The Role of Incentives in Dynamic Favour Exchange: An Experimental Investigation[☆]

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Abstract

We study the role of direct and indirect incentives, as well as accounting/feedback systems, in a system of dynamic favour exchange. Subjects are placed in small groups and each period are further matched in subgroups of two where one player (the sender) exerts costly effort, which generates a benefit for his/her match (the receiver). That is, she does a favour. Senders exert substantial costly effort on behalf of receivers, even when no direct incentives are in place. However we show that when direct monetary incentives are in place (receivers pay senders for their effort in points, and points are converted to money at the end of the experiment), it leads to higher average efficiency and less variability of behavior. In the absence of direct incentives, with or without a public accounting system, group behavior is markedly different between those groups that score highly on the social value orientation (SVO) and those that do not. High SVO groups are able to meet or exceed the efficiency level achieved under direct incentives. When direct incentives are present, there is no difference between high and low SVO groups; instead play converges to the subgame perfect Nash equilibrium based on selfish preferences.

Keywords: Favour exchange, reciprocity, incentives

1. Introduction

Monetary compensation for products and services has been around for thousands of years. However, even in modern economies, there are many instances in which people eschew monetary compensation. For example, within families, among friends and neighbors, and within work organizations, it is common to do

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favours without direct monetary compensation. In between pure favour exchange and money that makes part of a global currency system, systems of local currencies have emerged over the past decades (Michel and Hudon, 2015). Their introduction seems to be driven by a belief in the positive effect of money on welfare (i.e., as a coordinating device to facilitate exchange), and the hope that introducing local money can maintain such exchanges, but keep the benefits with the local community. Recently, there also exist groups that eschew any kind of money, and rely exclusively on "gifts" or favour exchange. One such example is the *Buy Nothing Project*, in which people from local communities form groups whose members make offers to provide goods/services, or make requests to receive goods/services. The rules of the project explicitly state, "[w]e do not permit trading, bartering, buying or selling within our groups. Keep in mind that all gifts here must be given without any strings attached, and without any expectation of reward other than the joy of giving."[1] That is, the rules forbid direct, contemporaneous trades of "this for that", but within the group the same person may be on both the giving and receiving side of different "gifts" at different points in time. Thus, it seems fair to claim that parts of society seek out systems that focus on serving the needs of its citizens, while looking beyond fiat currency as the medium of exchange, and some eschewing monetary exchange of any kind.

Motivated by these developments as well as literature on coordination devices, indirect reciprocity, and other regarding preferences which we summarize in Section 2, this paper provides new experimental insights on the behavioural effects of direct monetary and indirect non-monetary incentives on exchange economies. Absent any kind of monetary system, the parties must rely on favours to trade. This forms the basis of our Control treatment. Payments at the end of the experiment equal total benefits from favours minus total cost of providing favours. At the other extreme is an economy with a fully-fledged experimental currency, in which subjects directly and immediately pay for the effort – at a fixed price per unit – exerted on their behalf by other subjects. In this treatment benefits from receiving favours minus costs of providing favours, as well as their net experimental currency payments received or deducted from doing or receiving favors are fully converted to the local currency where the experiment took place (in our case, Euro) at the end of the experiment. In this case, there are no favours because the receiver immediately pays for the good or service with the experimental currency. This is the basis of our Monetary treatment, which we say has *direct* incentives.

In between these two extremes is an economy where the experimental currency is reduced to a pure accounting device. Similar to Local Exchange Trading Systems (LETS) or Time Banks, the effort for providing a favour is translated into "compensation" on a record-keeping ledger available to all participants in the system. To emphasize this accounting role in our experiments, this "compensation" has no effect on the monetary payoffs of the participants; that is, it is **not** convertible to Euro at the end of the experiment.

[1] See `https://www.buynothingproject.org/the-fine-print-2/#Discussions` (accessed on 27 September 2018).

Instead, as in the Control, earnings are solely based on the benefit from favours received and the cost of favours given. This is the basis of our Effort Accounting Treatment, which we say has *indirect* incentives.

Except for the possible presence and scope of "money", all three economies are otherwise identical. That is, the cost of providing a service and value from receiving a service are identical across treatments. The three economies have the same social welfare maximizing amount of favours, which generates strictly more welfare than the subgame perfect Nash equilibrium (SPNE) based on selfish preferences of the underlying games. As we show, the SPNE in the Monetary treatment generates strictly positive welfare, while the SPNE in the Control treatment and the Effort Accounting treatment with indirect incentives generates zero welfare.

Our first question is to ask which of these institutions is most efficient, and whether direct incentives (i.e., money) are necessary to promote beneficial exchange. As we show in our experiment, money does promote exchange and acts as a strong coordination device. However, an important result is to show that dynamic favour exchange can also be sustained with indirect incentives.

The second question is, what role does the record-keeping function of a traditional LETS system have on favour exchange? A natural hypothesis is that since the record-keeping system acts as a kind of reputation mechanism, it will promote greater favour exchange than the Control treatment, which lacks both direct monetary incentives and record-keeping (see, e.g., Kocherlakota (1998) for an argument on the memory function of money, and Bolton et al. (2005) for experimental evidence on the role of reputation in image scoring games).[2] Our second result is that this hypothesis turns out to be false. Absent monetary incentives, whether or not such public record-keeping is present, there is no difference in dynamic favour exchange (in terms of effort and earnings).[3]

One of the underlying motives for alternative currencies or record keeping systems is the idea that they are beneficial to cohesive groups. In the absence of any money to facilitate exchange, if Mary does a favour today for Jane, Mary needs to trust that she will be given a favour by either Jane herself or some other member of the community later. When group size increases, the required trust increases. With a LETS or complementary currency system (CCS), group members need only to trust that there will be opportunities in the future to get a favour in return for earned currency units. The level of required trust is thus diminished, and the expectation is that the alternative currency increases the willingness of group participants to do each other a favour.

Although we did not set out to directly manipulate group cohesiveness in our experiments, due to random assignment of subjects to groups, we naturally have exogenous variation in this. Specifically, separate from

[2]For example, Camera and Casari (2009) show that cooperation in an infinitely repeated prisoner's dilemma game with random matching is possible with a sufficiently rich reputation mechanism (along with other conditions on the environment).

[3]While there is no significant difference in dynamic favour exchange with or without the record-keeping system, as we show, subjects' behavior is influenced by the information contained therein.

the main experiment, we elicited each subject's social value orientation (Murphy et al., 2011), which measures how individualistic/competitive versus altruistic/pro-social a subject is.

Our final, and most interesting, result is to show the strikingly different effects of social value orientation depending on whether monetary incentives are present or not. In the absence of monetary incentives, there is a strong divergence in dynamic favour exchange between groups with high and low social value orientation. Specifically, groups with high social value orientation were able to sustain high levels of dynamic favour exchange that rivaled or exceeded even those groups who had direct monetary incentives.[4] On the other hand, groups with low social value orientation were unable to sustain high levels of dynamic favour exchange.

When monetary incentives were present, the degree of dynamic favour exchange was not affected by social value orientation. Due to the fact that the Nash equilibrium was not socially efficient, one might have predicted that groups with high social value orientation achieve higher, more socially efficient, levels of favour exchange. The fact that this did not occur provides strong evidence that monetary incentives crowded out social preferences.

The next section briefly reviews the related literature. Section 3 outlines the experimental design and generates hypotheses, which we subsequently test in Section 4. Finally, in Section 5 we provide some concluding remarks and discuss how our results may apply more broadly.

2. Related Literature

This paper is related to two broad streams of literature. First, there is a large literature that is concerned with both direct and indirect reciprocity. This includes the large literature on gift exchange, starting with Fehr et al. (1993, 1997). In these papers one player makes an unconditional gift (i.e., a wage) in the hope of being reciprocated with high effort. This literature has shown that robust gift exchange can occur. Our set-up is slightly different due to the fact that a favour cannot be directly reciprocated in the same period. At best, it can only be directly reciprocated in a later period, if the same pair of subjects are rematched with their roles reversed. Of course, in our Effort Accounting treatment, there is also scope for *indirect* reciprocity because the public accounting ledger provides a summary of the favours done and received.

Two other notable examples of papers focused on indirect reciprocity are Bolton et al. (2005) and Seinen and Schram (2006). Both papers study experimentally the image scoring game where groups of players are paired in each period with one having the option to "give" to another or "keep" for one's self.[5] What makes the game interesting is that the same two players will never interact with each other again, and the players

[4] Alós-Ferrer and Garagnani (2018) also show that social value orientation is positively associated with cooperation. Our result is more nuanced because it only appears to apply in the absence of explicit incentives. Our results also highlight the importance of negative reciprocity in explaining earnings in the absence of monetary incentives.

[5] As with Bigoni et al. (2019), the decision to give/help in these papers is binary.

know this. Yet, Bolton et al. (2005) show that even with a known, finite, last period players frequently give, and giving increases in the amount of past information players have about their current partners (e.g., did their previous partner give to someone who, when it was their turn to give, also gave?).

More recently, Jacobson and Petrie (2014) study favour exchange in a public goods game. In this setting, groups of players play a series of public goods games. Most relevant for us is that they consider treatments where a public ledger of past contributions either is or is not available, in order to distinguish between strategic, direct and indirect reciprocity. They show that contributions are about 14% higher in the presence of a public ledger. Their results provide support for strategic and direct reciprocity but they do not find evidence of indirect reciprocity. Our Effort Accounting treatment includes a public ledger, which may facilitate indirect reciprocity since it tracks the behavior of subjects across all pairings, and not just when matched with a specific subject.

Cabral et al. (2014) study the indefinitely repeated veto game in which nature randomly selects a payoff vector for two players. Although the sum is positive, one player may receive a negative payoff that period. Upon seeing the proposed payoff vector, players can accept or veto it. If one player vetoes, then both players earn 0 in the period. The situation in which one player's payoff is negative corresponds closely to our structure because one player is asked to accept a negative payoff for the social good. In our case, the sender – by doing a favor – earns a negative payoff to benefit the other player. Unlike us, pairs of subjects interact repeatedly. Therefore, they cannot test indirect reciprocity. Their results show that players are motivated by a self-serving, forward-looking notion of reciprocity. That is, they accept a negative payoff in one period because they expect reciprocation in later periods.

