma, Andreoni (1995), and Palfrey and Prisbrey (1997) on public goods.

[9] We may reformulate this fairness motive in the language of utility theory: the welfare of each individual will depend both on her own satisfaction and on the satisfaction obtained by the other. We have in mind both a positive relation, one of altruism, and a negative relation, one of envy.

[10] For early evidence from economists that participants dislike unequal payoffs see Prasnikar and Roth (1992) or Andreoni, Brown, and Vesterlund (1999).

payoff of 0). This motive gives rise to the conjecture that an announcement of an offer amount of 2 might be ineffective in increasing investment. This leads us to modify hypotheses 1 and 2, and replace them with hypothesis 5.

Hypothesis 5 *The frequency of Invest will be higher following an offer of a side payment of 3 than following an offer of 2 or 1 or no offer.*

Individuals may have an inherent tendency toward cooperation, which is damaged by a mechanism that provides financial incentives for such behavior.[11] This phenomenon– crowding out – is often seen in games like ours.[12] We consider the following interpretation of the notion of crowding out. In our control treatment- where no external rules are imposed- levels of investment and splitting ought to be higher than theory predicts. In our *side*10 treatment, the rules are not strong enough to make *Invest* rational, and levels of cooperation ought to be as theory predicts. Hence, cooperation levels should be less in the *side*10 treatment than in the control treatment. This leads us to modify hypotheses 2 and 3, and replace them with hypotheses 6 and 7.

Hypothesis 6 *The frequency of Invest will be higher in the control treatment than in each of the other treatments following an offer amount of 1 or 0.*

Hypothesis 7 *The frequency of Split will be higher in the control treatment than in the other treatments following an offer amount of a side payment of 1 or 0.*

In our *side*210 and *side*3210 treatments, the rules are strong enough to make *Invest* rational, so there should be high levels of investment and low levels of splitting. Hence, the frequency of *Split* will be higher in the control treatment than in the other treatments following an offer amount of 3 or 2. This leads us to add hypothesis 8 (while keeping hypotheses 1, 6, and 7).

Hypothesis 8 *The frequency of Split will be higher in the control treatment than in the other treatments following an offer of a side payment of 3 (2) or 0.*

Individuals may try to signal cooperative intentions. This phenomenon– signaling – is seen in games like ours.[13] We consider the following interpretation of the notion of signaling. Allocators who intend to *Split* will signal their cooperative intention by offering the maximum possible side payment amount. In the control treatment, there is no opportunity for signaling. In the *side*10 treatment, however, such signaling would imply that an offer amount of 1 leads to more cooperation: investors will anticipate that allocators intend to choose *Split*, and choose *Invest*; allocators will follow through and, indeed, choose *Split*. Thus, other things equal, cooperation should be more likely when the offer amount chosen by the allocator is the largest offer amount possible. This leads us to modify hypotheses 2 and 3, and replace them with hypotheses 9, 10, 11, and 12 (while keeping hypothesis 1).

[11] For discussions of crowding out and its implications, see Deci (1971), Arrow (1972), Kreps (1997), Frey (1997), and Ostrom (2000).

[12] See Fehr and Rockenbach (2003), Fehr and List (2004), Andreoni (2005), and Bracht and Feltovich (2008) for evidence of crowding out in trust games.

[13] See Bracht and Feltovich (2008) for evidence of signaling in the escrow game.

Hypothesis 9 *In the* $side10$ *treatment, the frequency of Invest will be higher following an offer amount of 1 than following an offer amount of 0.*

Hypothesis 10 *In the* $side10$ *treatment, the frequency of Split will be higher following an offer amount of 1 than following an offer amount of 0.*

Hypothesis 11 *Following an offer amount of 1, the frequency of Invest will be higher in the* $side10$ *treatment than in the* $side210$ *treatment or the* $side3210$ *treatment.*

Hypothesis 12 *Following an offer amount of 1, the frequency of Split will be higher in the* $side10$ *treatment than in the* $side210$ *treatment or the* $side3210$ *treatment.*

5 Results

Figure 4 shows the fraction of participants choosing to cooperate over all 15 iterations of the game. In rounds $1-5$, which are just the standard trust game, 58.3% of investor decisions are cooperative, which declines to 32.5% in the final round; 46.3% of allocator decisions are cooperative, which declines to 6.7% in the final round.[14][15]

Now, look at rounds $6-15$. In the second phase of the control treatment, the frequency of $Invest$ jumps sharply upward.[16] In the three new treatments, the corresponding frequencies also jump upward, though it is unclear whether this is also a restart effect or the result of change in the game. Despite those similarities in $Invest$ frequencies in round 6, the frequencies diverge quickly: in the control treatment, the frequency initially drops rapidly and then stays around 1/3; in the $side10$ treatment, the frequency stays around 1/2, drops in the final two rounds but stays above 1/5; in the $side210$ treatment, the frequency stays around 1/2; in contrast, in the $side3210$ treatment, the frequency jumps upward, steadily increases, and reaches 4/5 in the final rounds.

In the second phase of the control treatment, the frequency of *Split* jumps upward and stays around 2/5 with variations; in the $side10$ treatment, the frequency jumps upward as well and stays at a sizeable level at 1/3 with variations; in both the $side210$ and $side3210$ treatments, the frequency starts at a low level, declines slightly and stays somewhere around 1/10 with variations.

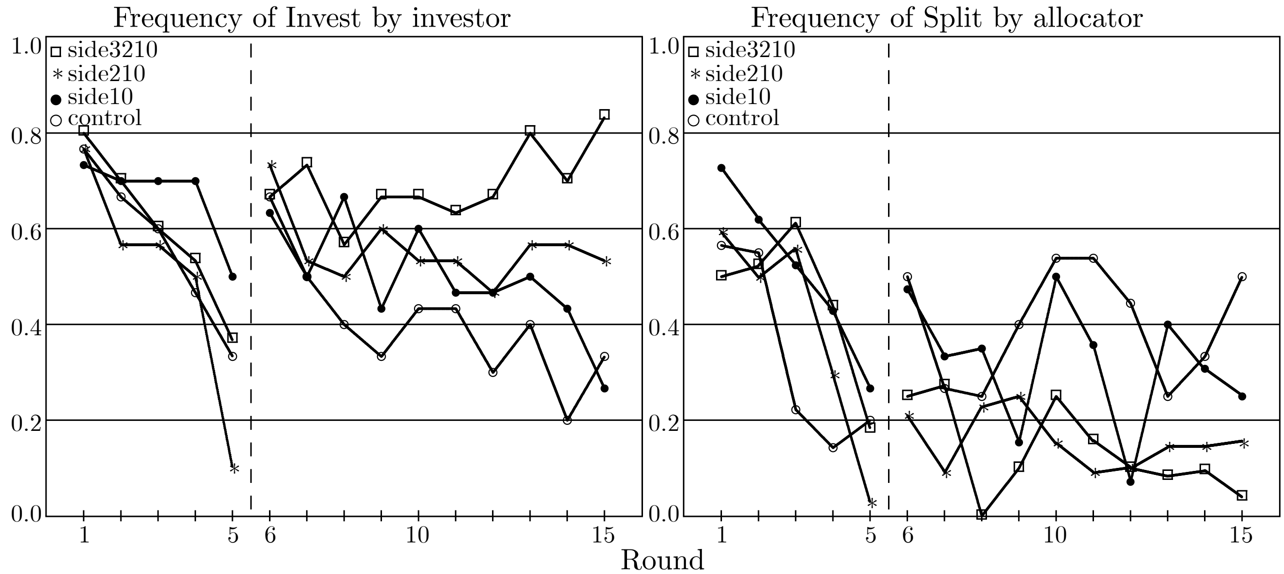
Table 1 reports the frequency of $Invest$ and *Split* on data from the first phase (Rounds $1-5$) and the second phase (Rounds $6-15$) of the experiment, aggregated over participants and rounds. We find

[14] The data from the first phase show evidence of the cooperation motive. This motive implies that investor cooperative behavior should be more frequent than allocator cooperative behavior. When we test for differences in the propensity to cooperate in the first half across the two types of players, investors and allocators, we find that the difference is statistically significant ($z = 2.231$, $p\ value \approx 0.026$ over 5 rounds; $z = 1.946$, $p\ value \approx 0.052$ in the final round).

[15] We test for differences in behavior between treatments on session-level data from the control phase. A Chi-square test rejects the null hypothesis that frequencies of Invest in periods $1-5$ are equal across the four cells ($\chi^2 = 8.9143$, $df = 3$, $p \approx 0.030$). A Chi-square tests fails to reject the null hypothesis that frequencies of Split in periods $1-5$ are equal across the four cells ($\chi^2 = 5.6597$, $df = 3$, $p \approx 0.129$). Hence, we found differences in behavior between treatments on data from the control phase (where there should be none). In this section, we abstract from those individual differences.

[16] One sees this restart effect (Andreoni (1988)), even though no feature of the game has changed.

Figure 4: Participant behavior in each round; control and treatments.



that, across all sessions in the second phase of the control treatment (i.e., the repetition of the basic trust game), the frequency of both *Invest* and *Split* is around 40%.[17] We now assess the impact of each of the three new treatments by analyzing the difference in the frequency of the cooperative action (*Invest* and *Split*) between the control treatment and the new treatments. During the entire second half, 49.7% of investor moves in the *side*10 treatment are cooperative; 55.7% of investor moves in the *side*210 treatment are cooperative; and 69.3% of investor moves in the *side*3210 treatment are cooperative. During the second half, 33.6% of allocator moves in the *side*10 treatment are cooperative; 13.2% of allocator moves in the *side*210 treatment are cooperative; 13.5% of allocator moves in the *side*3210 treatment are cooperative.[18]

We now report results of conventional Wilcoxon rank-sum tests of the hypothesis that two independent samples are from populations of the same distribution.[19] The test of the cooperative action *Invest* shows no difference between the control treatment and the *side*10 treatments ($z = -1.061$, $p\ value > 0.2888$). The test of *Invest* shows no difference between the control treatment and the *side*210 treatments ($z = -1.091$, $p\ value > 0.2752$). The test of *Invest* shows a significant difference between the control treatment and the *side*3210 treatments ($z = -1.964$, $p\ value > 0.0495$). The test of *Invest* shows no difference between the *side*10 and the *side*210 treatments ($z = -0.707$, $p\ value > 0.4795$). The test of *Invest* shows a weakly

[17] The data from the second phase show no evidence of the cooperation motive. This motive implies that investor cooperation is more frequent than allocator cooperation. We do not find a significant difference in cooperative behavior in the second half of the control treatment ($z = -0.218$, $p\ value$ 0.827 over 10 rounds; $z = -1.107$, $p\ value \approx 0.268$ in the final round).

