ISSN 2282-6483

Alma Mater Studiorum - Università di Bologna

DEPARTMENT OF ECONOMICS

Money and the Scale of Cooperation

Maria Bigoni
Gabriele Camera
Marco Casari

Quaderni - Working Paper DSE N°1045

Money and the Scale of Cooperation*

Maria Bigoni
University of Bologna

Gabriele Camera
Chapman University
& University of Basel

Marco Casari
University of Bologna
& IZA

December 19, 2015

Abstract

This study reveals the existence of a causal link between the availability of money and an expanded scale of interaction. We constructed an experiment where participants chose the group size, either a low-value partnership or a high-value group of strangers, and then faced an intertemporal cooperative task. Theoretically, a monetary system was inessential to achieve cooperation. Empirically, without a working monetary system, participants were reluctant to expand the scale of interaction; and when they did, they ended up destroying surplus compared to partnerships, because cooperation collapsed in large groups. This economic failure was reversed only when participants managed to concurrently develop a stable monetary system.
Keywords:
Endogenous institutions, experiments, repeated games, strategic uncertainty.

* We thank Vincent Bourke and Megan Luetje for outstanding research support, and seminar participants at the University of Bologna, Goethe University in Frankfurt, Tilburg University, GATE (Lyon), and at the Workshop in Behavioral Public Economics in Vienna. G. Camera acknowledges partial research support through the NSF grant CCF-1101627. M. Casari acknowledges financial support through the ERC Starting Grant number 241196.

Correspondence address: Gabriele Camera, Economic Science Institute, Chapman University, One University Dr., Orange, CA 92866; e-mail: camera@chapman.edu.

1 Introduction

Large scale cooperation is central to economic development but presents formidable challenges (Kimbrough et al., 2008; North, 1991). How can agents succeed in widening the scale of economic cooperation? Suitable institutions are of primary importance in overcoming these challenges. The literature has focused on monitoring, enforcement, communication, and punishment (Greif, 2006; Milgrom et al., 1990). The present study is about money. We consider a situation where agents can choose between a stable partnership or higher-value interactions in large groups of strangers. In a laboratory setting, we show that a monetary system is *behaviorally* crucial in widening the scale of cooperation, even when it is theoretically inessential.

History provides anecdotal evidence of a relation between the scale of economic cooperation and the availability of reliable monetary systems. For example, thirteenth century trade in Europe flourished at the time Genoa and Florence returned to strike gold coins (Lopez, 1971), and eighteenth century commerce in the West relied on the Spanish dollar. These observations do not constitute evidence of a causal link between emergence of monetary institutions and large scale cooperation. The expansion of trade may stem from superior legal institutions or military might, and not from monetary systems; conversely, the failure to expand the scale of cooperation may be due to low returns from trade or technological factors, and not due to absence of monetary systems. The advantage of the experimental methodology is that we can suppress institutional and environmental confounding factors that characterize field data, and understand what principles are in operation (Plott, 2001).

Consider a setup that captures the principle behind the typical monetary trade: a producer gives a valuable good to a consumer in exchange for a token, which is a symbolic object. The good is produced at a cost below its consumption value and though the token has no intrinsic value, it is storable for exchange in future rounds. In this situation, there are gains from trade. We construct a repeated helping game where agents face a sequence of pairwise encounters in which their role alternates between producer and consumer (Townsend, 1980). In each encounter the producer suffers a small cost to "help" the consumer by giving her the good. The consumer can concurrently transfer a token to the producer. Helping maximizes benefits in the pair, irrespectively of whether a token is transferred or not. This theoretical setup is contrasted to a situation where tokens are not present and therefore the consumer has nothing to transfer to the producer (Ellison, 1994; Friedman, 1971; Kandori, 1992). In all setups, cooperation amounts to an intertemporal exchange of help.

For each setup, we study the sustainability of cooperation both theoretically and in a controlled experiment. The experiment has two conditions, one in which a monetary system can spontaneously emerge, and another in which it cannot (Bigoni et al., 2014). In each condition, the scale of cooperation is endogenous. Participants can choose to interact either with a fixed counterpart in a partnership (Dal Bó and Fréchette, 2011; Palfrey, 1994), or in a large group of strangers (Camera et al., 2013). This choice is meaningful because by design help carries a greater benefit in large groups than in partnerships.