Our paper is also somewhat related to literature concerned with the role and emergence of *fiat* money – that is, money that has no inherent value within the experiment and is not convertible to cash outside of the experiment – to facilitate exchange. Two notable examples are Duffy and Puzzello (2014) and Bigoni et al. (2019).[6] Duffy and Puzzello (2014) consider a game in which there is a monetary equilibrium and a continuum of non-monetary equilibria based on gift exchange. Some of the non-monetary equilibria Pareto dominate the monetary equilibrium; however, subjects play the monetary equilibrium. Moreover, welfare is higher when money is available than when it is unavailable.

More closely related is Bigoni et al. (2019). They are interested in the role of "money" to promote the exchange of favours in economies of indefinite duration. In each period of an indefinitely repeated helping game, players in the group are sub-divided into pairs with one player being a "helper" and the other player being a "receiver". The helper makes a binary decision to help or to not help the receiver. Helping is socially efficient but costly to the helper. They show that welfare is higher when subjects have access to money,

[6]Other prominent examples are Camera et al. (2013) and Camera and Casari (2014). Duffy (2015) contains a more comprehensive review.

which is in limited supply and where trade can only occur if the receiver has one unit of money. In contrast, welfare is lower in other treatments where either money is not present or where trade can even occur when the receiver has a zero or negative balance. In contrast to Bigoni et al. (2019), in our game helpers (which we call senders) not only decide whether to help or not, but also the extent of help that is given. Like in their paper, helping is socially efficient (up to a point) but costly to the sender. We also consider a finitely repeated game, which makes help impossible to sustain absent direct monetary incentives. Thus, our result showing that groups with a high social value orientation can sustain dynamic favour exchange (i.e., help) even without direct financial incentives is much stronger. Lastly, we also consider a treatment in which players directly pay for favours, using points that are directly convertible to currency at the end of the experiment.

Finally, our paper relates to literature that analyzes the emergence of alternative currencies from a socio-economic point of view. Seyfang and Longhurst (2013) and Michel and Hudon (2015) classify many examples where groups of people, communities or even regions seek to formalize systems of non-monetary exchange by setting up what are often referred to as *Local Exchange Trading Systems*, Time Banks or CCS. They emphasize the role such systems are meant to have for community-level sustainable development. Our results suggest that they have the risk of either having little effect (limiting them to pure accounting), or crowding out pro-social behaviour in the same way that established currencies do.

3. Experimental Design

The main experiment consisted of 30 periods. At the beginning of the experiment, subjects were placed into groups of 4 and were told that they would remain in that group for all 30 periods. In all treatments, the basic structure of a period was as follows:

1. In each period the four players in a group were randomly divided into two sub-groups. In each sub-group, one player was chosen at random to be a sender and the other player a receiver.

2. The receiver made a request, $r \in \{0, 1, \ldots, 16\}$, to the sender, which we interpret as asking for a favour, but was framed in the experiment in terms of tokens.

3. The sender observed the request, r, and exerts effort, $e \in \{0, 1, \ldots, 16\}$, which is costly to the sender.

4. Payoffs are realized.

Additionally, each subject was assigned a unique ID number for the duration of the experiment. Subjects were shown the ID number of the person with whom they were matched with in each period. This creates the possibility of individual-specific reputation building and positive/negative reciprocity across periods.

However, the history of previous actions was not displayed by the software, nor could players directly observe the behavior of the other two players in the other sub-group.

The stage game represents a kind of dictator game in which the receiver can ask for a certain amount to receive.[7] Given a request, r, and effort, e, the receiver received value $\pi^r(e,r) = 24 \times \min\{e,r\} \geq 0$, while the sender's cost was $\pi^s(e,r) = -\min\{e,r\}^2$. The stage game was played for 30 periods, with the sub-groups and player roles (i.e., sender or receiver) being randomly determined each period.

The experiment consisted of three treatments, which differed in the information players had and whether the sender was compensated by the receiver for her effort or not.

Control Treatment. This treatment proceeded exactly as described above with no modification to the payoffs of the players and no additional information provided to the players. At the end of 30 periods, a subject's final payoff in experimental currency units was the sum over the payoffs over 30 periods (value $\pi^r(e,r)$ received when being a receiver or costs $\pi^s(e,r)$ made when being a sender). The total was converted to Euro at the rate of 125ECU = €1.

Effort Accounting Treatment (EAT). In this treatment, we modified the Control treatment to introduce an accounting system that subjects could use to keep track of their debts/obligations to their other group members. The accounting system was framed to subjects as tokens from a separate account. Specifically, subjects were told that, for receivers, upon receiving $\min\{e,r\}$ units of effort from the sender, the experimental software would automatically deduct $12\min\{e,r\}$ tokens from her account and credit them to the sender's account.[8] Subjects were informed that tokens in this account would not have any monetary consequences for subjects' final payoffs, but that one's own and one's partner's current account balance would be visible to subjects at the time decisions were made. Thus, it could be inferred that a subject with a positive balance exerted more effort on behalf of others than had effort exerted by others on her behalf. This system of accounting is similar to the *MEMORY* treatment of Bigoni et al. (2019). As in the Control treatment, only $\pi^r(e,r)$ and $\pi^s(e,r)$ were used to determine their actual monetary payoff at the end of the experiment.

Monetary Treatment (MT). This treatment was exactly the same as the EAT, except that the transfer from the receiver to the sender had actual monetary consequences. Therefore, in each round the net payoff to the receiver was $\pi^r(e,r) = 24 \times \min(e,r) - 12 \times \min\{e,r\} = 12 \times \min\{e,r\}$, while the net payoff to the sender would be $\pi^s(e,r) = 12 \times \min\{e,r\} - \min\{e,r\}^2$.

[7] Our stage game shares some similarities with Rankin (2006) and Andreoni and Rao (2011). The difference is that, in our case, the total surplus is not fixed but determined by the effort of the sender. Additionally, in our case, senders' payoffs are zero (if nothing is sent) or negative (if something is sent), while in these papers, both players' earnings are non-negative. While they focus on one-shot interactions, we are interested in a repeated interaction setting. Nevertheless, like them, our results show that subjects who request more typically receive more.

[8] That is, the transfer was done automatically by the experimental software and did not require any intervention by subjects.

3.1. Theoretical Predictions

It is not too difficult to see that in both the EAT and Control treatments, the unique stage-game subgame perfect Nash equilibrium outcome is for the sender to provide effort $e = 0$ (and for the receiver to make any request). Therefore, in the finitely repeated game, the unique subgame perfect equilibrium outcome is for the sender to always provide an effort of 0, leading to a surplus of 0.

In contrast, in the MT treatment, given a request of $r > 0$, the sender's optimal effort is easily seen to be $e^*(r) = \min\{r, 6\}$.[9] Since the payoff to the receiver is increasing in effort received, her request will be any $r \geq 6$ in a subgame perfect Nash equilibrium. The total surplus generated in the subgame perfect Nash equilibrium is then $24 \times 6 - 6^2 = 108$, with the sender earning $1/3$ of the surplus and the receiver earning $2/3$.

This leads to the following hypothesis:

Hypothesis 1 (Subgame Perfect Nash Equilibrium). *In the Control and EAT treatments, effort is 0 in every period and total surplus is 0. In the MT treatment, receivers request at least 6 units of effort and senders supply exactly 6 units of effort, generating a surplus of 108. Consequently, subjects in the MT treatment earn significantly more than subjects in either the Control or EAT treatments.*

Finally, observe that the social surplus generated is given by $\pi^s(e) = 24e - e^2$. Therefore, the socially efficient effort level occurs when $r \geq e^s = 12$, leading to a surplus of 144. Thus, even the monetary treatment has a socially inefficient SPNE.

3.2. Survey Questions and Social Value Orientation

The research cited above and an even larger literature, nicely reviewed by Cooper and Kagel (2016) show that many individuals are motivated by social preferences such as altruism and reciprocity, which can promote efficiency-enhancing exchange even in the absence of explicit incentives to do so. The literature on group identity (e.g., Goette et al. (2006) and Chen and Li (2009)) also shows that cooperation and social welfare maximizing behavior increase in cohesive groups. In order to investigate this, we conducted a series of non-incentivized survey questions and an incentivized social value orientation elicitation.

Regarding the non-incentivized survey questions, we had subjects complete the Big Five personality questionnaire (Goldberg, 1992) and other non-incentivized measures. We also had students complete an incentivized social value orientation (SVO) elicitation along the lines of Murphy et al. (2011). The SVO survey consists of a series of questions in which subjects have to choose amounts of money to allocate to oneself and to another subject. This task was incentivized in the following way: pairs of subjects were formed, with one subject selected at random to be the "dictator" and the other to be the "receiver". Then,

[9] Technically, if $r \leq 6$, then any effort $e \in [r, \infty)$ is a best response. This follows because the sender's costs and benefits are only determined by $\min\{e, r\}$.

for the chosen dictator, one of the SVO questions was selected at random to be implemented. Therefore, subjects were paid for one randomly selected question, either as a dictator or as a receiver.

Table 1 depicts two SVO allocation questions drawn from our experiment. In each case, subjects were in the role of a dictator and had to pick one of 9 options, with each option representing an allocation of payoffs to oneself and to another subject. Panel (a) depicts a standard dictator game setting in which changing from option n to $n+1$ raises the payoff of the other subject, while lowering their own payoff by the **same** amount. Therefore, a purely selfish decision maker would choose option 1, while an altruistic decision maker, or someone who cares about inequality, may choose to allocate more to the other subject. Panel (b) represents a slightly different scenario. In this case, one's own payoff is maximized by choosing option 1, but the total surplus is maximized, and inequality is minimized, by choosing option 9.