[18] The data show some evidence of crowding out. Crowding out implies that cooperation should be less frequent when mechanism *side*10 is imposed than when there is no mechanism. In fact, the overall frequency of *Invest* is 49.7% and the overall frequency of *Split* is 33.6%, one higher and one lower than their counterparts in the control treatment.

[19] We follow the convention that a p value of 1% or less indicates a highly significant difference, a p value between 1% and 5% indicates a significant difference, and a p value between 5% and 10% indicates a weakly significant difference.

In the nonparametric tests, sessions are treated as individual observations.

Table 1: How effective are the treatments?

	Frequency of Invest		Frequency of Split	
Treatment	Rounds 1–5	Rounds 6–15	Rounds 1–5	Rounds 6–15
*side*0	0.5667 (85/150)	0.4000 (120/300)	0.3765 (32/85)	0.4083 (49/120)
*side*10	0.6667 (100/150)	0.4967 (149/300)	0.5300 (53/100)	0.3356 (50/149)
*side*210	0.5000 (75/150)	0.5567 (167/300)	0.4533 (34/75)	0.1317 (22/167)^{a,b}
*side*3210	0.6000 (90/150)	0.6933 (208/300)^{a,b,c}	0.4778 (43/90)	0.1346 (28/208)^{a,b}
All	0.5833 (350/600)	0.5367 (644/1200)	0.4629 (162/350)	0.2314 (149/644)

a significantly different from *side*0; b sig. different from *side*10; c sig. different from *side*210.

significant difference between the *side*10 and the *side*310 treatments ($z = -1.768$, $p\ value > 0.0771$). The test of *Invest* shows a significant difference between the *side*210 and the *side*3210 treatments ($z = -1.964$, $p\ value \approx 0.0495$).

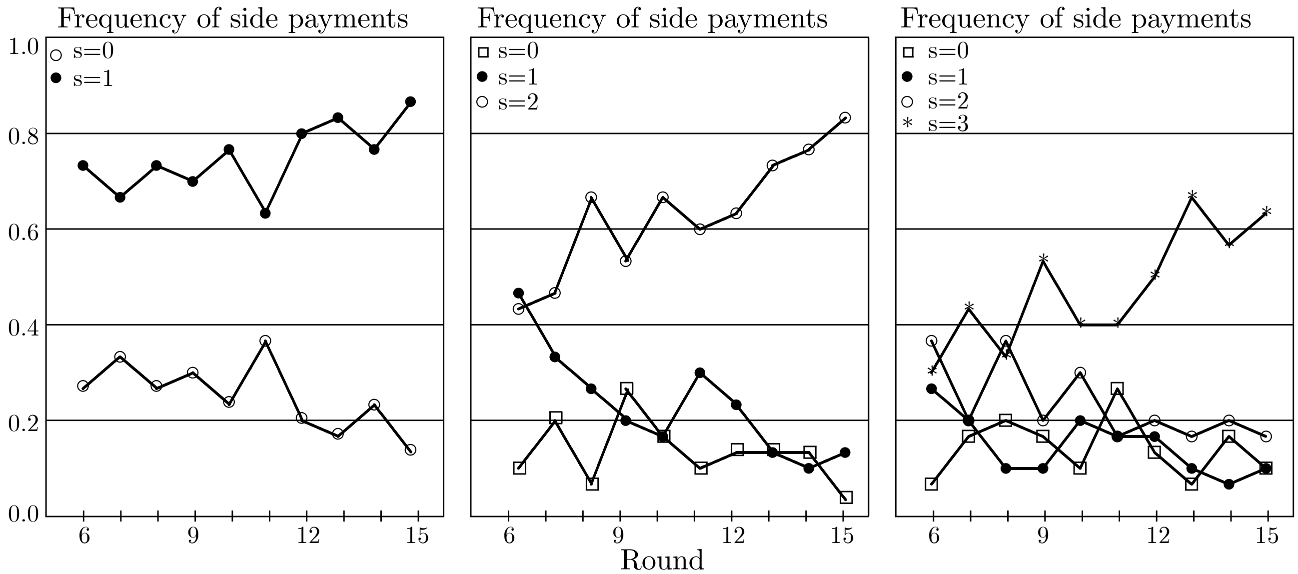
The test of the cooperative action *Split* shows no difference between the *side*0 and the *side*10 treatments ($z = 0.707$, $p\ value > 0.4795$). The test of *Split* shows a significant difference between the *side*0 and the *side*210 treatments ($z = 1.964$, $p\ value \approx 0.0495$). The tests of *Split* shows a significant difference between the *side*0 and the *side*3210 treatments ($z = 1.964$, $p\ value \approx 0.0495$). The test of *Split* shows a weakly significant difference between the *side*10 and the *side*210 treatments ($z = 1.768$, $p\ value \approx 0.0771$). The test of *Split* shows a weakly significant difference between the *side*10 and the *side*310 treatments ($z = 1.768$, $p\ value \approx 0.0771$). The test of *Split* shows a no significant difference between the *side*210 and the *side*3210 treatments ($z = 0.218$, $p\ value > 0.8273$).

Clearly, the mechanism has an overall effect on investor cooperation and efficiency when the allocator is able to make a very good offer of a side payment. But, even when a very good offer is possible, the mechanism is far from 100% successful. Furthermore, when only a break-even offer is possible, the mechanism's success is uncertain. There could be two reasons: allocator participants could be failing to make the subgame perfect side payments in the first stage, and, when investor participants are offered good side payments, they fail to respond optimally. We look at the possibilities next.

5.1 Side Payments

Figure 5 shows the time series of the average frequency of allocator offers in each of the three side-payment treatments; the panel to the left displays *side*10 with $S = \{1, 0\}$, the panel in the middle displays *side*210 with $S = \{2, 1, 0\}$, and the panel to the right displays *side*3210 with $S = \{3, 2, 1, 0\}$. Over the first 5 rounds in treatment *side*10, the average offer is 0.720, which rises to 0.780 for the final 5 rounds. Over the first 5 rounds in treatment *side*210, the average offer is 1.393, which rises to 1.607 for the final 5 rounds. Over the first 5 rounds in treatment *side*3210, the average offer is 1.947, which rises to 2.140 for the final five rounds.

Figure 5: Relative frequency of side payments in each side-payment treatment; *side*10 (left panel), *side*210 (middle panel), and *side*3210 (right panel).



5.2 Conditional Cooperation

Table 2 reports the relative frequencies of the choices of the amount of the side payment, the relative frequencies of *Invest* choices– conditioned on the side payment amount– and the relative frequency of *Split* choices– conditioned on the side payment amount and given *Invest*, in the second phase of the experiment. The table shows these quantities broken down by treatment.

Following the offer amount of 3 in the *side*3210 treatment, investors invest over 90% of the time; after investment, allocators split less than 4% of the time; allocators choose the amount of 3 almost half of the time. When allocators choose a lower amount in *side*3210, investors frequently invest, though they do invest more often following an amount of 2 (67% of the time) than after an offer amount of 1 (34% of the time) or a 0 amount (30% of the time); following investment after a small offer, allocators split with frequency between 26% and 47%, depending on the side payment amount.

Following the offer amount of 2 in the *side*210 treatment, investors invest 70% of the time; after investment, allocators split less than 8% of the time; allocators choose the amount of 2 almost two-thirds of the time. When allocators choose a lower amount in *side*210, investors seldom invest, though they do invest more often following an amount of 1 (36% of the time) than after a 0 amount (23% of the time); following investment after a small offer, allocators split with frequency between 33% and 36%, depending on the side payment amount.

Following the offer amount of 1 in the *side*10 treatment, investors invest 56% of the time; after investment, allocators split more than 39% of the time; allocators choose the amount of 1 three-fourths of the time. When allocators make no offer in *side*10, investors invest one-third of the time; following investment after no offer, allocators split only 4% of the time, depending on the side payment amount.

Table 2: How rational are participants?

Treatment	Side payment amount	Frequency chosen		Conditional frequency- *Invest*		Conditional frequency- *Split*	
*side*0	0	1.000	(300/300)	0.400	(120/300)	0.408	(49/120)
*side*10	0	0.250	(150/600)	0.320	(48/150)	0.042	(2/48)
	1	0.750	(450/600)	0.556	(250/450)	0.392	(98/250)
*side*210	0	0.133	(80/600)	0.225	(18/80)	0.333	(6/18)
	1	0.233	(140/600)	0.357	(50/140)	0.360	(18/50)
	2	0.633	(380/600)	0.700	(266/380)	0.075	(20/266)
*side*3210	0	0.143	(86/600)	0.302	(26/86)	0.308	(8/26)
	1	0.147	(88/600)	0.341	(30/88)	0.467	(14/30)
	2	0.233	(140/600)	0.671	(94/140)	0.255	(24/94)
	3	0.477	(286/600)	0.930	(266/286)	0.037	(10/266)

Note that the levels of *Invest* and *Split* depend not only on the side payment amount, but also on what choices were available. Investment tends to be higher following a given offer decision when that was the largest possible amount than when it was not. For instance, a 0 amount is the largest possible offer of a side payment in the *side*0 treatment, but a larger amount was possible in three other treatments. In the *side*0 treatment, the frequency of *Invest* is 0.40; in the *side*10 treatment, the frequency of *Invest* is 0.32; in the *side*210 treatment, the frequency of *Invest* is 0.23; in *side*3210 treatment, the frequency of *Invest* is 0.30. A 1 amount is the largest possible amount in the *side*10 treatment, but a larger amount was possible in the *side*210 and *side*3210 treatments. In fact, the frequency of investment following 1 is 0.56 in the *side*10 treatment but only 0.36 in the *side*210 treatment and 0.34 in the *side*3210 treatment. A 2 amount is the largest possible amount in the *side*210 treatment, but a larger amount is possible in the *side*3210 treatment. Indeed, the frequency of *Invest* is 0.70 in the *side*210 treatment and 0.67 in the *side*3210 treatment.