Having repeated opportunities to help gives rise to a social dilemma that is especially conspicuous in large groups, where interaction is impersonal and reciprocity impossible (Fehr and Gächter, 2000; Gächter and Hermann, 2011). Payoffs are maximized under full cooperation, i.e. when every producer helps in every round. However, participants may refuse to help to avoid an immediate cost, and this inevitably minimizes payoffs for everyone in the group.

Folk theorem-type of results imply the existence of multiple equilibria (Ellison, 1994; Kandori, 1992). This situation is characterized by strategic uncertainty (Van Huyk et al., 2007), which makes it harder to attain efficient outcomes in large groups (Weber, 2006). If participants succumb to short-run opportunistic temptations, then there will be little or no cooperation. However, full cooperation can also spontaneously emerge if participants succeed in coordinating actions intertemporally through a norm of mutual help. This requires sufficient trust that help given today will be reciprocated by strangers in future encounters. When tokens are available, cooperation can also be achieved by developing a convention of monetary trade. This requires trusting that strangers will help in exchange for a token in future encounters (Camera and Casari, 2014). However, by construction monetary trade is neither necessary nor sufficient to achieve cooperation. Contrasting the two conditions thus reveals whether or not the availability of symbolic media of exchange behaviorally influences the chosen scale of interaction, and of cooperation.

There is a growing experimental literature on money as a means of payment, starting with the early contributions of McCabe (1989), and of Lian and Plott (1998). In particular, this paper is related to our previous experiments about decentralized, fiat monetary systems: Camera and Casari (2014) studies the coordination role of monetary exchange, while Camera et al. (2013) studies cooperation with and without money in small and large groups, offering an evolutionary explanation for the use of money. Duffy and Puzzello (2014) also exogenously manipulates group size in reference to a specific monetary model. The endogenous choice of the scale of interaction is an original aspect of the present study, as previous experiments study group formation only without money (Ahn et al., 2009).

2 The game

The experiment has a CONTROL and a TOKENS condition. In the CONTROL condition, participants play a "helping game" in pairs composed of a producer and a consumer. Each producer starts with $d = 6$ consumption units (CUs) and can choose to help ("give help") or not ("no help"). The consumer has $d - l = 3$ CUs. Helping yields a payoff of 0 CUs to the producer and a payoff of $k > 2d - l$ CUs to the consumer; the net benefit from help is $k - 2d + l$ CUs.

Participants play this game repeatedly, in "cycles" that last nineteen rounds on average. In each round, half of the participants are consumers and half producers. Roles are randomly assigned in the first round, and deterministically alternate in the following rounds. Participants know they play sixteen rounds and from round sixteen on they play an additional round with 75% probability, otherwise the cycle ends (Fréchette and Yuksel, 2013; Palfrey, 1994; Roth and Murnighan, 1978). CUs cumulate across rounds, and are converted into dollars at the end of the session. This set-up captures the essence of an interaction, in which there are gains from intertemporal trade.

A session includes six cycles. In each cycle, participants interact either in *partnerships* or *large groups* of 12 or 24 individuals. In a partnership, the counterpart is fixed throughout a cycle. In large groups, the counterpart is randomly chosen in every round, and identities remain undisclosed; hence, individuals interact as strangers. There is public monitoring of defections: strangers can observe whether or not every producer in their group helps.

Benefits are greater in large groups ($k = 18$) than in partnerships ($k = 15$). If no one cooperates, then average per-capita payoffs are 4.5 CUs both in partnerships and large groups. Instead, under full cooperation they reach 7.5 CUs in partnerships, and 9 CUs in large groups. The difference between per-capita payoffs attained and the 4.5 CUs baseline measures how much *surplus* is created in a round of play. Hence, expanding the scale of interaction can be beneficial since large groups can create 50% more surplus than partnerships (4.5 CUs vs. 3 CUs). However, surplus creation is not an automatic process—it depends on the degree of cooperativeness actually achieved in the group. We assess a group's success in creating surplus by measuring economic *efficiency*, defined as the proportion of surplus created by the group in the average round of play relative to the maximum potential of 4.5 CUs. Efficiency is directly proportional to the cooperation rate in the group. Efficiency is zero when no one cooperates—in groups of any size—while if everyone cooperates it reaches 67% (=3 out of 4.5 CUs) in partnerships and 100% (=4.5 out of 4.5 CUs) in large groups. Rational, self-interested participants can attain full cooperation by following a simple rule of conduct: they help as long as everyone else does the same; otherwise, they will never help again (Ellison, 1994; Friedman, 1971; Kandori, 1992).