Table 1: Examples of an SVO Allocation Question

(a) Standard Dictator

Option	1	2	3	4	5	6	7	8	9
You receive	100	94	88	81	75	69	63	56	50
Other receives	50	56	63	69	75	81	88	94	100

(b) Efficiency & Equality

Option	1	2	3	4	5	6	7	8	9
You receive	100	98	96	94	93	91	89	87	85
Other receives	50	54	59	63	68	72	76	81	85

A subject's SVO measure is given by the angle relative to the horizontal axis in the space (payoff to self, payoff to other), with smaller angles (meaning the person allocated more to him/herself) indicating a more competitive/individualistic orientation and larger angles (meaning the person allocated relatively more to the other subject) indicating a more pro-social or altruistic orientation. We have the following hypothesis:

Hypothesis 2 (Behavioral Mechanisms). *1. There is a positive relationship between effort/earnings and group SVO.*

2. The relationship between SVO and effort/earnings is weaker when direct monetary incentives are available.

3. The Effort Accounting Treatment will generate greater effort/earnings than the Control Treatment.

The second part of the hypothesis is motivated by the literature on incentives and social preferences. For example, Bowles and Polanía-Reyes (2012) argue that the presence of monetary incentives may crowd out

social preferences. This is potentially relevant in our case because, while the MT treatment has a unique subgame perfect Nash equilibrium in the stage game, it is still not socially efficient. Thus, there is still scope for social preferences to lead to higher, social welfare-improving, effort unless the presence of monetary incentives crowds out such behavior. Finally, the third part of the hypothesis draws from the experimental literature on repeated games, which shows that strong reputation mechanisms promote cooperation (see e.g., Camera and Casari, 2009). The specific channel may be via indirect reciprocity: the EAT treatment provides a metric of cooperative behavior overall, including in matches between other subjects, which is not observable in the Control treatment.

3.3. Experiments

The experiments consisted of three parts: (i) "favour exchange" game, (ii) non-incentivized survey questions and (iii) incentivized SVO elicitation. We conducted two sets of experiments, which varied the order of tasks. In 2013-14, we had 144 subjects participate across all three treatments, with each treatment consisting of 12 groups of 4 subjects.[10] Subjects participated in the aforementioned order, namely, favour exchange game, survey questions and SVO elicitation. Because of concerns raised by a referee about possible order effects, in 2019 we had an additional 148 subjects (12 groups of 4 for each of the Control and Monetary treatments and 13 groups of 4 for the Effort Accounting treatment) participate in the reverse order.

Remark 1. *Regardless of the order in which stages of the experiment were completed, it is possible that the presence of the first stage influences behavior in the third stage. That is, in the original order, it is possible that the outcomes in the favour exchange game could have influenced behavior in the SVO elicitation. However, it is just as plausible that having the SVO elicitation up front could influence behavior in the favour exchange game. To try to mitigate any spillover effects between stages, the non-incentivized survey – which was always done in the second stage – served as a "cooling-off" period between the stages where our main measures of interest were elicited: behavior in the favour exchange game and the SVO elicitation. This introduces a non-negligible time between the main task and the SVO task, which provides an opportunity for any emotions from the first task to dissipate. In addition, the "other subject" in the SVO elicitation was drawn randomly from amongst all other subjects in the session and not just from the group of 4 that the subject was a part of in the favour exchange game. Lastly, in the 2019 experiments in which the SVO elicitation was conducted first, subjects did not receive feedback on their realized payoff from this task until the end of the experiment. Our hope was that these measures would reduce any spillover between stages 1 and 3 of the experiment.*

[10] We only collected the SVO measure for 4 groups (16 subjects) in the Control treatment. We have the SVO measure for all 96 subjects in the other two treatments.

The experiment was conducted in the BEELab at Maastricht University between December 2013 and March 2014 for the first order and in September 2019 for the reverse order. The experiment was programmed in zTree (Fischbacher, 2007). Subjects were paid for the 30 periods of the favour exchange game and for one randomly selected decision of the SVO elicitation. Subjects earned €18.96 on average and the experiment lasted approximately 90 minutes.[11]

4. Results

In Table 2, we report the average effort received (i.e.,$\min\{e, r\}$) within a subgroup consisting of a sender and a receiver, as well as the average earnings earned within that subgroup. As our unit of independent observation, we take the average over all 30 periods for each group of 4 subjects. As can be seen, in strong contrast to the theoretical prediction (Hypothesis 1), average effort is substantially greater than zero in both the Effort Accounting and Control treatments.[12] For the Monetary Treatment, effort is also significantly higher than the Nash equilibrium of 6 ($p < 0.01$; Wilcoxon signed-rank test).

Second, for the Control and Effort Accounting treatments, average effort appears to be significantly lower in the 2019 treatments, where the main task came after the SVO elicitation.[13] On the other hand, for the Monetary Treatment, average effort is higher but not statistically so in the 2019 sessions.

We now turn to comparisons of effort across treatments. As can be seen, in the 2014 data, there were no significant differences in effort across all three treatments and, in fact, effort was highest in the Control treatment. In contrast, in the 2019 data, effort is at least weakly significantly higher in the Monetary treatment than either the Control treatment ($p = 0.058$) or the EAT treatment ($p \ll 0.01$). In either year, there is no difference in effort between the Control and EAT treatments; however, as noted above, effort is lower in the 2019 data. With regard to earnings, they were always higher in the Monetary treatment than any other treatment, regardless of the year; however, comparing the Control and Monetary treatments in 2014, the difference just misses marginal significance ($p = 0.101$).

Considering the 2014 data, it is somewhat counter-intuitive that despite effort received being similar or even higher in the Control and Effort Accounting treatments than in the Monetary treatments, earnings are lower. The reason for this can be seen by looking at the standard deviations in Table 2 and the histogram

[11] Two subjects (out of 292 overall) went bankrupt in our experiment, even after accounting for the show-up fee and earnings from the SVO elicitation. These subjects left the experiment with no payment.

[12] We do not give a formal test because 0 is represents the lower bound of possible effort.

[13] We hesitate to say that having the SVO elicitation up-front in the 2019 session *caused* effort to be lower. There is a substantial lag between the sessions so there may be other confounding factors. For example, Fisman et al. (2015) show evidence that exposure to the "Great Recession" increased selfishness and shifted towards greater emphasis on efficiency than equality. To be sure, however, there is no evidence that social value orientations differ by year, either pooled overall or individually for each of our three treatments.

Table 2: Average Effort Received and Earnings (Subgroup Level)

(a) Summary Statistics

Treatment	Year	Effort	Std. Dev.	Earnings	Std. Dev
Control	2014	7.76	2.11	99.44	24.78
	2019	5.55	2.52	80.64	23.29
$p-$value; rank-sum test		0.037		0.068	
Effort Accounting	2014	7.02	2.12	93.30	18.78
	2019	5.00	1.51	78.06	18.58
$p-$value; rank-sum test		0.014		0.046	
Monetary	2014	7.02	0.72	114.98	4.56
	2019	7.46	1.43	118.19	10.34
$p-$value; rank-sum test		0.682		0.443	

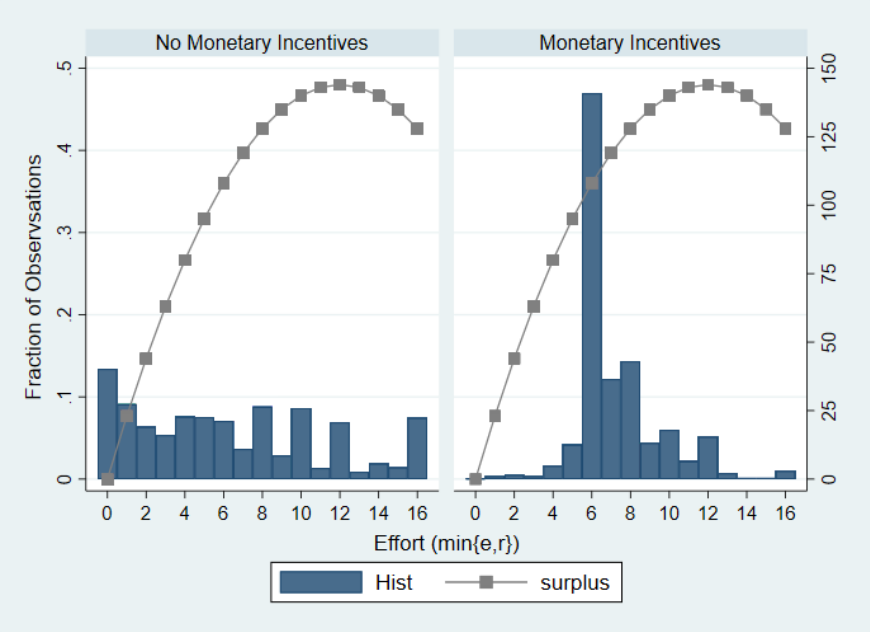
(b) Treatment Comparisons

	Effort		Earnings	
Comparison	2014	2019	2014	2019
Control vs. Monetary	0.165	0.058	0.101	$\ll 0.01$
EAT vs. Monetary	0.810	$\ll 0.01$	$\ll 0.01$	$\ll 0.01$
Control vs. EAT	0.378	0.841	0.378	0.650

Note 1: The unit of independent observation is the 4-person group, averaged over all periods of their interaction. Tests reported in the table are the $p-$values from Mann-Whitney rank sum tests based on the unit of independent observation. For each (treatment, year) pair, we have 12 independent observations, except for (Effort Accounting, 2019), where we have 13 independent observations.

Note 2: Effort received is equal to $\min\{e, r\}$. Earnings are measured in experimental currency units (ECU).