We conclude that the data from investors show strong evidence of signaling. Signaling implies that *Invest* should be more frequent when an offer of 1 is made in the *side*10 treatment than when there is no offer. Signaling also implies that *Invest* should be more frequent when an offer of 1 is made in the *side*10 treatment than in the other treatments in which an offer of 1 is possible.

The pattern does not always hold for allocator. Splitting tends to be higher following a given side payment decision rather when that is the largest possible amount than when it is not. A 0 amount is the largest possible offer of a side payment in the *side*0 treatment, but a larger amount was possible in three other treatments. Indeed, in the *side*0 treatment, the frequency of *Split* is 0.41; in *side*10, the frequency is 0.04; in *side*210, the frequency is 0.33; in *side*3210, the frequency is 0.31. A 1 amount is the largest possible amount in the *side*10 treatment, but a larger amount was possible in the *side*210 and *side*3210 treatments; in *side*10, the frequency of *Split* is 0.39; in *side*210, the frequency is 0.36; however, in *side*3210, the frequency is 0.47. A 2 amount is the largest possible in the *side*210 treatment, but a larger amount is possible in the *side*3210 treatment; the frequency is 0.08 in *side*210; however, the frequency is 0.26 in *side*3210. We conclude that the data from allocators show some evidence of signaling.[20]

The crowding-out theory makes almost the opposite prediction to signaling. Crowding out implies that *Invest* should be more frequent in the control treatment than in the other treatments following a low offer amount of 1 or 0. The frequency of *Invest* in the control treatment is 0.40. Following a bad offer, the frequency of *Invest* is 0.50 in *side*10, 0.31 in *side*210, 0.32 in *side*3210, and 0.42 across *side*10, *side*210, and *side*3210. We conclude that there is no evidence of crowding out for investors.

Crowding out implies that *Split* should be more frequent in the control treatment than in *side*10, *side*210, and *side*3210 following a bad offer. The frequency of *Split* in the control treatment is 0.41. Following a bad offer, the frequency of *Split* is 0.34 in *side*10, 0.35 in *side*210, 0.39 in *side*3210, and 0.35 across *side*10, *side*210, and *side*3210. We conclude that there is some evidence of crowding out for allocators.

Figure 6: Conditional frequency of investment in the three side payment treatments *side*10 (left panel), *side*210 (middle panel), and *side*3210 (right panel).

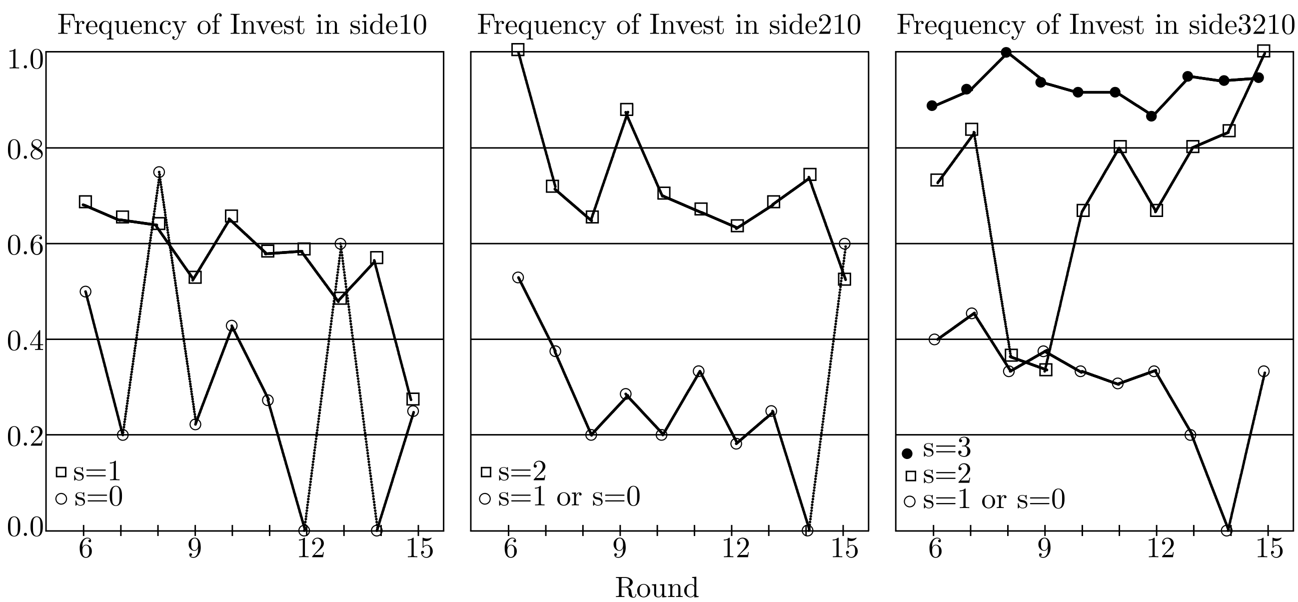


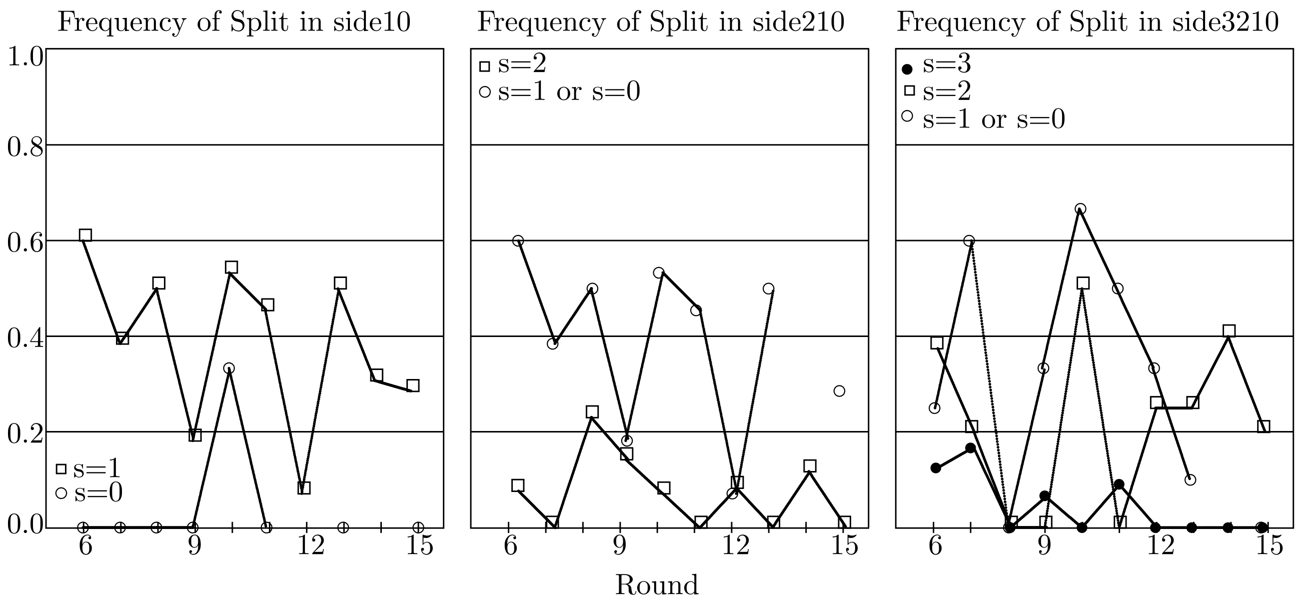
Figure 6 shows the time series of average frequency of investor cooperation conditioned on the offer amounts. The panel to the left shows treatment *side*10, the panel in the middle shows treatment *side*210, and the panel to the right shows *side*3210. When receiving a good offer over the first 5 rounds in treatment *side*210, investor cooperation is 0.77, which falls to 0.65 in the final 5 rounds. When receiving a good offer over the first 5 rounds in *side*3210, investor cooperation is 0.79, which rises to 0.90 in the final 5 rounds.

Figure 7 shows the time series of the average frequency of allocator cooperation in the three side payment treatments *side*10 (left panel), *side*210 (middle panel), and *side*3210 (right panel). When making a good offer over the first 5 rounds in treatment *side*210, allocator cooperation is 0.08, which falls to 0.03

[20]Signaling implies that *Split* should be more frequent when an offer of 1 is made in the *side*10 treatment than when there is no offer. Signaling also implies that *Split* should be more frequent when an offer of 1 is made in the *side*10 treatment rather than in either the *side*210 treatment or the *side*3210 treatment.

in the final 5 rounds. When making a good offer over the first 5 rounds in treatment *side*3210, allocator cooperation is 0.11, which falls to 0.06 for the final 5 rounds.

Figure 7: Conditional frequency of splitting in the three side-payment treatments *side*10 (left panel), *side*210 (middle panel), and *side*3210 (right panel).



Let us summarize the data: When equilibrium predicts a change across or within treatments, that change in the predicted direction is seen in the data.[21] Consistent with hypothesis 1, investment is more frequent in the *side*210 and *side*3210 treatments following a good offer than after any bad offer in any treatment. On the other hand, equilibrium does not predict a change across treatments for allocator behavior given any offer amount. However, inconsistent with hypothesis 3, splitting is less frequent following a 3 or a 2 amount (given $Invest$) than following a 1 or 0 amount (given $Invest$), but splitting is more frequent following a 1 amount (given $Invest$) than following a 0 amount (given $Invest$). Inconsistent with hypothesis 2 but consistent with hypothesis 4, investment is more frequent following a 1 amount than following a 0 amount. Hence, the data show some evidence of taste for cooperation. Consistent with hypothesis 5, investment is more frequent following a 3 amount than following a 2 amount. Hence, the data show some evidence of taste for fairness.

Second, we have seen evidence that investors believe that allocators signal cooperative intentions by choosing the largest offer amount possible. Indeed, investment is less frequent in treatment *side*210 after a small offer amount than in both *side*10 and *side*0 treatments in which only small offer amounts were possible. Furthermore, while allocators make efficient offers more frequently as play continues, overall in the *side*210 treatment, one-third of the actual offer amounts are too low to induce rational investment. When investors do receive efficient offers, there is some hesitation to invest– even after participants gain experience–, possibly because of some investor aversion to unfairness.[22] Hence, the *side*210 treatment

[21] Equilibrium's point predictions perform poorly; we rarely see levels of investment and splitting close to zero.