In the TOKENS condition, we add symbolic and intrinsically worthless objects— or "tokens" — which cannot be redeemed for CUs or dollars, and have no reference to outside currencies. This expands the strategy space, by introducing the possibility of trading help through a direct mechanism (see Table A1 in Appendix A). The supply of tokens is fixed: in round one, every consumer has one token and producers have none. This introduces the possibility of fiat monetary exchange. The consumer has three alternative actions: carry over the token to the next round ("Do nothing"); unilaterally "transfer a token"; or "buy help" in exchange for a token. The producer can "give help" or not—as in the CONTROL condition—but can also "sell help" in exchange for a token. Choices are made simultaneously and without communication.

The two possible payoff configuration are the same as in the CONTROL condition. The payoffs are 0 CUs for the producer, and k CUs for the consumer, when the producer helps unconditionally or help is exchanged for a token. Otherwise the payoffs are 6 CUs for the producer, and 3 CUs for the consumer. At the end of each round, a participant observes the outcome in the pair but not the action of the opponent. If a consumer has no tokens, he has no actions to take, and the producer can only choose whether or not helping unconditionally: hence the decision situation is identical to the CONTROL condition. Token holdings are partially observable by the opponent: in every pair, each player can verify whether the opponent has either 0 or at least one token; the exact number is unobservable in order to preserve anonymity and to reduce the cognitive load.

In the TOKENS condition the cooperative equilibrium can also be sustained through a monetary trade convention, where all consumers buy help, and all producers sell help. However, trading tokens for help is theoretically unnecessary to sustain full cooperation. The TOKENS condition neither precludes the adoption of the social norm of cooperation, nor forces participants to use tokens; it simply expands the strategy set, without removing any equilibria of the CONTROL condition or adding more efficient equilibria.

Each session consists of a *Training Phase* (cycles 1-4) and a *Selection Phase* (cycles 5-6). Training Phase interaction exogenously alternates across cycles between partnerships and groups of 12. Instead, the scale of interaction in the Selection Phase is endogenous. Session participants express a preference between partnerships and groups of 12 before cycles 2-5 start, and between partnerships and a group of 24 before cycle 6 starts. The majority of preferences determines the scale of interaction in cycles 5 and 6, respectively (see Section 4 for details).

3 Theoretical considerations

In this Section, we show the existence of a fully cooperative equilibrium under both experimental conditions. These considerations apply to economies of any size, due to the availability of public monitoring of defections.

3.1 Control condition

Define a generic meeting in round t by $\{i, o_i(t)\}$, where i is a player and $o_i(t)$ is the other player in the pair. To support full cooperation as an equilibrium outcome we consider a trigger strategy described by an automaton with two states, I and II.

Definition 1 (Cooperative strategy). *At the start of any round t, player i can be in state I or II, and takes actions only as a producer. As a producer, player i selects "give help" in state I, and "no help" in state II. In $t = 1$, the state is I; in all $t \geq 1$*

(i) *if player i is in state I, then i moves to state II in $t + 1$ only if some producer in the group—not necessarily the producer in $\{i, o_i(t)\}$—chooses "no help." Otherwise, player i remains in state I;*

(ii) *there is no exit from state II.*

If this strategy is commonly adopted, then it is called a social norm. This social norm can support full cooperation in groups of any size. Intuitively, this norm consists of a rule of cooperation and rule for punishment: (i) Cooperation: if the player is a producer, then he selects "give help"; (ii) Punishment: if a defection is observed in the group, then the player will always select "no help" whenever he is a producer. The central feature of this norm is that the entire group participates in enforcing defections. In equilibrium no one defects. In what follows we show that, under this social norm, cooperation is a sequential equilibrium if the players' discount factor β is sufficiently large.

Proposition 1. *If* $\beta \geq \beta^* := \dfrac{d}{k-d+l}$, *then the strategy in Definition 1 supports full cooperation in sequential equilibrium.*

The proof is constructed by means of two lemmas. We start by calculating equilibrium payoffs. Recall that players deterministically alternate between the two roles of producer and consumer. Hence, in equilibrium players earn k every other round. Discounting starts on date T, when the random termination rule starts; hence, only payoffs from rounds $t = T+1$ (included) are discounted at rate β. Let $v_s(t)$ denote the equilibrium payoff at the start of $t = 1, 2, \ldots$ to a player who is in role $s = 0, 1$, where $0 =$producer and $1 =$consumer.