Figure 1: Histogram of Effort Received



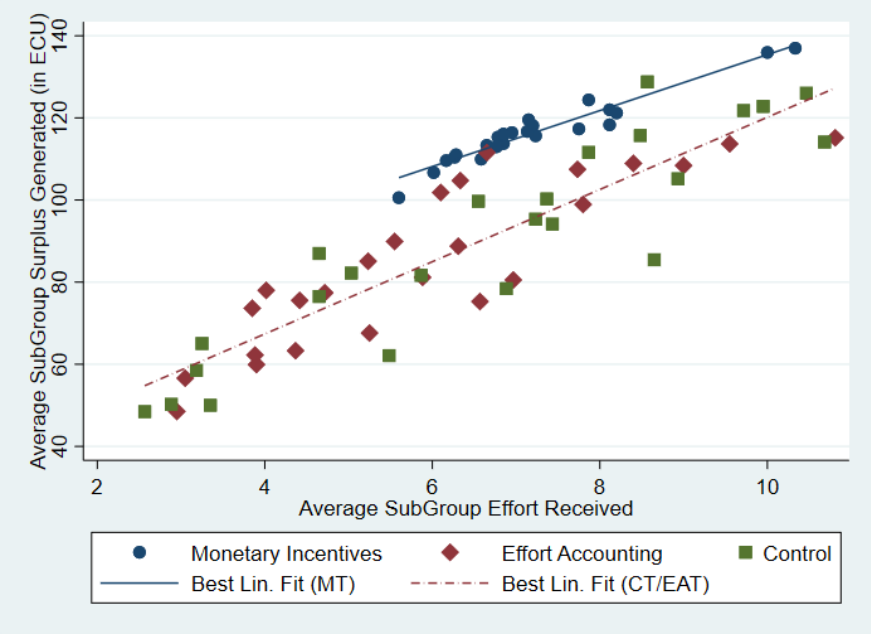
of effort received in Figure 1.[14] Effort received is much more variable in the absence of monetary incentives. Moreover, approximately 11.6% of effort decisions are *above* the social welfare maximizing effort level of 12, while 13.4% of effort decisions are exactly 0. Thus, although effort is the same or higher, on average, in the Control and EAT treatments, because of the higher frequency of extreme (and inefficient) effort levels and the concavity of the total surplus function, average earnings are lower than in MT where only 1.9% of effort decisions are higher than 12, *one* effort decision is 0, and 46.9% are at the Nash equilibrium of 6. Thus, monetary incentives act as a strong coordination device.

The variability in effort and earnings can be seen even more clearly in Figure 2, where we provide a scatter plot of the relationship between average effort and average surplus as well as the best-fitting line through the data-points. For all three treatments, there is a positive relationship between effort and surplus, but the observations for the Monetary treatment are much more concentrated and shifted vertically upwards. Indeed, while many groups in the EAT and Control treatments had lower surplus than in the Monetary treatment, there were some high performing groups that exerted both more effort and received higher surplus than in the monetary treatment. We will investigate this interesting observation below.

As can be seen from the analysis so far, there is very little to distinguish the Control and Effort Accounting Treatments. In fact, contrary to Hypothesis 2.3, subjects actually exert somewhat less effort and have

[14]For ease of presentation, the figure pools the EAT and Control treatments, as well as across years. We also overlay the total surplus generated for each effort level to make clear the concavity of the surplus function and that despite average effort being the same or higher without monetary incentives, earnings are lower due to the greater variability.

Figure 2: Scatter Plot of Average Surplus Generated vs. Average Effort Received



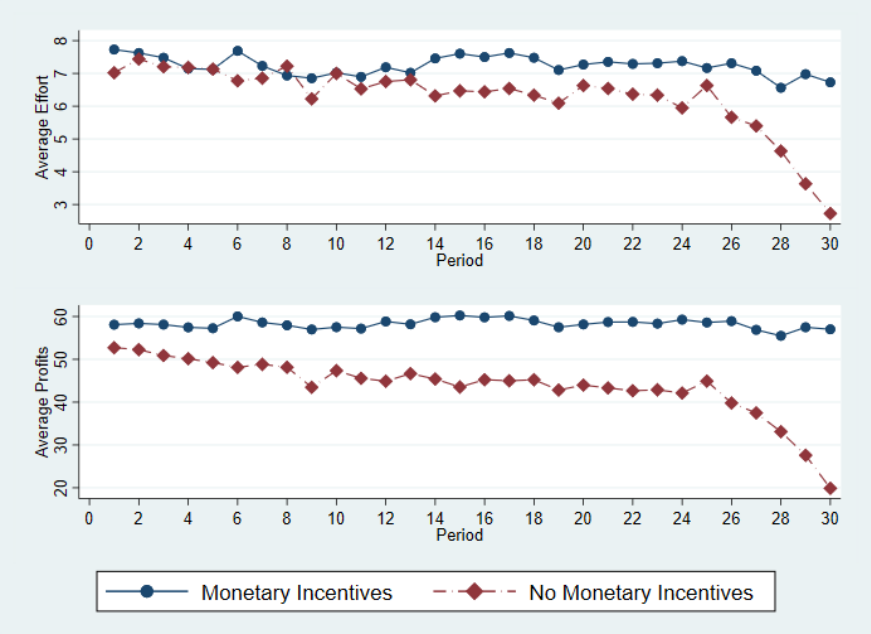
somewhat lower earnings in the EAT treatment than in the Control treatment. Thus, an effort accounting system does not promote greater favour exchange. However, the effort accounting system does seem to reduce variability of earnings, though not significantly so (variance test; $p > 0.1$). Henceforth, to ease the exposition, we will pool the data from our EAT and Control treatments and refer to these as treatments with *no monetary incentives* while our Monetary treatment will be said to have *monetary incentives*. We will, however, return to our EAT treatment to study whether and how subjects used the effort accounting system to make decisions.

4.1. Dynamics of Behavior

We turn now to the dynamics of behavior. In Figure 3 we plot the evolution of effort and earnings both for monetary and non-monetary incentives.[15] Consider first effort. As can be seen, in the monetary treatment, there is no obvious trend, and effort settles around 7. However, without monetary incentives, effort declines modestly for the bulk of the experiment and, starting at about Period 25, there is a strong end-game effect with effort declining rapidly and substantially. In terms of earnings, which can be seen in the bottom panel, average earnings are fairly stable with monetary incentives but there is a distinct downward trend in earnings without monetary incentives, and the drop is particularly sharp, consistent with the drop in effort, over the last five periods.

[15]We provide separate figures, broken down by year in Figure A.1 in the Appendix.

Figure 3: Effort and Earnings Across Periods (Pooled Across Years)



4.1.1. Behavior in the Effort Accounting Treatment

Recall that in the Effort Accounting treatment, hypothetical points were transferred into an account when a player exerted effort on behalf of his/her match, and the person receiving the effort had an equal number of points deducted. Although these points had no direct monetary consequences in the EAT treatment, they were visible when decisions were made. Therefore, someone with a negative account balance could be identified as a person who has received relatively more effort from his/her matches than he/she has exerted on behalf of others. Subjects may infer that such a person has a *debt* to the other group members or that the subject is a free-rider. In this case, they may be unwilling to exert costly effort on such a person's behalf. We explore this possibility in Table 3 which report fixed-effects regressions where the dependent variable is either the effort requested (Columns (1) and (2)) or the effort offered (Columns (3) and (4)) and as explanatory variables we include either the own/match account balances or the difference in account balances between the two matched subjects (Own − Match) in each period.

As can be seen, account balances do not appear to influence behavior strongly regarding requests for effort. Subjects request more the higher is the account balance of their match. This is sensible because the match's high balance indicates that they have exerted relatively more effort on behalf of others. Turning to columns (3) and (4) which consider effort, we can see from column (3) that the higher is one's own account balance, the less effort they exert for others. One possible interpretation is that subjects feel like their high balance implies that they have already exerted "enough" effort for others, so they can justifiably exert less effort now. Column (3) also shows that there is some indirect reciprocity: the higher is one's match's account

Table 3: Effort Accounting System and Behavior (Fixed Effects Regression)

	Request				Effort			
	(1)		(2)		(3)		(4)	
Request					0.158***	(0.045)	0.153***	(0.044)
Period	−0.004	(0.021)	−0.007	(0.022)	−0.085***	(0.020)	−0.087***	(0.021)
Own Account†	0.118	(0.075)			−0.332**	(0.127)		
Match Account†	0.081**	(0.032)			0.092	(0.057)		
(Own − Match)†			−0.019	(0.021)			−0.169***	(0.032)
Constant	12.233***	(0.329)	12.265***	(0.335)	5.615***	(0.664)	5.701***	(0.657)
R^2 (within)	0.012		0.001		0.102		0.094	
Observations	1494		1494		1494		1494	

Note: Dependent variable given in the column heading. Standard errors are clustered at the group level. ***, ** and * denotes significance at the 1%, 5% and 10% level, respectively.

† Account balances are normalized to be in €. This is done to scale the coefficients.

balance the more effort they are willing to exert on their match's behalf. However, the coefficient falls short of significance ($p = 0.122$). Column (4) suggests that effort providers look at the difference between their own account balance and their match's balance. The significantly negative coefficient is also suggestive of indirect reciprocity: a high positive balance (indicating that the subject has exerted more effort than his/her match) leads to less effort. On the other hand, a high negative balance (indicating that the subject has exerted less effort than his/her match) leads to more effort.

4.1.2. Effort Dynamics More Broadly

In Table 4, we report fixed effects regressions where the dependent variable is the amount of effort offered. The main explanatory variable of interest is the difference between the amount of effort given and the amount of effort received up to the current period. This corresponds exactly to one's own effort account balance from the Effort Accounting Treatment and was, of course, observable to subjects in that treatment. In the other two treatments, this information was not given to subjects but could have been calculated by them based on game outcomes.