[22] We saw that seven-tenths of invester decisions are uncooperative following an offer amount of 2.

does not have a significant overall effect on investor cooperation (versus either the *side*10 treatment or the *side*0 treatment). The *side*3210 treatment performs better than the *side*210 treatment: in *side*3210, allocators learn only slowly to offer the largest possible amount, but when the largest possible amount is offered, investors invest very frequently (given the usual noise and confusion in experiments). The *side*10 treatment performs worse than the *side*210 treatment: In *side*10, when allocators choose the larger of the two inefficient offers, investment is encouraged. However, allocators do not offer the amount of 1 frequently enough, and even following an offer amount of 1, the rate of investment is still well below 100%.

The four models of Table 3 refines our understanding of both treatment effects and cooperation dynamics. We look at two samples: the first set includes all observations; the second set is a subset of this data- which includes observations only if the investor invested. We work with two types of regression models. The first specification (Short Invest (SI)) looks at investor behavior, and the dependent variable is an indicator variable for the invest choice. The variable invest takes on the value 1 if the investor invests and 0 if the investor does not invest. To control for time dependencies, we include the variable round number (which takes on the values $1, 2, ..., 15$, corresponding to the rounds) and an indicator variable for the second phase of the experiment (value 1 if round number≥ 6 and 0 otherwise). The main explanatory variables are six pairs: three indicator variables for each treatment (*side*10, *side*210, and *side*3210), three variables for each offer amount (1, 2, and 3) and the interactions of those variables with the time trend variable. The second model looks at allocator behavior (Short Split (SS)) and so the dependent variable is an indicator for the split choice. We use the same set of independent variables. The data is restricted to the subsample that follows investment by the investor.

Our next two specifications (Long Invest (LI) and Long Split (LS)) are similar to SI and SS, but add three pairs of variables designed to capture the interaction between the non-control treatments and a 1 offer amount, so that we can assess our crowding-out and signaling hypotheses. The first pair consists of the product of our *side*10 and 1 offer amount indicator, and the product of this new variable with the round number. The other two pairs use the *side*210 or *side*310 variables instead of *side*10. To avoid perfect collinearity, we remove both the 1 offer amount variable and its product with the round number.

Table 3 shows the results of the four regressions: coefficient estimates, standard errors, value of the log-likelihood, the $pseudo-R^2$, and the BIC and AIC information criteria.[23] These models are not nested, so we compare them with information criteria. The Bayesian Information Criterion favors the two simpler models: model specification SI has a BIC of 2075.06 which is lower (and thus better) than the 2098.52 of model specification LI, and model specification SS's BIC of 1010.89 is better than model specification LS's BIC of 1023.87. The Akaike Information Criterion favors the simpler model for investors but the less simple for allocators: SS has a AIC of 942.26 which is higher and (thus worse) than LS's 935.64, and SI's AIC of 1998.13 is better than LI's AIC of 1999.60. Because the simpler models tend to fare better under these criteria, and because comparison of the columns of Table 3 suggests that the results are reasonably robust to our specification, we will confine our discussion of results to the first two columns– unless we need the extra variables used in the other columns.[24]

[23] These regressions were performed using STATA (version 11), and incorporate individual-subject random effects.

[24] The two variables of time dependence (round and round number≥ 6) are jointly highly significant for investors ($\chi^2 = 41.56$, $df = 2$, $p \approx 0.0000$) but are not jointly significant at conventional error levels for allocators ($\chi^2 = 2.95$, $df = 2$, $p \approx 0.2282$).

Table 3: Estimates from probit models with random effects (standard error in parentheses)

Model	Short Invest (SI)	Short Split (SS)	Long Invest (LI)	Long Split (LS)
Dependent variable	Invest	Split	Invest	Split
	$n = 1800$	$n = 994$	$n = 1800$	$n = 994$
cons	0.5898*** (0.1949)	−0.2549 (0.2764)	0.5908*** (0.1957)	−0.2555 (0.2789)
round number	−0.1343*** (0.0232)	−0.0604* (0.0365)	−0.1352*** (0.0232)	−0.0631* (0.0368)
round number≥ 6	0.4644** (0.1809)	0.4513 (0.2939)	0.4732*** (0.1812)	0.4782 (0.2979)
*side*10 (treatment)	0.5761** (0.2810)	1.1555*** (0.4103)	0.5459* (0.2968)	1.5216*** (0.4513)
*side*10*round	−0.0781*** (0.0284)	−0.2334*** (0.0527)	−0.0859*** (0.0318)	−0.3709*** (0.0821)
*side*210 (treatment)	0.2395 (0.2854)	0.7941* (0.4120)	0.2206 (0.2908)	0.7420* (0.4195)
*side*210*round	−0.1091*** (0.0298)	−0.2043*** (0.0534)	−0.0982*** (0.0334)	−0.1702*** (0.0590)
*side*3210 (treatment)	0.3882 (0.2886)	0.6432 (0.4036)	0.4022 (0.2936)	0.4674 (0.4100)
*side*3210*round	−0.1145*** (0.0302)	−0.1552*** (0.0518)	−0.1041*** (0.0331)	−0.0874 (0.0553)
1 (side payment) amount	−0.8620** (0.3862)	−0.6842 (0.6149)	−	−
1 amount*round	0.1495*** (0.0388)	0.1686** (0.0673)	−	−
2 (side payment) amount	−0.3863 (0.4368)	−1.9347*** (0.6718)	−0.3840 (0.4405)	−1.8484*** (0.6776)
2 amount*round	0.2139*** (0.0436)	0.1959*** (0.0725)	0.2041*** (0.0469)	0.1490*** (0.0757)
3 (side payment) amount	0.3294 (0.7417)	−0.3355 (1.1788)	0.3275 (0.7442)	−0.2203 (1.2004)
3 amount*round	0.2544*** (0.0686)	−0.0658 (0.1356)	0.2439*** (0.0699)	−0.1293 (0.1393)
1 amount**side*10			−0.4081 (0.4687)	−0.9155 (0.7243)
1 amount**side*10*round			0.1244*** (0.0473)	0.3026*** (0.0979)
1 amount**side*210			−1.0454 (0.6557)	−1.8061 (1.1045)
1 amount**side*210*round			0.1546** (0.0702)	0.2629** (0.1184)
1 amount**side*3210			−2.2849*** (0.8715)	2.3396 (2.1199)
1 amount**side*3210*round			0.2554*** (0.0871)	−0.2518 (0.2119)
$ln(L)$	−985.0633	−457.1316	−981.7991	−449.8214
$pseudo - R^2$	0.5881	0.1523	0.5895	0.1659
BIC	2075.0642	1010.8875	2098.5180	1023.8741
$Akaike$	1998.1266	942.2631	1999.5982	935.6428

$*(**, ***)$: Coefficient significant different from zero at the 10% (5%, 1%) level.

We estimate the partial effects of the offer amounts on participant choices. The partial effect of, for instance, a 1 offer amount rather than no offer in round t has the form $G(\beta_0 + \beta_{1\ amount} + \beta_{1\ amount*round} + \beta_3 x_3 + ... + \beta_k x_k) - G(\beta_0 + \beta_3 x_3 + ... + \beta_k x_k)$ with G being the normal *cdf*. Figure 8 reports the point estimates and 95 percent confidence intervals of the partial effects of the expression above (upper left panel), for each round and for both player types. The figure also shows the corresponding estimates for both the effect of a 2 offer amount versus no offer (upper center panel) and the effect of a 3 offer amount versus no offer (upper right panel). Figure 8 also shows the partial effects of a 2 offer amount versus a 1 offer amount (lower left panel), the effects of a 3 offer amount versus a 1 offer amount (lower middle panel), and the effects of a 3 offer amount versus a 2 offer amount (lower right panel).

The point estimates for the effect of a 3 amount versus a 0 amount are positive for investors. The corresponding confidence intervals are positive as well. The point and interval estimates for the effect of a 3 amount versus a 1 offer amount are positive. The point and interval estimates for the effect of a 2 amount versus a 1 offer amount are positive. The point and interval estimates for the effect of a 2 amount versus a 0 offer amount are positive. Hence, $Invest$ is significantly more likely

1) after a 3 offer amount than after a 0 offer amount,

2) after a 3 offer amount than after a 1 offer amount,

3) after a 2 offer amount than after a 1 offer amount, and

4) after a 2 offer amount than after a no offer.

These four findings are consistent with hypothesis 1. Moreover, the point estimate of the partial effect tends to grow over the length of the session. The differences are substantial and become stronger as participants gain experience.

We also see that $Invest$ is, initially, not significantly more likely after a 1 offer amount than after a 0 offer amount. However, the point estimate of the effect increases over time; it becomes significant after three rounds. To the extent that this effect is significant following either a 1 or a 0 offer amount, this finding is at odds with hypothesis 2, according to which frequencies of $Invest$ should be the same following either a 1 or 0 amount. On the other hand, the finding is consistent with hypothesis 4.