Lemma 1. *Fix* $T \geq 1$ *and* $\beta \in (0, 1)$. *In the cooperative equilibrium we have* $v_1(t) > v_0(t)$ *for all* $t = 1, 2, \ldots$, *where for* $h = 1, 2 \ldots$,

$$v_s(t) := \begin{cases} k \times \dfrac{T-t}{2} + v_s, & \text{if } \; T-t = 2h \\ k \times \dfrac{T-t+1}{2} + \beta v_s, & \text{if } \; T-t = 2h-1, \\ v_s, & \text{if } \; T-t \leq 0, \end{cases} \tag{1}$$

and

$$v_s := \frac{\beta^{1-s}}{1-\beta^2} \times k \qquad \text{for } s = 0, 1.$$

Proof. See Appendix. □

The equilibrium payoff is found by substituting $t = 1$ in expression (1). To determine the optimality of the cooperative strategy we must check two items: (i) in equilibrium no producer has an incentive to defect; (ii) out of equilibrium no producer has an incentive to cooperate. We let $\hat{v}_s(t)$ denote the continuation payoff to a player in role s on date t, off equilibrium.

Consider a generic producer in a round $t \geq 1$. In equilibrium, choosing "give help" is a best response if

$$v_0(t) \geq \hat{v}_0(t). \tag{2}$$

The left-hand-side of the inequality denotes the payoff to a producer who cooperates in the round, choosing "give help." The right-hand-side denotes the continuation payoff on date t if the producer defects in equilibrium (reverting back to playing the social norm in the following round), given that off-equilibrium everyone follows the group punishment rule prescribed by the social norm. Hence, if a defection occurs on t, then every producer selects "no help" from $t+1$ because equilibrium defections are public.

It should be clear that

$$\hat{v}_0(t) = \hat{v}_0 := \frac{d + \beta(d-l)}{1-\beta^2} \qquad \text{if } t \geq T.$$

For $h = 1, 2, \ldots$, the continuation payoff off-equilibrium satisfies

$$\hat{v}_0(t) := \begin{cases} (d + d - l) \times \dfrac{T-t}{2} + \hat{v}_0 & \text{if } \; T - t = 2h \\ (d + d - l) \times \dfrac{T-t+1}{2} + \beta\hat{v}_0 & \text{if } \; T - t = 2h-1, \\ \hat{v}_0 & \text{if } \; T - t \leq 0. \end{cases} \tag{3}$$

Off equilibrium payoffs are independent of the size of the group N since producers defect forever after seeing a defection.

Lemma 2. *Fix $T \geq 1$ and $\beta \in (0,1)$. If $\beta \geq \beta^* := \dfrac{d}{k-d+l}$, then $v_0(t) \geq \hat{v}_0(t)$ for all $t \geq 1$.*

Proof. See Appendix. □

Given that everyone else follows the strategy in Definition 1, it is always individually optimal to punish out of equilibrium, because "no help" is the dominant action when everyone forever defects.

Note that $\hat{v}_s(1)$ is the payoff associated to infinite repetition of the static Nash equilibrium (every producer chooses "no help"), which is always an equilibrium of the repeated game. The condition $\beta \geq \beta^*$ is therefore necessary and sufficient for existence of a cooperative equilibrium because it ensures that players earn payoffs above those guaranteed by defecting in any round. The condition $\beta \geq \beta^*$ does not guarantee that cooperation will be realized because many equilibria exist in the game. Given the experimental parameters, $\beta^* = 0.4$ in large groups and $\beta^* = 0.5$ in partnerships. Hence, if participants are risk-neutral, then the fully cooperative equilibrium exists in the Control condition, in groups of any size, because in the experiment $\beta = 0.75$.

3.2 Tokens condition

All the equilibria that exist in the Control condition also exists in the Tokens condition, because tokens are intrinsically worthless, do not restrict action sets, and can be ignored. In addition, cooperation can be supported as an equilibrium by means of monetary trade.

Definition 2 (**Monetary trade strategy**). *In any round t, after any history, if the player has no tokens, she has no action to take as a consumer and chooses "sell help" as a producer. If the player has some tokens, she chooses "buy help" as a consumer and selects "no help" as a producer.*

We call *monetary trade* the outcome that results when everyone adopts the strategy in Definition 2. Here, help is only given quid-pro-quo in exchange for a token. Otherwise, help is not given. Monetary trade sustains the socially efficient allocation on the same parameter set as the social norm. The reason is that in monetary equilibrium all encounters support trade due to the deterministic alternation between roles.