As can be seen in the table, results are very different depending on whether monetary incentives are present or not. With monetary incentives, there is no evidence of reciprocity as the coefficient on **net** effort given is quantitatively small and not significantly different from zero. Thus it appears that monetary incentives crowd out social preferences. We also see that subjects respond positively to higher requests up to the subgame perfect Nash equilibrium and then, essentially, ignore requests for effort beyond that level. Lastly, observe that effort declines modestly over time. In contrast, in the absence of monetary incentives, we see evidence for reciprocity as the coefficient is negative and highly significant. That is, subjects who have exerted a lot of effort on behalf of others feel less inclined to do so, while subjects who have received a lot of effort from others "return the favour" by exerting more effort. Finally, consistent with Figure 3, we

Table 4: The Determinants of Effort Each Period (Fixed Effects Regression)

	Monetary Incentives		No Monetary Incentives	
Request (R)	1.748***	(0.266)	0.220***	(0.036)
$\mathbf{1}(\text{R} \geq 6)$	7.213***	(1.424)		
$\mathbf{1}(\text{R} \geq 6) \times \text{R}$	−1.737***	(0.268)		
Period	−0.011	(0.015)	−0.084***	(0.016)
Effort Given − Effort Received†	−0.070	(0.050)	−0.373***	(0.078)
Constant	0.220	(1.384)	5.131***	(0.541)
R^2 (within)	0.022		0.102	
Observations	1440		2934	

Note 1: Dependent variable is amount of effort by subject in a period.

Note 2: † Effort Given − Effort Received is constructed to be equivalent to a subject's own account balance in the Effort Accounting Treatment. Like Table 3, it is normalized to be in Euros (€).

Note 3: These regressions pool across years. Absent monetary incentives, there are no differences in results if estimated separately by year. With monetary incentives, the coefficient on net effort given is marginally significant (coefficient −0.145, $p = 0.076$) but less than half the magnitude of the coefficient without monetary incentives.

Note 4: Standard errors are clustered at the group level. ***, ** and * denotes significance at the 1%, 5% and 10% level, respectively.

see that effort declines over time.

4.1.3. Evidence of Direct Reciprocity

Recall that subjects were assigned unique, stable ID numbers in the experiment, and these ID numbers were visible to their matched partner in each period. Therefore, direct reciprocity is possible if subjects pay attention to these ID numbers when making decisions. Indeed, for each subject in a group, we can compute the average effort sent to and received from each of the other three group members. Evidence for direct reciprocity would arise as a positive correlation between these variables.

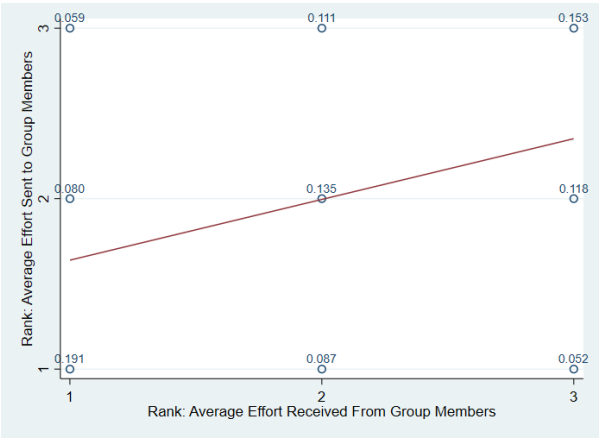
Figure 4 shows a series of scatter plots – one for each treatment – where for each subject in a group of four, we rank the average effort sent to and received from the other subjects in the group. Under perfect reciprocity, a subject should send the least (respectively most) – among all other subjects – to the other subject from which they received the least (respectively most) effort. In the figure, this would imply only observations on the 45 degree line. As can be seen in the figure, for all treatments we see evidence for direct reciprocity, but that the relationship is not perfect. However, as can be seen, for each rank, 1, 2 or 3, of average effort received, the modal rank of average effort sent is assortative.

4.2. The Differing Role of Social Value Orientation

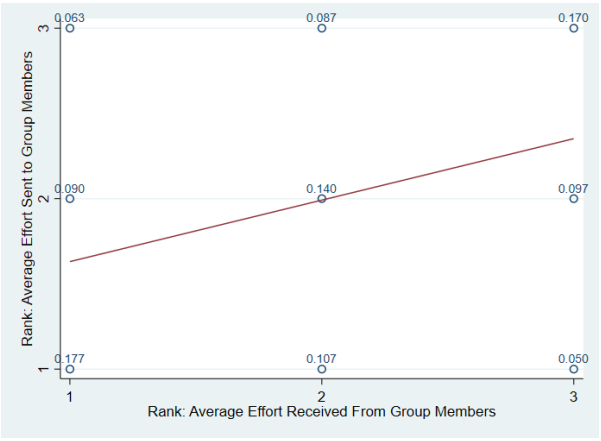
Hypothesis 2.1 conjectured that subjects who are more pro-social according to the social value orientation will exert higher effort, while Hypothesis 2.2 conjectured that this effect would be moderated by the presence of monetary incentives. If these hypotheses are correct, we would expect that earnings and effort would

Figure 4: Direct Reciprocity: Effort Received versus Effort Sent

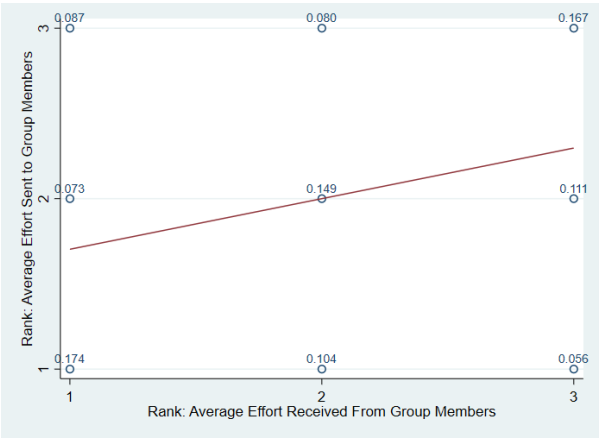
(a) Control Treatment



(b) Effort Accounting Treatment



(c) Monetary Treatment



Note: The numbers above each dot represent the frequency with which we observe the particular rank combination. For example, looking at the $(1, 1)$ cell in panel (c), we see that 17.4% of the time a subject sent the least effort to the subject from which he/she received the least effort.

be more variable when monetary incentives are not present. Moreover, we would expect this additional variation to be explained by social value orientation. Table 2 shows that the first part of this argument is true: earnings and effort are generally more variable in the Control and EAT treatments. We now seek to understand whether this variation can be explained by social value orientation.

In Table 5, we provide summary statistics on subjects' social value orientation, broken down by the year in which the sessions took place and also by whether or not there were monetary incentives. As can be seen, the elicited SVO are slightly lower in 2019 than in 2014 and also slightly lower in the Monetary Treatment than in our two treatments without monetary incentives. However, no pairwise comparison yields a statistically significant difference ($p > 0.34$).

Table 5: Summary Statistics on Social Value Orientation

	Incentives	
	Non-Monetary	Monetary
2014	20.76	20.35
2019	19.15	18.39

Note: The table reports the average of the SVO angle across subjects in the various treatments. Higher numbers indicate greater pro-sociality. With monetary incentives, we have 48 subjects in each of 2014 and 2019. Without monetary incentives we had 64 subjects in 2014 and 100 subjects in 2019.

Remark 2. *Recall that the order in which the SVO was elicited differed between 2014 and 2019, with the latter sessions having the SVO elicitation as the first part of the experiment and the former sessions having the SVO elicitation as the last part of the experiment. The fact that there are no differences across years in Table 5 supports an assertion that there are no order effects. Furthermore, if, in the 2014 data, the SVO elicitation was influenced by earnings (where subjects with higher earnings in the main experiment being more pro-social in the SVO elicitation), then we would expect a higher SVO in the monetary treatment than in the non-monetary treatments. In fact, we see the opposite.*[16] *Again, this supports an assertion that the SVO was not influenced by the order in which the various parts of the experiment took place.*

In Table 6 we report regressions of subject average effort exerted in the main experiment on subjects' social value orientation as elicited in the SVO elicitation part of the experiment, as well as various other non-incentivized control variables elicited in Part 2 of the experiment. The results are pooled across years. As we show in Table A.1 in the Appendix, the slope of the coefficients SVO (for both the monetary and

[16] Lastly, we would also expect a lower SVO in the 2019 treatments for the non-monetary treatments (which we do observe) but also a higher SVO in the monetary treatments (which we do not observe).

non-monetary cases) are stable across years. However, there is a positive scale (i.e., intercept) effect for the Monetary treatment in 2019. That is, as noted earlier, subjects simply exert less effort overall in the 2019 non-monetary treatments. The overall conclusion from this analysis is that, absent monetary incentives, the relationship between SVO and average effort is positive, significant and robust to the inclusion of other control variables. The only difference is that in the 2019 sessions, subjects exert lower effort overall in the non-monetary treatments.

Table 6: The Relationship Between Social Value Orientation and Average Effort (OLS Regression)

	(1)	(2)	(3)	(4)	(5)
SVO (Monetary)	0.014 (0.009)	0.011 (0.010)	0.013 (0.009)	0.014 (0.010)	0.019 (0.012)
SVO (No Monetary)	0.049*** (0.012)	0.045** (0.014)	0.045*** (0.013)	0.048*** (0.013)	0.052*** (0.013)
Monetary/Year Indicators	Yes	Yes	Yes	Yes	Yes
Field of Study	No	Yes	Yes	Yes	Yes
Gender	No	No	Yes	Yes	Yes
Nationality	No	No	No	Yes	Yes
Big 5	No	No	No	No	Yes
R^2	0.163	0.202	0.224	0.252	0.269
Observations	260	256	256	256	256

Note 1: Dependent variable is the subject average of effort exerted over all periods while in the role of sender.

Note 2: Standard errors correct for clustering at the group level. ***, ** and * denotes significance at the 1%, 5% and 10% level, respectively.

Thus, individual-level average effort is positively correlated with social value orientation in the absence of monetary incentives, and the relationship is stable over time. Furthermore, when monetary incentives are present, there is virtually no relationship between effort and social value orientation.