We also see that $Invest$ is significantly more likely after a 3 offer amount than after a 2 offer amount. However, the point estimate of the effect decreases slowly over time; it becomes significant in the final

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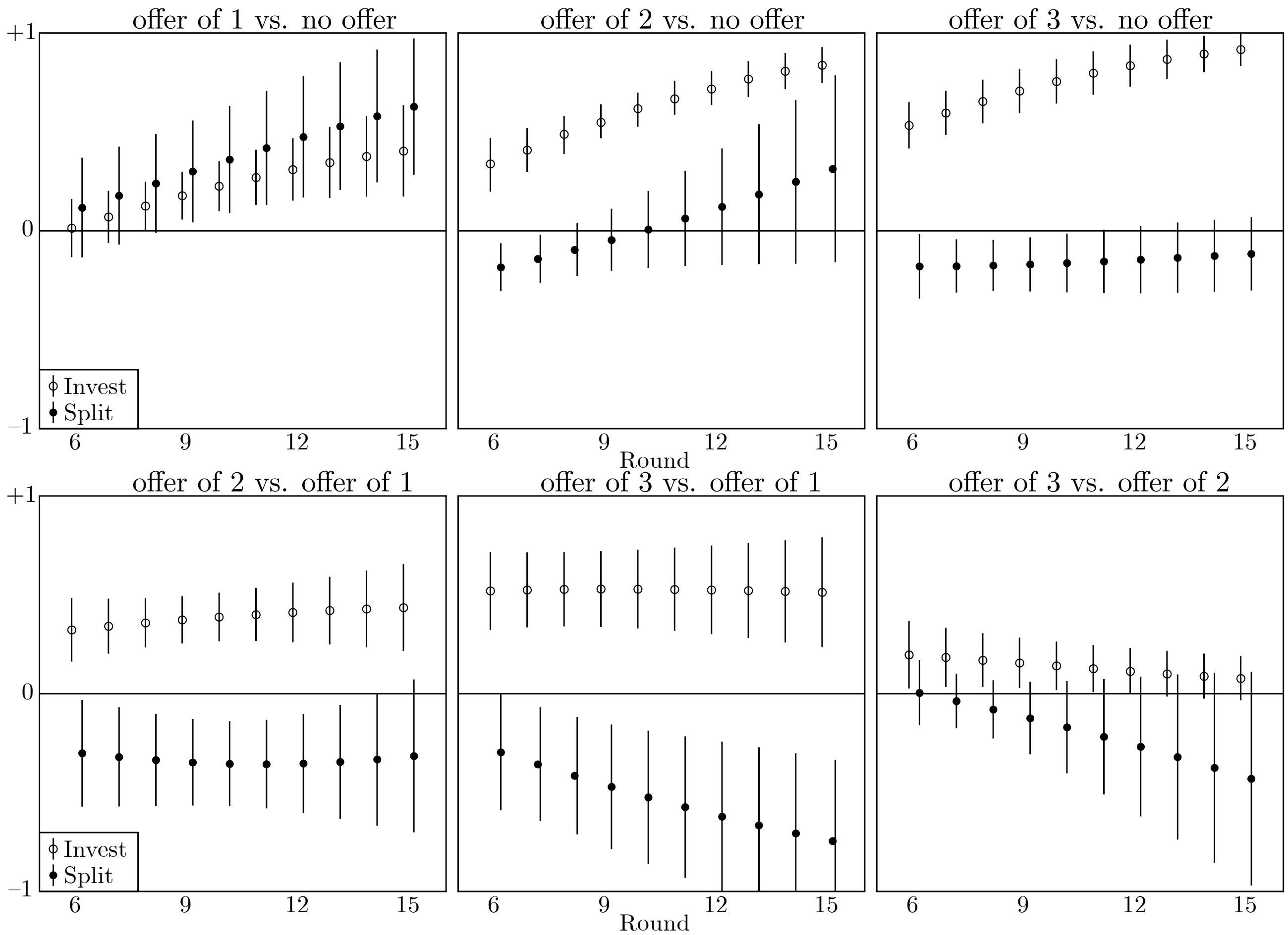
The two *side*10 variables (*side*10 and *side*10_*round*) are jointly significant for investors ($\chi^2 = 7.77$, $df = 2$, $p \approx 0.0229$) and jointly highly significant for allocators ($\chi^2 = 21.61$, $df = 2$, $p \approx 0.0000$). The two *side*210 variables (*side*210 and *side*210_*round*) are jointly highly significant for both investors ($\chi^2 = 17.20$, $df = 2$, $p \approx 0.0002$) and allocators ($\chi^2 = 15.65$, $df = 2$, $p \approx 0.0004$). The two *side*3210 variables (*side*3210 and *side*3210_*round*) are jointly highly significant for both investors ($\chi^2 = 16.57$, $df = 2$, $p \approx 0.0003$) and allocators ($\chi^2 = 9.39$, $df = 2$, $p \approx 0.0092$). The two *side*_1 variables (*side*_1 and *side*_1_*round*) are jointly highly significant for both investors ($\chi^2 = 29.48$, $df = 2, p \approx 0.0000$) and allocators ($\chi^2 = 12.49$, $df = 2$, $p \approx 0.0019$). The two *side*_2 variables (*side*_2 and *side*_2_*round*) are jointly highly significant for both investors ($\chi^2 = 128.64$, $df = 2, p \approx 0.0000$) and allocators ($\chi^2 = 8.57$, $df = 2$, $p \approx 0.0138$). The two *side*_3 variables (*side*_3 and *side*_3_*round*) are jointly highly significant for both investors ($\chi^2 = 186.33$, $df = 2, p \approx 0.0000$) and allocators ($\chi^2 = 5.27$, $df = 2$, $p \approx 0.0716$).

Figure 8: Partial effects of the offer amounts versus no offer on choices; circles represent point estimates; segments represent 95% confidence intervals.



three rounds. To the extent that this effect is significant following either a 3 or a 2 offer amount, this finding is consistent with hypothesis 5, according to which frequencies of $Invest$ should be lower following an amount of 2 than following an amount of 3.

In Figure 8, we do see large differences in point and interval estimates of the effects of the offer amounts on split choices for allocators. The point estimates for the effect of an offer amount of 3 versus an offer amount of 0 are negative, but the effect, while significant at first, becomes insignificant as participants gain experience. Similarly, the point estimates for the effect of an offer amount of 2 versus an offer amount of 1 are negative, but the effect, while initially significant, becomes insignificant. The effects of an offer amount of 3 versus an offer amount of 2 are insignificant. Similarly, the effects of a 2 offer amount versus a 0 offer amount are insignificant with the exception of the first two rounds. In contrast, the point estimates of the effect of an offer amount of 3 versus an offer amount of 1 are negative and significant. Furthermore, the effect increases over the length of the session and becomes substantial. In contrast, the point estimates of the effect of an offer amount of 1 versus an offer amount of 0 are positive and significant (with the exception of the first three rounds). Furthermore, the effect increases as participants gain experience and becomes substantial. These findings, taken together, are inconsistent with Hypothesis 3.

Note also that the larger the difference in the offer amount, the smaller the partial effects on allocator choice. For instance, the difference between offer amount 1 and offer amount 0 is small, and the partial effect on allocator choice is positive; the difference between offer amount 3 and offer amount 0 is large, and the partial effect on allocator choice is negative.

We move to the partial effects of the treatments on participant choices. The partial effect of, for instance, the $side10$ treatment rather than the $side0$ control treatment in round t has the form $G(\beta_0 + \beta_{side10} + \beta_{side10*round} + \beta_3 x_3 + ... + \beta_k x_k) - G(\beta_0 + \beta_3 x_3 + ... + \beta_k x_k)$ with G being the normal cdf. Figure 9 shows, for rounds $6 - 15$ and for both investors and allocators, the estimation results for each of the non-control treatments (upper panels). Figure 9 also allows us to make pairwise comparisons between treatments (lower panels).[25] The six panels show point estimates and 95 percent confidence intervals for the effects. We see little evidence of crowding out: the point estimates for the effects of each non-control treatment on allocator choices are negative, but the effects are small and insignificant; the corresponding estimates for the effect on investor choices are negative (with the exception in the first two rounds of the $side10$ treatments), but the effects are small and usually insignificant. In addition, we fail to find substantial and significant differences in cooperation frequencies between the non-control treatments (with the exception of the first five rounds in the comparison between the $side210$ and $side10$ treatments). Crowding out, as we defined it, requires that cooperation frequencies should be lower in the control treatment not only following no offer, but overall– including a 1 offer amount as well. Figure 10 reports this overall partial effect on frequencies, combining the effect of the treatment, the joint effect of the treatment and a 1 offer amount, and the observed frequency of the 1 offer amount in the treatment.[26] The overall partial effect of, for instance, the $side10$ treatment rather than the $side0$ control treatment in round t has the form $G(\beta_0 + \beta_{side10} + \beta_{side10*round} + p(1|side10) * \beta_{1 \text{ offer amount}*side10} + p(1|side10) * \beta_{1 \text{ offer amount}*side10*round} + \beta_5 x_5 + ... + \beta_k x_k) - G(\beta_0 + \beta_5 x_5 + ... + \beta_k x_k)$ with G being the normal cdf. Figure 10 shows, for rounds

[25] The estimation of the effects is based on model specification Short Invest (SI) and Short Split (SS).

[26] The estimation of the effects is based on model specification Long Invest (LI) and Long Split (LS).

Figure 9: Partial effects of each of three treatments versus the control treatment on choices (upper panels) and pairwise comparison of non-control treatments (lower panels); circles represent point estimates; segments represent 95% confidence intervals.