Of course, we are interested in understanding what features allow *groups* of subjects to sustain high effort and high earnings in the absence of monetary incentives. Table 7 looks at group average effort and earnings for both the Monetary Treatment and the Non-Monetary Treatments. As can be seen, group average social value orientation has no effect on either effort or earnings when monetary incentives are present. On the other hand, absent monetary incentives, there is a significantly positive relationship between effort and SVO and also between earnings and SVO. However, as the table shows, the effect appears to be attenuated in the 2019 sessions.

The evidence presented in Table 7 suggest that, the social value orientation of the group affects neither effort nor earnings when monetary incentives are present. This should not be surprising because, when monetary incentives are present, there is a unique subgame perfect Nash equilibrium with positive effort, and our previous results have shown that behavior is closely aligned with the equilibrium. However, *absent monetary incentives*, the social value orientation of the group is a robust predictor for group-level earnings. That is, higher average SVO within the group translates into higher earnings. This effect is highly significant

Table 7: Social Value Orientation and Group Average (OLS Regression) Effort/Earnings

	Monetary				Non-Monetary			
	Effort		Earnings		Effort		Earnings	
SVO	0.033	(0.039)	0.128	(0.138)				
SVO × 2014					0.123***	(0.044)	0.776***	(0.211)
SVO × 2019					0.039	(0.048)	0.410*	(0.230)
Constant	6.605***	(0.793)	55.818***	(2.798)	4.516***	(0.921)	31.577***	(4.431)
R^2	0.031		0.037		0.255		0.306	
Observations	24		24		41		41	

Note 1: Dependent variable is group average effort or earnings, as in the column headings.

Note 2: Standard errors in parentheses. ***, ** and * denotes significance at the 1%, 5% and 10% level, respectively.

Note 3: Model selection – with respect to slope interactions and year-specific constants – was driven by the BIC.

in the 2014 data, but somewhat weaker and only marginally significant in the 2019 data. Regarding average group effort, the differences are even more stark: in the 2014 data the relationship between average effort and group SVO is positive and highly significant, while in the 2019 data, the effect is much smaller and not significant.

The differences in the group-level relationships between earnings/effort and social value orientation across years is interesting because, as Table A.1 show, the relationship between effort and social value orientation at the individual level differ by only a scalar (i.e., effort is lower overall in 2019, but the slopes are identical). Therefore, it would seem there must be other group-level characteristics that differed between 2014 and 2019 that explain the attenuation in the effect of group average social value orientation on average earnings.

To explore the possible group-level differences in the non-monetary treatments across years and determine whether they are responsible for the observed attenuation of SVO, we first tested whether the group averages of any of the variables from Part 2 of the study differed significantly across years. These variables included measures for trust, reciprocity, negative reciprocity, risk attitudes as well as the Big 5 index. The only variable which showed a difference across years was negative reciprocity, with subjects in 2019 reporting themselves as being less willing to punish others at a personal cost (6.39 in 2014 versus 5.15 in 2019; $p = 0.001$).[17] In addition to these variables, we also observed a significant difference in gender composition. Specifically, in our 2019 data, the groups had significantly more females (1.25 in 2014 versus 2.44 in 2019; $p = 0.003$).'[18] Table A.2 in the appendix shows that, after controlling for these variables, the relationship between group-level social value orientation and average earnings are statistically indistinguishable across years. This table also shows that groups who score more highly on negative reciprocity (i.e., a willingness to

[17]The specific question asked was, "Are you a person who is generally willing to punish unfair behavior, even if this is costly to you?"

[18]Note that the differences in gender balance appears to be unique to the non-monetary treatments. Looking at all subjects, we cannot reject that the male-female split is the same across years. In the case of negative reciprocity, this appears to be a more general shift. Over all subjects, the self-reported negative reciprocity is significantly lower in the 2019 data.

punish others) earn marginally significantly more.[19] This suggests that two factors are necessary to support favour exchange in groups without monetary incentives: the members must be (i) pro-social but (ii) willing to punish deviations from pro-social behavior.

5. Concluding Remarks

Our results have shown that a system of exchange with direct monetary incentives is the most efficient at capturing gains from trade. When such monetary transfers were present as a medium of exchange, on average, subjects earned significantly more money. When subjects could not use direct monetary transfers and, instead, had to rely on non-monetary exchange of favours average earnings were lower. However, we found that some groups were able to sustain high levels of favour exchange, and generate surplus on par with, or even exceeding, groups who had direct monetary incentives. Moreover, our results were able to show that the distinguishing feature between successful and unsuccessful groups in the absence of monetary incentives was the social value orientation of the group, as well as a willingness to punish deviations from pro-social behavior. Those with high social value orientations sustained consistently high effort. Methodologically, the result is robust to the order in which behavioral metrics were elicited, especially when we control for feelings of negative reciprocity (as measured by subjects' reported willingness to punish unfair behaviour).

This suggests several avenues for future research. For example, one could think about ways of building social value orientation within groups, such as via team-building exercises. Furthermore, allowing for endogenous selection into groups with monetary or non-monetary incentives would seem fruitful. It would be interesting to see if those subjects who had high social value orientation would select into non-monetary incentives and be even more successful than in our experiments where subjects were randomly assigned to a group. Both of these features seem to be at work in the start-up community. For example, one technology incubator speaks of the importance of building a community based on altruism, reciprocity and strong relationships both at work and outside of work.[20] Beyond this, it would be interesting to explore the fraught nature of favour exchange. In particular, it seems natural that many people would rather pay money to avoid the obligation that comes with receiving a favour. This is just a small sample of the type of questions that could be fruitfully explored from our research.

[19] As a further robustness check, we also ran another specification including all other variables even if there was no significant differences at the group level across years. With these additional control variables, we also cannot reject that social value orientation has the same effect across years. Furthermore, a joint test that these nine additional variables are all zero can only be rejected at $p = 0.058$.

[20] See, `https://atlantatechvillage.com/about/our-story/` where they speak of their mission to "pay it forward", "be nice" and "work hard; play hard" (Accessed on 14 Sep 2018).

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Appendix A. Supplemental Figures and Tables

Table A.1: Social Value Orientation and Individual Average Effort (OLS Regression)

(a) 2014 Sessions

	(1)	(2)	(3)	(4)	(5)
SVO (Monetary)	0.020 (0.015)	0.024 (0.016)	0.022 (0.016)	0.020 (0.019)	0.014 (0.021)
SVO (No Monetary)	0.052*** (0.018)	0.046** (0.021)	0.053** (0.019)	0.051** (0.019)	0.053*** (0.018)
Monetary Indicator	Yes	Yes	Yes	Yes	Yes
Field of Study	No	Yes	Yes	Yes	Yes
Gender	No	No	Yes	Yes	Yes
Nationality	No	No	No	Yes	Yes
Big 5	No	No	No	No	Yes
R^2	0.074	0.188	0.228	0.280	0.307
Observations	112	108	108	108	108

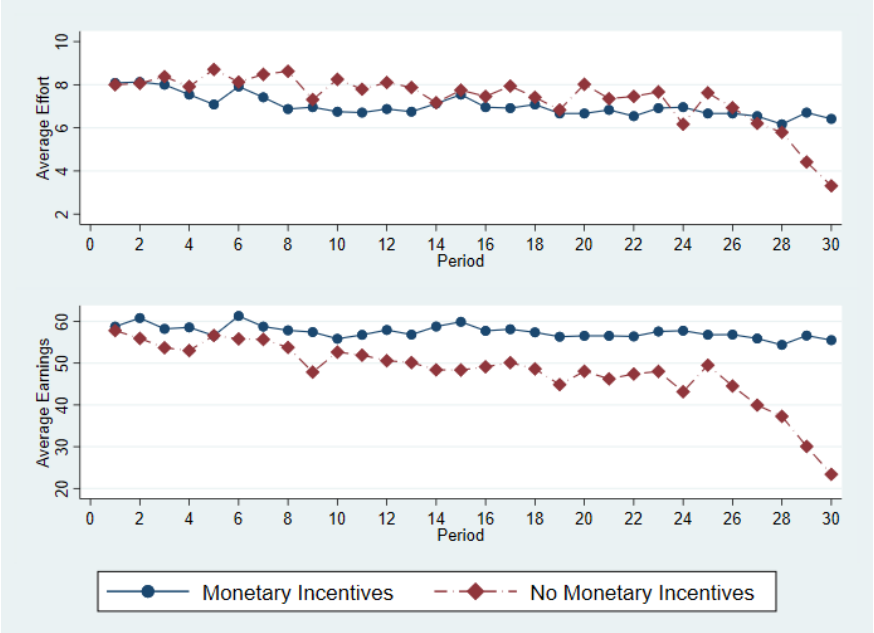
(b) 2019 Sessions

	(1)	(2)	(3)	(4)	(5)
SVO (Monetary)	0.009 (0.011)	−0.005 (0.012)	−0.000 (0.011)	−0.005 (0.012)	0.009 (0.017)
SVO (No Monetary)	0.047*** (0.017)	0.040** (0.018)	0.036** (0.016)	0.037* (0.020)	0.040** (0.019)
Monetary Indicator	Yes	Yes	Yes	Yes	Yes
Field of Study	No	Yes	Yes	Yes	Yes
Gender	No	No	Yes	Yes	Yes
Nationality	No	No	No	Yes	Yes
Big 5	No	No	No	No	Yes
R^2	0.165	0.233	0.248	0.288	0.332
Observations	148	148	148	148	148

Note: In Panel (a), the unit of independent observation is the 4-person group, averaged over all periods of their interaction. In panel (b), we take the individual average. Panel (b) corrects for clustering at the group level. ***, ** and * denotes significance at the 1%, 5% and 10% level, respectively.

Figure A.1: Effort and Earnings Across Periods

(a) 2014 Data



(b) 2019 Data

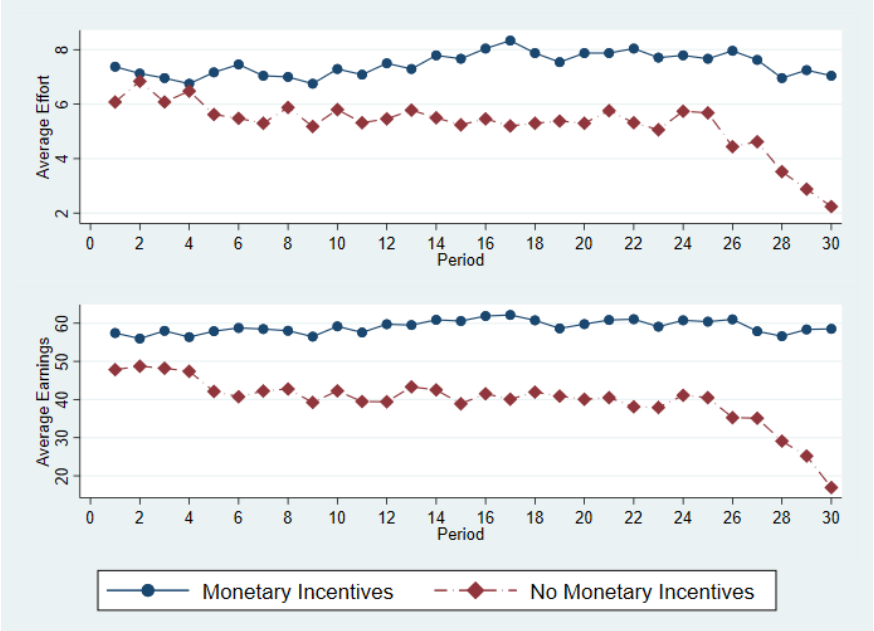


Table A.2: Group-Level Differences Across Years That Explain Attenuation of SVO Relationship in Non-Monetary Treatments (OLS Regression; Dependent Variable: Group Average Earnings)

SVO × 2014	0.776***	(0.211)	0.826***	(0.220)	0.710***	(0.209)	0.761***	(0.217)
SVO × 2019	0.410*	(0.230)	0.481*	(0.246)	0.493**	(0.229)	0.569**	(0.244)
Number of Females			−1.066	(1.263)			−1.117	(1.228)
Negative Reciprocity					2.386*	(1.368)	2.416*	(1.371)
Constant	31.577***	(4.431)	32.716***	(4.648)	17.698*	(9.049)	18.720**	(9.140)
R^2	0.270		0.319		0.359		0.373	
Observations	41		41		41		41	
Test of Year Effect ($p-$value)	0.016		0.025		0.199		0.260	

Note 1: Standard errors in parentheses. ***, ** and * denotes significance at the 1%, 5% and 10% level, respectively.

Appendix B. Instructions for Control Treatment (Online Appendix)

General Instructions

Welcome to this experiment on decision-making! In this experiment you can earn money. The amount you earn depends on the decisions you and other participants make. Therefore please read these instructions carefully. In the experiment you will earn points. At the end of the experiment we will convert the points you have earned into euros according to the rate: 125 points equal €1. You will be paid your earnings privately and confidentially after the experiment.

Throughout the experiment you are not allowed to communicate with other participants in any way. If you have a question please raise your hand. An experimenter will come to your desk to answer it.

Your group

At the beginning of the experiment the computer will randomly assign you (and all other participants) to a group of 4 participants. Group compositions do not change during the experiment. Hence, you will be in the same group with the same people throughout the experiment. The composition of the groups is anonymous. Neither during nor after the experiment will you get to know the identities of the other people in your group. The other people in the group will also not get to know your identity. On your computer screen, you will see your randomly generated experiment ID, which stays the same throughout the experiment. When matched with another group member, you will see his or her experiment ID as well.

Number of rounds

The experiment consists of 30 rounds. You will receive a show-up fee of €5. In each round you can earn additional points. Your total earnings will be the sum of the show-up fee and your earnings converted from points in each of the 30 rounds.

The decision task

At the beginning of each round, you will get randomly paired with **one** of your three other group members. You will be able to see the experiment ID of your partner and he/she will get to see your experiment ID. One of you will be assigned the role of **receiver**; the other will be assigned the role of **sender**. Please note that your role and paired group member can change each round.

Every round, the receiver makes a request on how many tokens he or she would like to receive from the sender. The sender makes a decision on how many tokens to send to the receiver. The precise details are provided below.

As a receiver, you can choose a number between 0 and 16, which denotes the amount of tokens you would like to receive from the sender. When the sender receives your request, he or she can decide on the amount of tokens to send to you, by choosing a number between 0 and 16. The lower of the two numbers will determine the number of tokens created. For example, if you request 5 tokens but the sender chooses 3, then the number of tokens created will be 3. Likewise, if you request 5 tokens and the sender chooses 8, then the number of tokens created will be 5. You will earn points for the tokens that you receive according to the following formula.

$$\text{Earnings} = 24 \times \text{Tokens Received}.$$

As a sender, you will decide on how many tokens to send to the receiver, based on the request you receive. This request is a number between 0 and 16. The amount of tokens that will be created is a number between 0 and 16 as well. The **lower** of the two numbers will determine how many tokens will be created. You will pay costs for the tokens created according to the following formula.

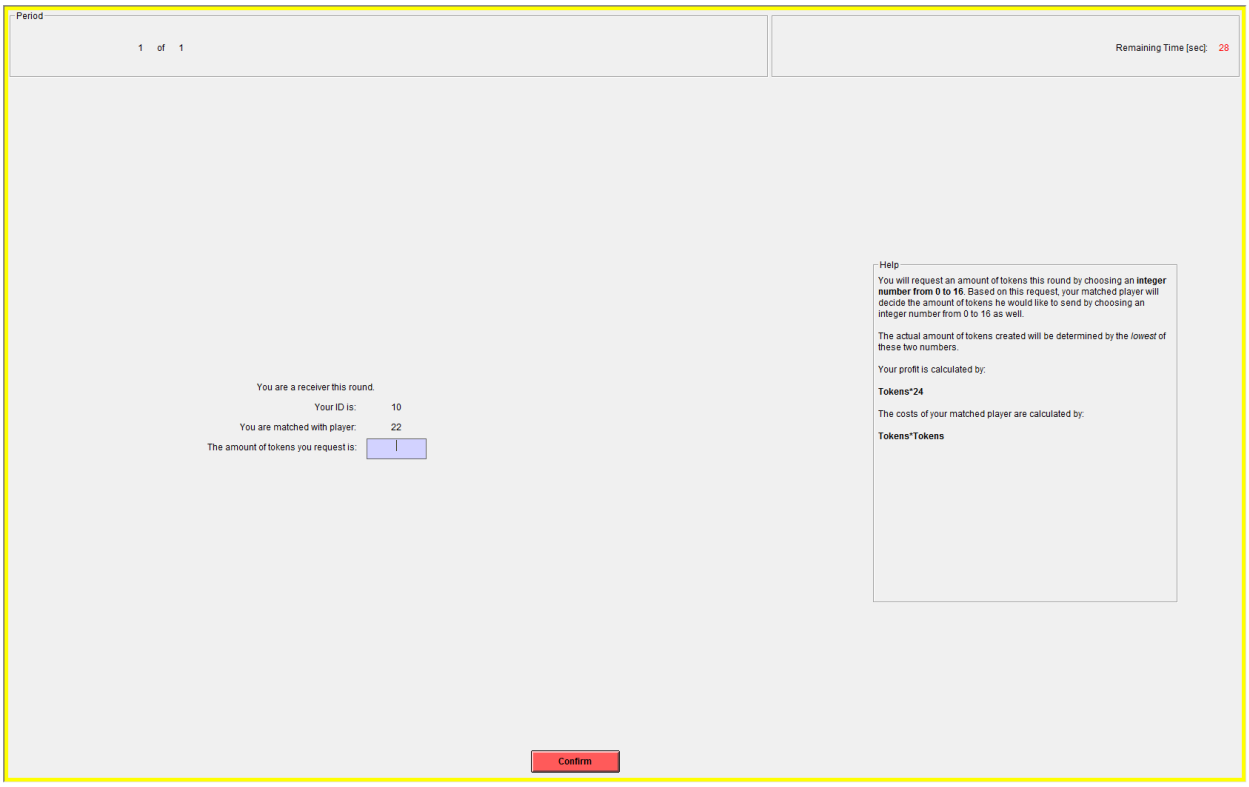
$$\text{Costs} = \text{Tokens} \times \text{Tokens}.$$

At the end of the experiment, you will be paid out the following: **initial show-up fee + points of Earnings − points of Costs**. Minimum earnings will be 0 and therefore you cannot earn a negative amount.