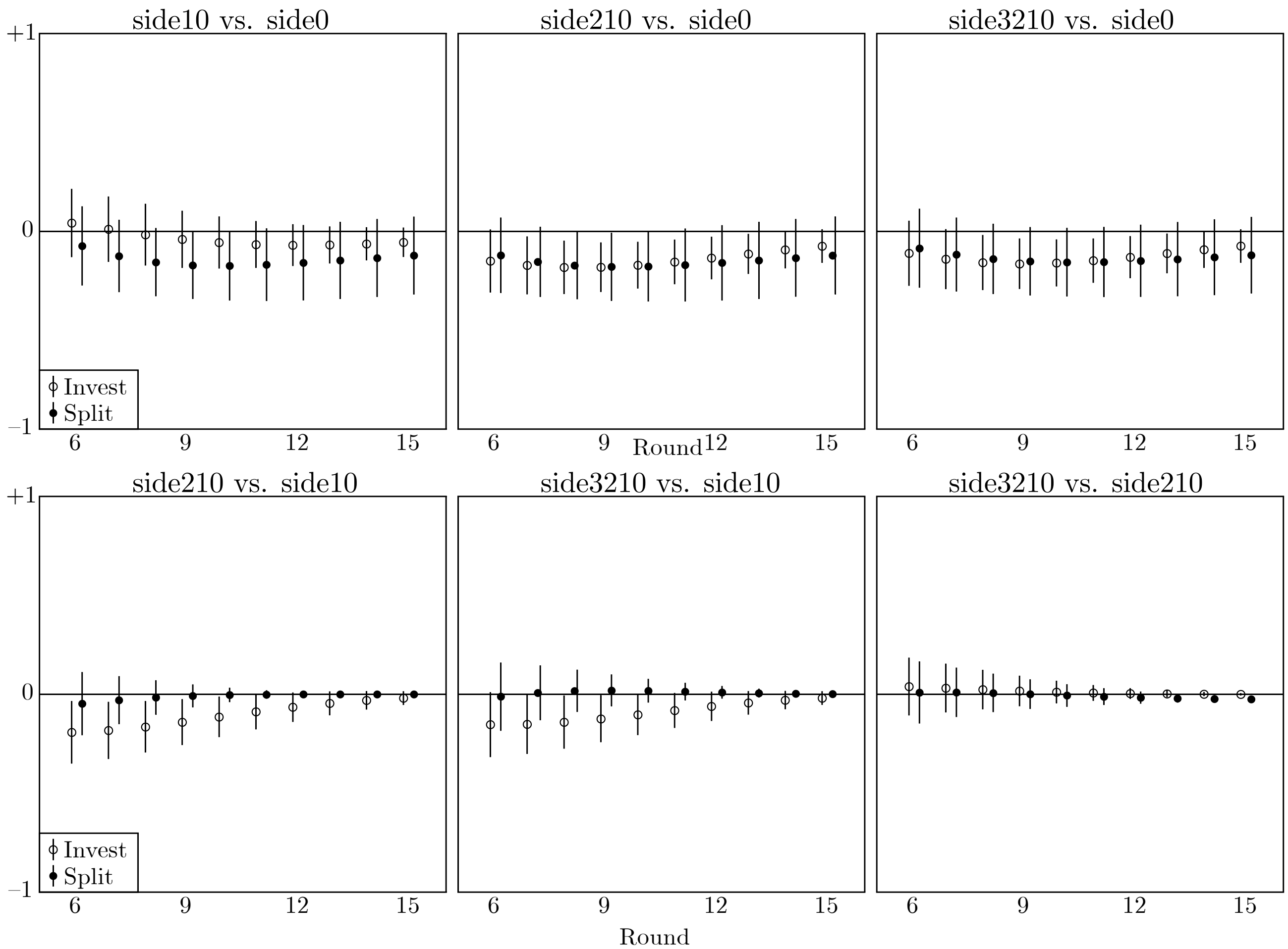


Figure 10: Is there crowding out? Overall partial effects of each treatment on choices following an offer of 1 or 0; circles represent point estimates; segments represent 95% confidence intervals.

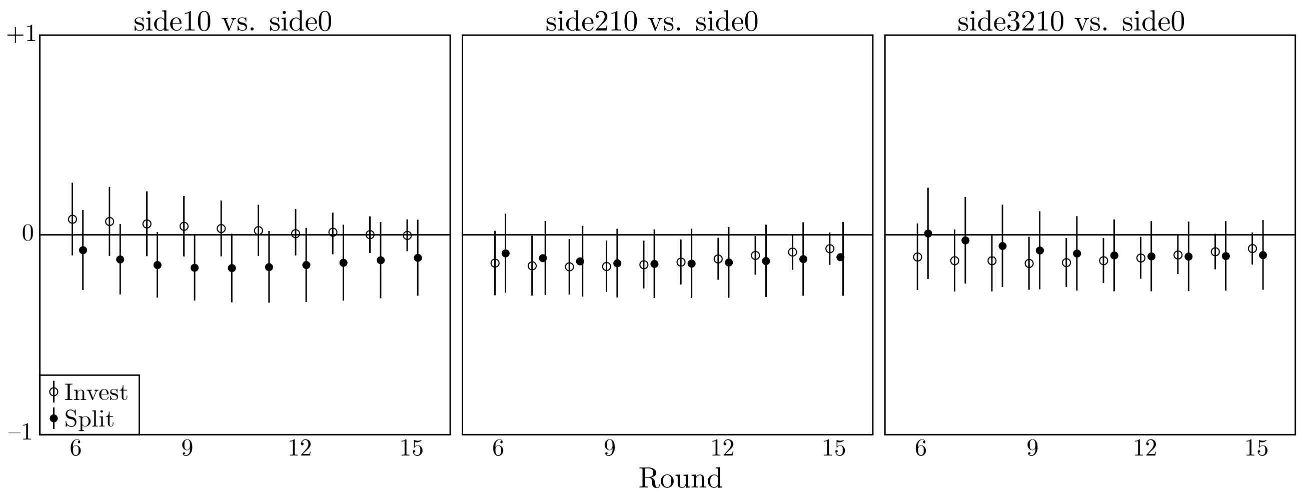
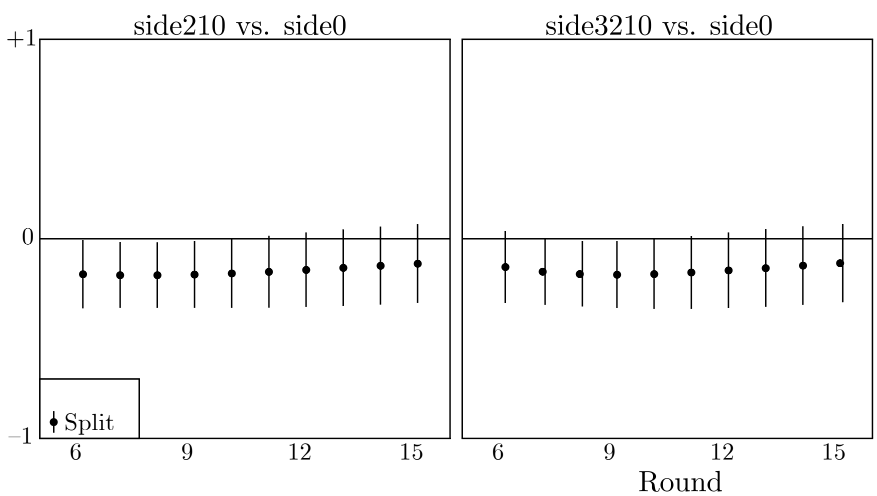


Figure 11: Is there crowding out? Overall partial effects of the side210 treatment on allocator choices following an offer of 2 or 0 (left panel) and overall partial effects of the side3210 treatment on choices following an offer of 3 or 0 (right panel); circles represent point estimates; segments represent 95% confidence intervals.



$6-15$ and for both investors and allocators, the estimation results of this overall effect of each of the non-control treatments on choices. We fail to find substantial and systematic differences for the treatments for investors (with the exception of five rounds of the $side210$ treatment in which the predicted effect is significantly negative). These results cast doubt on our crowding-out hypothesis 6. We do find systematic differences between the treatments for allocators as the point estimates are negative; however, the effects are not significantly different from zero. These results cast some doubt on our crowding-out hypothesis 7.

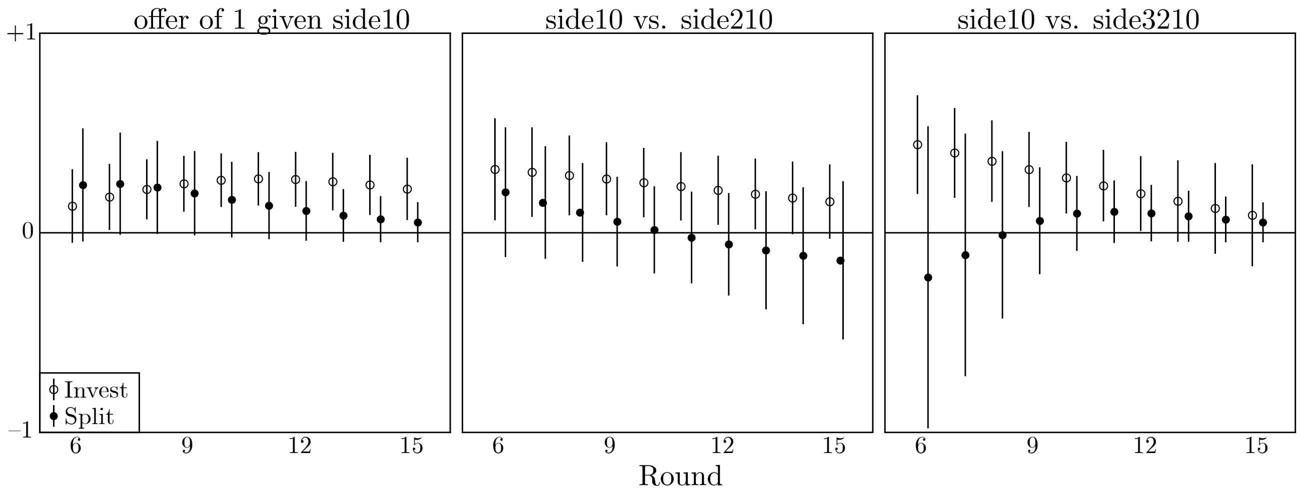
We now test hypothesis 8. Figure 11 reports the overall partial effect on Split frequencies, combining the effect of the treatment, the joint effect of the treatment and a 2 (3) offer amount, and the observed frequency of the 2 (3) offer amount in the treatment.[27] Figure 11 shows, for rounds $6-15$ and for allocators, the estimation results of this overall effect of each of the non-control treatments on choices. We do find systematic differences between the treatments for allocators as the point estimates are negative however the effects are significantly different from zero in only five instances. These results cast some doubt on our crowding-out hypothesis 8.

We turn to our signaling hypotheses. Recall that these hypotheses involve interaction between the 1 offer amount and the $side10$ treatment. We look at the effect of a 1 offer amount versus no offer, conditional on the $side10$ treatment, and at the effect of the $side10$ treatment versus another non-control treatment, conditional on a 1 offer amount. The partial effect of, for instance, a 1 offer amount rather than no offer in round t in the $side10$ treatment, has the form $G(\beta_0+\beta_{1\text{ offer amount}*side10}+\beta_{1\text{ offer amount}*side10*round}+...+\beta_k x_k)-G(\beta_0+\beta_3 x_3+...+\beta_k x_k)$ with G being the normal *cdf*. The effect of the $side10$ versus the $side210$ treatment, conditional on a 1 offer amount, has the form $G(\beta_0+\beta_{1\text{ offer amount}*side10}+\beta_{1\text{ offer amount}*side10*round}+\beta_{side10}+\beta_{side10*round}+...+\beta_k x_k)-G(\beta_0+\beta_{1\text{ offer amount}*side210}+\beta_{1\text{ offer amount}*side210*round}+\beta_{side210}+\beta_{side210*round}+...+\beta_k x_k)$. Figure 11 shows the estimation results.[28] The panel to the left shows the effect of a 1 offer amount, conditional on the $side10$ treatment. The effect is initially insignificant but turns positive, substantial, and significant for both investors and allocators (in this treatment, participants are more likely to choose cooperation after a 1 offer amount than after a 0 offer amount). The remaining panels tell a different story: there is no persistent increase in the frequency of allocator choices following 1 offer amounts in the $side10$ rather than in the $side210$ or the $side3210$ treatment. For investors, there is a significant and substantial increase in invest frequencies in early rounds, but this effect declines over the length of the session, becoming insignificant by the end of the session. We conclude that the evidence for signaling is mixed. There is little indication of signaling by allocators, but a belief by investors that allocators are signaling (when they are not).

[27] The overall partial effect of, for instance, the $side210$ treatment rather than the $side0$ treatment in round t has the form $G(\beta_0+\beta_{side210}+\beta_{side210*round}+p(2|side210)*\beta_{2\text{ offer amount}*side210}+p(2|side210)*\beta_{2\text{ offer amount}*side210*round}+\beta_5 x_5+...+\beta_k x_k)-G(\beta_0+\beta_5 x_5+...+\beta_k x_k)$ with G being the normal *cdf*. The estimation of the effects is based on a model specification similar to LI and LS, replacing the 2 offer amount variable by the 1 offer amount variable, the 2 amount*round variable by the 1 amount*round variable, the 1 amount $*side10$ by the 2 amount$*side210$ variable, and the 1 $amount*side10*round$ variable by the 2 $amount*side210*round$ variable.

[28] The estimation of the effects is based on model specification Long Invest (LI) and Long Split (LS).

Figure 12: Is there signaling? Partial effects of selected variables on choices; circles represent point estimates; segments represent 95% confidence intervals.



5.3 Discussion

We know of a few experimental studies which evaluate the performance of the compensation mechanism. Our study is the first investigation of the mechanism in a trust game framework. This is a much simpler game than the one used in previous studies, but it is still a difficult test case: In our setting, there are two decision variables for allocators, one decision variable for investors, and backward induction of both allocators and investors is required. In addition, the multiplicity of equilibria raises a coordination problem.

We assess the overall performance of the mechanism. Then, we isolate causes for the mechanism's success and failure by revisiting our series of experimental games.

We find that our mechanism is quite successful, and its performance compares favorably with the experimental results for other efficiency-inducing mechanisms. We saw that 67.00% of the time allocator participants made offers that should induce cooperation when efficient offers were feasible. Nevertheless, this offer rate is well below 100%. Furthermore, 77.67% of the time investor participants responded with cooperation when receiving an efficient offer. Consequently, 62.5% of the time participants managed to achieve the efficient outcome when that was possible. In contrast, 44.83% of the time participants managed to achieve the efficient outcome even when it was not possible. In the final round, 68.33% of the time participants managed to achieve the efficient outcome when it was possible, and only 30.00% of the time participants managed to achieve the efficient outcome when it was not possible. Thus, the overall effect is significant, substantial, and sustainable.

We interpret these numbers to show support for the contention that if individuals adjust their behavior to that of the other player in the game, searching for choices that give them better results, and if they are able to gain experience in a simple enough environment, then the compensation mechanism works well. Chen and Gazzale (2004) look carefully at the dynamic properties of a generalized version of the compensation mechanism. Their game is taken from the literature on externalities and it is a bit more complicated than ours. They find that supermodular games converge significantly better than those well below the threshold for supermodularity. Supermodularity is a technical property of games that ensures convergence to equilibrium under various learning dynamics.

Andreoni and Varian (1999) assess the compensation mechanism in a prisoner's dilemma. In their experiment, participants first play a prisoner's dilemma, then a modified version. In the modified version, prior to play, each player chooses how much to offer her opponent in exchange for cooperating, and then each player is told what she has been offered to cooperate. This mechanism changes the equilibrium to one in which both players cooperate. Andreoni and Varian do indeed find more cooperation under this mechanism, but the increase was much smaller than predicted: cooperation was higher than predicted without the mechanism, but much lower than predicted with the mechanism. The overall frequency of cooperation is 22.9% without and 54.5% with the incentive scheme. Charness et al (2007) assess the mechanism in a series of prisoner's dilemma. They confirm Andreoni and Varian's result: overall cooperation rates are between 10.8% and 17.5% without and 42.9% and 68.1% with the incentive scheme. Both prisoner's dilemma studies find no evidence of learning; both suggest that individuals are motivated by fairness.

In our setting, we saw evidence of the presence of investor fairness motives as well (Hypothesis 5; Table 2, Figure 6 (middle panel), Figure 8 (bottom right panel)). Some investor participants seem to be averse to extremely unequal payoffs; this aversion might have lead to more frequent cooperation when the offer amount is 3 than when the offer amount is 2. However, it seems that this aversion goes away as participants gain experience (Figure 8, bottom right). Nevertheless, this phenomenon has a negative overall impact on the effectiveness of the mechanism.

Bracht et al (2008) find that the compensation mechanism performs very poorly in a public-goods setting. They find very little evidence of learning. They conjecture that the mechanism is ineffective because it undermines intrinsic motivation for cooperation, and provides only weak incentives as actual subsidy rates are indeed well below the efficient level.

In our setting, the offer amounts in both the $side210$ treatment and the $side3210$ treatment are initially much lower than predicted. We found some evidence of systematic crowding out but we were not sure whether the effects are reliable (Figure 10, middle and right panels). In any case, this phenomenon seems to have a small negative impact on the effectiveness of the mechanism.[29]

We also considered signaling theory, according to which a choice by the allocator of the largest possible offer amount is a signal that the allocator intends to split the proceeds of investment– even if this amount does not change the allocaotor's best response after investment from keeping to splitting. In treatment $side10$, we found strong support for investors' perceptions of signaling, and for allocators actually signaling (Table 2, Figure 8 (top left panel) and Figure 12). However, allocators do not choose the offer amout of 1 frequently enough to allow for the perception of signaling (Table 2 and Figure 5 (left panel)), and whenever allocators choose the offer amount of 0, investors are more reluctant to invest than in the $side0$ treatment (Figure 4 (left panel) and Figure 6 (left panel)). Hence, the net effect of the $side10$ treatment versus the control treatment $side0$ is positive but not significant.

[29] This finding is not surprising as the crowding out effect does usually not emerge when there is no personal relationship between participants. See Frey (1993) or Bartling, Fehr, and Schmidt (2010).

6 Conclusion

We considered the compensation mechanism that was applied to the trust game. We saw that the mechanism is largely successful at implementing efficient outcomes: when offer amounts of side payments that should induce investment are possible, allocator participants make such efficient offers 67% of the time; when efficient offers are received, investor participants respond with cooperation nearly 78% of the time; participants manage to achieve an efficient outcome– when this is possible – 63% of the time.

We compared the mechanism's performance in our simple game with the performance in more complex games reported in previous studies. We conclude that the compensation mechanism is successful if the setting is simple; indeed, our participants were able to exploit the option to encourage the other player to play cooperatively, even though they had only a few rounds to gain experience. Nevertheless, even in our environment– possibly the simplest setting in which to implement the mechanism in an experiment - the mechanism is not 100% successful. We designed three treatment, $side3210$, $side210$, and $side10$, to find out what makes the mechanism successful and what makes the mechanism unsuccessful.

In our $side3210$ treatment with $S = \{3, 2, 1, 0\}$, efficient offer amounts of side payments, $s = 3$ and $s = 2$, were possible. Initially, offer amounts were very low. However, allocator participants slowly learned to make efficient offers. We conclude that allocator participants' learning to make efficient offer amounts made the mechanism successful.

In our $side210$ treatment with $S = \{2, 1, 0\}$, initial offers were very low, but allocator participants slowly learned to make the efficient offer, $s = 2$. However, even when allocator participants made efficient offers, investor fairness concerns might have led some investor participants to choose non-cooperation. Furthermore, when offers were too low to make cooperation rational, a natural tendency to cooperate was repressed by the introduction of weak incentives. These factors distort the incentives presented by the mechanism; the overall effect is that the $side210$ treatment is not effective. This finding is surprising as we found strong evidence of investor perception of efforts of allocator participants to signal cooperative intentions in all treatments.

In our $side10$ treatment with $S = \{1, 0\}$, only inefficient offer amounts were possible. Once again, we found strong evidence of investor perception of signaling, and, in addition, of allocator signaling. Nevertheless, these behavioral tendencies while strong, are not strong enough to assure effectiveness of the mechanism when efficient offers are not possible.

We conjecture that a treatment that allows an offer amount of 4 (and an equal split of the returns following this offer) could highlight allocator discomfort with unequal equilibrium payoffs in our $side210$ and $side3210$ treatments. This could be an interesting direction for future research.[30]

[30] Estelle Midler suggested this modification for future research.

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This appendix contains the instructions for the *side0* treatment, the *side10* treatment, the *side210* treatment, and the *side3210* treatment. The instructions for the first half of the treatment are the same.

Instructions—first half of experiment—all treatments

You are about to participate in an experiment in the economics of decision-making. If you follow these instructions carefully and make good decisions whenever possible, you might earn a considerable amount of money that will be paid to you in cash. If you have a question at any time, please feel free to ask me. We ask that you not talk with the other participants during the experiment.

This experimental session is made up of 2 halves. The first half lasts for 5 rounds, and the second half lasts for 10 rounds. Each round in the first half consists of one play of a simple investment game, which is described below. Each round of the second half consists of one play of a more complicated investment game, which will be described after the first half has ended.

The investment games are played between 2 players, called **Investor** and **Allocator**. Before the first round begins, each of the participants is randomly assigned one of these roles; half will be **Investors** and half will be **Allocators**. Participants will remain in the same role throughout the experimental session.

In each round, you will be randomly matched to a player of the opposite role. You will not be told the identity of the person you are matched with in any round, nor will they be told your identity—even after the end of the session.