Information

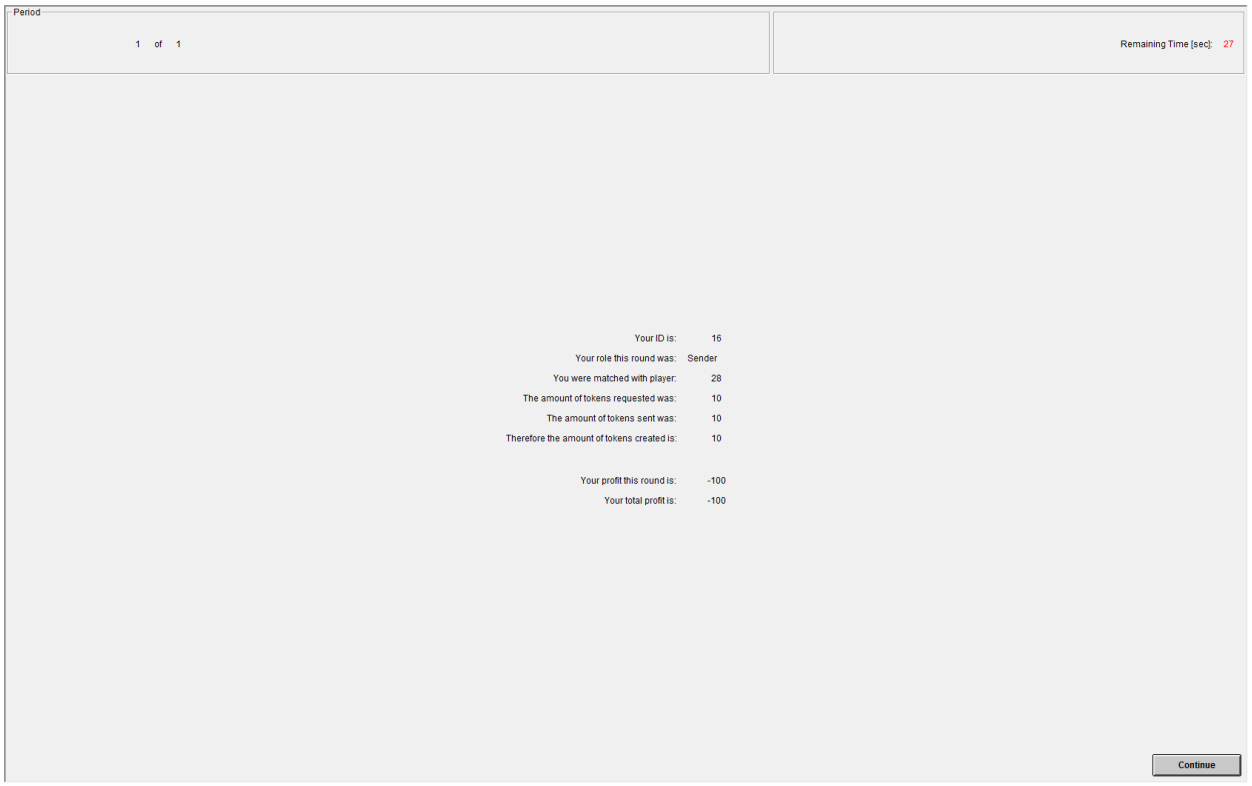
During the experiment, when making your decision, you will see the following information on your screen (see also the screenshot):

- Your role this round
- Your experiment ID
- The experiment ID of the group member you are paired with
- If a sender, the request of the group member you are paired with



During the experiment, when receiving the outcome of the round, you will see the following information on your screen (see also the screenshot):

- Your experiment ID
- Your role this round
- The experiment ID of the group member you were paired with
- Tokens requested/chosen to send
- The tokens requested/chosen to send of the group member you were paired with
- Amount of tokens created
- Your earnings/costs this round
- Your total earnings



Examples

Here are two examples of hypothetical play to make the instructions clear. Note that all numbers mentioned are points.

Example 1: Participant A is a receiver this round and requests 10 from the sender. Participant B is the sender who receives a request of 10 and decides to send 4 tokens. The number of tokens created will be 4. Participant A earns $24 \times 4 = 96$ and participant B pays $4^2 = 16$.

Example 2: Participant A is a receiver this round and requests 6 from the sender. Participant B is the sender who receives a request of 6 and decides to send 14 tokens. The number of tokens created will be 6. Participant A earns $24 \times 6 = 144$ and participant B pays $6^2 = 36$.

Control questions

To ensure that you understood the instructions we ask you to answer a few control questions. Please take the page with control questions in front of you. After all participants have correctly answered these questions, the experiment will continue. Raise your hand when you have completed the control questions and an experimenter will come to your desk and check your answers.

Concluding remarks

You have reached the end of the instructions. If anything remains unclear to you or if you have any questions, please raise your hand.

Appendix C. Instructions for the EAT Treatment (Online Appendix)

In the interest of space, we only report the part of the instructions that differed from the Control.

The decision task

... There will be two accounts in this experiment: **Account A** and **Account B**. You will earn points, which will be deposited in **Account A** for the tokens that you receive according to the following formula:

$$\text{Earnings in Account A} = 24 \cdot \text{Tokens Received}.$$

In addition, whenever you receive tokens from the sender, you will transfer points from your Account B to the senders Account B according to the formula:

$$\text{Points transferred to Account B of Sender} = 12 \cdot \text{Tokens Received}.$$

As a sender, you will decide on how many tokens to send to the receiver, based on the request you receive. This request is a number between 0 and 16. The amount of tokens that will be created is a number between 0 and 16 as well. The **lowest** of the two numbers will determine how many tokens will be created. You will pay costs, which will be deducted from Account A, for the tokens created according to the following formula:

$$\text{Costs from Account A} = \text{Tokens} \times \text{Tokens}.$$

In addition, as noted above, you will receive points from the sender, which will be deposited in your Account B according to the formula:

$$\text{Points transferred to Account B from Receiver} = 12 \cdot \text{Tokens}.$$

At the end of the experiment, you will be paid out the following: initial show-up fee + (Final Account A Balance)/125. That is, your final balance in Account B will not influence your payment at the end of the experiment. Minimum earnings will be 0 and therefore you cannot earn a negative amount.

Information

[Subjects are informed that they will also observe:]

- Your own Account B balance
- The Account B balance of the group member you are paired with

Examples

Here are two examples of hypothetical play to make the instructions clear. Note that all numbers mentioned are points.

Example 1: Participant A is a receiver this round and requests 10 from the sender. Participant B is the sender who receives a request of 10 and decides to send 4 tokens. The number of tokens created will be 4. Participant A earns $24 \times 4 = 96$ to be credited in his/her Account A and participant B pays $4^2 = 16$ to be deducted from his/her Account A. In addition, Participant A will have $12 \cdot 4 = 48$ points deducted from his/her Account B, which will be transferred into Account B of Participant B.

Example 2: Participant A is a receiver this round and requests 6 from the sender. Participant B is the sender who receives a request of 6 and decides to send 14 tokens. The number of tokens created will be 6. Participant A earns $24 \cdot 6 = 144$ in Account A and participant B pays $6^2 = 36$, which will be deducted from his/her Account A. In addition, $12 \cdot 6 = 72$ points will be transferred from Account B of Participant A to Account B of Participant B.

Appendix D. Instructions for Monetary Treatment (Online Appendix)

In the interest of space, we only report the part of the instructions that differed from the Control.

The decision task

You will earn points for the tokens that you receive according to the following formula.

$$\text{Gross Earnings} = 24 \cdot \text{Tokens Received}.$$

You must pay the sender for each token that you receive. The cost per token is 12. Therefore, your net earnings are given by:

$$\text{Net Earnings} = 24 \cdot \text{Tokens Received} - 12 \cdot \text{Tokens Received} = 12 \cdot \text{Tokens Received}.$$

As a sender, you will decide on how many tokens to send to the receiver, based on the request you receive. This request is a number between 0 and 16. The amount of tokens that will be created is a number between 0 and 16 as well. The lower of the two numbers will determine how many tokens will be created. You will pay costs for the tokens created according to the following formula:

$$\text{Costs} = \text{Tokens} \times \text{Tokens}.$$

In addition, you will receive a payment of 12 per token from the receiver. Therefore, your net earnings are given by:

$$\text{Net Earnings} = 12 \cdot \text{Tokens} - \text{Tokens} \times \text{Tokens}.$$

At the end of the experiment, you will be paid out the following: initial show-up fee + (net earnings)/125. Minimum earnings will be 0 and therefore you cannot earn a negative amount.

Examples

Here are two examples of hypothetical play to make the instructions clear. Note that all numbers mentioned are points.

Example 1: Participant A is a receiver this round and requests 10 from the sender. Participant B is the sender who receives a request of 10 and decides to send 4 tokens. The number of tokens created will be 4. Participant A earns $24 \times 4 - 12 \times 4 = 96 - 48 = 48$ and participant B receives a net payment of $12 * \times 4 - 4^2 = 48 - 16 = 32$.

Example 2: Participant A is a receiver this round and requests 6 from the sender. Participant B is the sender who receives a request of 6 and decides to send 14 tokens. The number of tokens created will be 6. Participant A earns $24 \times 6 - 12 \times 6 = 144 - 72 - 72$ and participant B receives a net payment of $12 \times 6 - 6^2 = 72 - 36 = 36$.

Appendix E. Control Questions (Online Appendix)

Control Treatment

Please answer the following control questions to ensure that you have understood the instructions.

1. You are a receiver this round and request 12 tokens. The sender decides to create 6.
   - The actual amount of tokens created will be [**response**] this round.
   - I will earn [**response**].
   - The sender will pay costs of [**response**].
2. You are a sender this round and receive a request of 8. You decide to send 12.
   - The actual amount of tokens created will be [**response**] this round.
   - I will pay costs of [**response**].
   - The receiver will earn [**response**].

Effort Accounting Treatment

Please answer the following control questions to ensure that you have understood the instructions.

1. You are a receiver this round and request 12 tokens. The sender decides to create 6.
   - The actual amount of tokens created will be [**response**] this round.
   - I will earn [**response**] in Account A.
   - I will transfer [**response**] from my Account B to the senders Account B.

- The sender will pay costs of [**response**] in Account A.

2. You are a sender this round and receive a request of 8. You decide to send 12.

   - The actual amount of tokens created will be [**response**] this round.
   - I will pay costs of [**response**] in Account A.
   - I will receive [**response**] in my Account B from the receivers Account B.
   - The receiver will earn [**response**] in Account A.

Monetary Treatment

Please answer the following control questions to ensure that you have understood the instructions.

1. You are a receiver this round and request 12 tokens. The sender decides to create 6.

   - The actual amount of tokens created will be [**response**] this round.
   - My gross earnings will be [**response**].
   - I will pay [**response**] to the sender.
   - My net earnings will be [**response**].
   - The sender will pay costs of [**response**].
   - The senders net earnings will be [**response**].

2. You are a sender this round and receive a request of 8. You decide to send 12.

   - The actual amount of tokens created will be [**response**] this round.
   - I will pay costs of [**response**].
   - I will receive a payment of [**response**] from the receiver.
   - My net earnings will be [**response**].
   - The receiver's gross earnings will be [**response**].
   - The receiver's net earnings will be [**response**].