The **sequence of play** in a round is as follows:

1. The **Investor** has 2 points and chooses whether to Invest or Not Invest them.
2. If the **Investor** chooses Not Invest, the round ends, and the **Allocator** has no decision to make. If the **Investor** chooses Invest, then the investment is successful, yielding 8 points. The **Allocator** chooses whether to Split these 8 points or Keep them; after this, the round ends.

Scoring:

- If the **Investor** chooses Not Invest, then the **Investor** earns 2 points and the **Allocator** earns 0 points.
- If the **Investor** chooses Invest, and the **Allocator** chooses Split, then the **Investor** earns 4 points and the **Allocator** earns 4 points.
- If the **Investor** chooses Invest, and the **Allocator** chooses Keep, then the **Investor** earns 0 points and the **Allocator** earns 8 points.

So, if you are an **Investor**, your score in each round will depend on your choice and, in some cases, the choice of the person you are matched with. If you are an **Allocator**, your score in each round will depend on the choice of the person you are matched with, and, in some cases, your choice.

Payments: At the end of the experimental session, two rounds are chosen randomly: one from the first half and one from the second half. Each participant receives, in Pounds, the total number of points he/she earned in those two rounds. Each participant additionally receives £5 for completing the session. Payments are made in cash at the end of the session.

Instructions—second half of experiment—side0

The procedure in this half of the experiment is very similar to that in the first half. Your role will be the same as in the first half. In each round, you will be randomly matched to a player of the other role. The only difference is that you are now playing 10 rounds rather than 5. The investment game itself is the same as before.

The **sequence of play** in a round is now as follows:

1. The **Investor** has 2 points and chooses whether to Invest or Not Invest them.
2. If the **Investor** chooses Not Invest, the round ends, and the **Allocator** has no decision to make. If the **Investor** chooses Invest, then the investment is successful, yielding 8 points. The **Allocator** chooses whether to Split these 8 points or Keep them; after this, the round ends.

Scoring:

- If the **Investor** chooses Not Invest, then the **Investor** earns 2 points and the **Allocator** earns 0 points.
- If the **Investor** chooses Invest, and the **Allocator** chooses Split, then the **Investor** earns 4 points and the **Allocator** earns 4 points.
- If the **Investor** chooses Invest, and the **Allocator** chooses Keep, then the **Investor** earns 0 points and the **Allocator** earns 8 points.

Instructions—second half of experiment—side10

The procedure in this half of the experiment is very similar to that in the first half. Your role will be the same as in the first half. In each round, you will be randomly matched to a player of the other role. The only difference is that the investment game has an additional stage. Before the **Investor** makes a choice, the **Allocator** can offer a side payment of 1 point to the Investor to encourage him/her to Invest.

Rules of the side payment:

- If the **Investor** chooses Invest, the **Allocator** makes a payment.
- If the **Investor** chooses Not Invest, the **Allocator** does not make a payment to the Investor.
- Making an offer of the side payment is voluntary, but if the **Investor** chooses Invest, paying the side payment is required.

The **sequence of play** in a round is now as follows:

1. The **Allocator** chooses whether to Offer Side Payment or Do Not Offer Side Payment. The **Investor** sees whether the **Allocator** has chosen Offer Side Payment or Do Not Offer Side Payment, then chooses whether to Invest or Not Invest the 2 points.
2. If the **Investor** chooses Not Invest, the round ends, and the **Allocator** has no further decision to make. If the **Investor** chooses Invest, then the investment is successful, yielding 8 points. The **Allocator** chooses whether to Split these 8 points or Keep them.
3. If the **Allocator** chose Offer Side Payment, and the **Investor** chose Invest, the side payment is made from the **Allocator** to the **Investor**. If the **Allocator** chose Do Not Offer Side Payment, or if the **Investor** chose Not Invest, no side payment is made.

Scoring:

- If the **Investor** chooses Not Invest, then the **Investor** earns 2 points and the **Allocator** earns 0 points (regardless of whether the Allocator chose Offer Side Payment or Do Not Offer Side Payment).
- If the **Investor** chooses Invest, and the **Allocator** chooses Keep, then
    - the **Investor** earns 0 points if the **Allocator** chose Do Not Offer Side Payment,
    - or 1 point if the **Allocator** chose Offer Side Payment;
    - and the **Allocator** earns 8 points if he/she chose Do Not Offer Side Payment,
    - or 7 points if he/she chose Offer Side Payment.
- If the **Investor** chooses Invest, and the **Allocator** chooses Split, then
    - the **Investor** earns 4 points if the **Allocator** chose Do Not Offer Side Payment,
    - or 5 points if the **Allocator** chose Offer Side Payment;
    - and the **Allocator** earns 4 points if he/she chose Do Not Offer Side Payment,
    - or 3 points if he/she chose Offer Side Payment.

Instructions—second half of experiment—side210

The procedure in this half of the experiment is very similar to that in the first half. Your role will be the same as in the first half. In each round, you will be randomly matched to a player of the other role. The only difference is that the investment game has an additional stage. Before the **Investor** makes a choice, the **Allocator** can offer a **side payment of 1 point** or a **side payment of 2 points** to the Investor to encourage him to Invest.

Rules of the side payment:

- If the Investor chooses Invest, the Allocator makes a payment.
- If the Investor chooses Not Invest, the Allocator does not make a payment to the Investor.
- Making an offer of the side payment is voluntary, but if the Investor chooses Invest, paying the side payment is required.

The **sequence of play** in a round is now as follows.

1. The **Allocator** chooses whether to Offer Side Payment or Do Not Offer Side Payment. The **Investor** sees whether the **Allocator** has chosen Offer Side Payment or No Offer Side Payment, then chooses whether to Invest or Not Invest the 2 points.
2. If the **Investor** chooses Not Invest, the round ends, and the **Allocator** has no further decision to make. If the **Investor** chooses Invest, then the investment is successful, yielding 8 points. The **Allocator** chooses whether to Split these 8 points or Keep them.
3. If the **Allocator** chose Offer Side Payment, and the **Investor** chose Invest, the side payment is made from the **Allocator** to the **Investor**. If the **Allocator** chose Do Not Offer Side Payment, or if the **Investor** chose Not Invest, no side payment is made.

Scoring:

- If the **Investor** chooses Not Invest, then the **Investor** earns 2 points and the **Allocator** earns 0 points (regardless of whether the Allocator chose Offer Side Payment or Do Not Offer Side Payment).
- If the **Investor** chooses Invest, and the **Allocator** chooses Keep, then
    - the **Investor** earns 0 points if the **Allocator** chose Do Not Offer Side Payment,
    - or 1 point if the **Allocator** chose Offer Side Payment 1,
    - or 2 points if the **Allocator** chose Offer Side Payment 2;
    - and the **Allocator** earns 8 points if he/she chose Do Not Offer Side Payment,
    - or 7 points if he/she chose Offer Side Payment 1,
    - or 6 points if he/she chose Offer Side Payment 2.
- If the **Investor** chooses Invest, and the **Allocator** chooses Split,
    - then the **Investor** earns 4 points if the **Allocator** chose Do Not Offer Side Payment,
    - or 5 points if the **Allocator** chose Offer Side Payment 1,
    - or 6 points if the **Allocator** chose Offer Side Payment 2;
    - and the **Allocator** earns 4 points if he/she chose Do Not Offer Side Payment,
    - or 3 points if he/she chose Offer Side Payment 1,
    - or 2 points if he/she chose Offer Side Payment 2.

Instructions—second half of experiment—side3210

The procedure in this half of the experiment is very similar to that in the first half. Your role will be the same as in the first half. In each round, you will be randomly matched to a player of the other role. The only difference is that the investment game has an additional stage. Before the **Investor** makes a choice, the **Allocator** can offer a **side payment of 1 point, of 2 points, or of 3 points** to the Investor to encourage him/her to Invest.

Rules of the side payment:

- If the Investor chooses Not Invest, the Allocator does not make a payment to the Investor.
- If the Investor chooses Invest, the Allocator makes a payment.
- Making an offer of the side payment is voluntary, but if the Investor chooses Invest, paying the side payment is required.

The **sequence of play** in a round is now as follows.

1. The **Allocator** chooses whether to Offer Side Payment or Do Not Offer Side Payment. The **Investor** sees whether the **Allocator** has chosen Offer Side Payment or Do Not Offer Side Payment, then chooses whether to Invest or Not Invest the 2 points.
2. If the **Investor** chooses Not Invest, the round ends, and the **Allocator** has no further decision to make. If the **Investor** chooses Invest, then the investment is successful, yielding 8 points. The **Allocator** chooses whether to Split these 8 points or Keep them.
3. If the **Allocator** chose Offer Side Payment, and the **Investor** chose Invest, the side payment is made from the **Allocator** to the **Investor**. If the **Allocator** chose Do Not Offer Side Payment, or if the **Investor** chose Not Invest, no side payment is made.

Scoring:

- If the **Investor** chooses Not Invest, then the **Investor** earns 2 points and the **Allocator** earns 0 points (regardless of whether the Allocator chose Offer Side Payment or Do Not Offer Side Payment).
- If the **Investor** chooses Invest, and the **Allocator** chooses Keep, then
  - the **Investor** earns 0 points if the **Allocator** chose Do Not Offer Side Payment,
  - or 1 point if the **Allocator** chose Offer Side Payment 1,
  - or 2 points if the **Allocator** chose Offer Side Payment 2,
  - or 3 points of the **Allocator** chose Offer Side Payment 3;
  - and the **Allocator** earns 8 points if he/she chose Do Not Offer Side Payment,
  - or 7 points if he/she chose Offer Side Payment 1,
  - or 6 points if he/she chose Offer Side Payment 2,
  - or 5 points if he/she chose Offer Side Payment 3..
- If the **Investor** chooses Invest, and the **Allocator** chooses Split,
  - then the **Investor** earns 4 points if the **Allocator** chose Do Not Offer Side Payment,
  - or 5 points if the **Allocator** chose Offer Side Payment 1,
  - or 6 points if the **Allocator** chose Offer Side Payment 2
  - or 7 points if the **Allocator** chose Offer Side Payment 3;
  - and the **Allocator** earns 4 points if he/she chose Do Not Offer Side Payment,
  - or 3 points if he/she chose Offer side Payment 1,
  - or 2 points if he/she chose Offer side Payment 2,
  - or 1 points if he/she chose Offer side Payment 3.