Proposition 2. *If $\beta \geq \beta^*$, then the monetary trade strategy in Definition 2 supports full cooperation in equilibrium.*

Proof. See Appendix. □

4 Experimental procedures

Sessions consisted of six cycles that lasted an average of 18.7 rounds of play. Cycle duration varied across cycles and sessions, but was identical for all groups in the same session. The size of groups in the four cycles of the Training Phase followed either the order 2-12-2-12 or 12-2-12-2. During the Selection Phase (cycles 5-6) the group size was determined endogenously by majority rule as follows. Before cycles 2-5, participants expressed a preference for groups of size 2 or 12. These preferences were all counted in order to select the group size for cycle 5. Before cycle 6, participants expressed a preference for groups of size 2 or 24 in cycle 6.

The experiment involved 384 undergraduate volunteers, each of whom participated in only one session. Between September and October 2014, we ran 8 sessions for the Control and 8 for the Tokens condition, with 24 participants each. The conversion rate was 1CUs=US$0.20. Sessions lasted 2.5 hours on average, and participants were paid on average US$26.73 in cash, privately, at the end of the session. Only one randomly selected cycle from the session was paid.

In each round, participants observed own payoff, tokens held (Tokens conditions), and if all producers in their group helped or not. Participants had continual access to such feedback from all past rounds of the cycle. They were informationally isolated across groups and no one interacted with any person met in previous cycles (except possibly in cycle 6). The experiment was programmed using the software z-Tree (Fischbacher, 2007) and ran at the laboratory in the Economic Science Institute at Chapman University. No eye contact was possible. Participants' demographic characteristics were collected through an end-of-session anonymous survey.

The experimenter read the instructions and participants followed on individual copies. The instructions adopted a neutral language: the words "help," "cooperation," and "money" were never used (see Appendix B). Before the Training Phase, participants took a quiz with ten questions testing their understanding of the instructions, and received 25 cents for each correct answer.

5 Results

Before presenting the two main results, based on the behavior in the Selection Phase, we provide an overview of behavior in the Training Phase.

In the Training Phase, average cooperation rates were higher in partnerships than in large groups (69.4% vs. 50.0% in Control; 67.6% vs. 48.8% in Tokens; p-value < 0.001; Table A2 in Appendix A). However, partnerships did not create more surplus than large groups because of their lower potential (efficiency was 46.2% vs. 50.0% in Control, and 46.1% vs. 48.8% in Tokens; p-value > 0.1; Table A2 in Appendix A).

While efficiency levels were similar across conditions, observed actions differed. Whenever monetary trade was possible in Tokens, consumers overwhelmingly chose "buy help" (81.8%) and producers mostly chose "sell help" (63.4%). Help was rarely given when consumers had no token (18.3%); this contrasts with behavior observed under the same decisional situation in CONTROL, where "give help" was the predominant choice (59.7%). These differences across conditions during the Training Phase influenced the participants' willingness to widen the scale of interaction in the Selection Phase.

Result 1. *Money promotes the formation of large groups.*

Participants in TOKENS selected to interact in large groups more frequently than in CONTROL (Table A3 in Appendix A). In the Selection Phase the difference is statistically significant (55.8% vs. 39.3%, p-value 0.014; Table A4 in Appendix A).

Two elements of the experience during the Training Phase determined the participants' disposition to widen the scale of interaction: experiences of exploitation by free-riders and of full cooperation. We measure exploitation as the *help imbalance*, which is the difference between how frequently a participant gave and received help in a cycle (Figure 1). The imbalance goes from -1 to 1: it is negative for someone who gave help more frequently than she received it, positive otherwise. The imbalance is zero for someone who equally gave and received help, which occurs with full cooperation (dark bars in Figure 1), no cooperation, and some instances of partial cooperation.

Participants are unsure which strategy others will use. This *strategic uncertainty* (Van Huyk et al., 2007) implies that those who help in order to establish a cooperative norm may not receive help in future rounds. This exploitation hazard is captured by the dispersion of help imbalance across participants; Figure 1 reveals that it was greater in large groups than partnerships. A zero imbalance was more frequently attained in partnerships than large groups

(0.563 vs. 0.156 in Control, p-value < 0.001, 0.609 vs. 0.299 in Tokens, p-value < 0.001; Table A5 in Appendix A), and also in Tokens than Control, especially in large groups where it was almost twice as frequent (0.299 vs. 0.156, p-value < 0.001; Table A6 in Appendix A). The possibility to trade tokens for help *quid-pro-quo* offers protection against exploitation hazards: a participant must transfer a token to receive help, and the only way to obtain tokens is to help others.

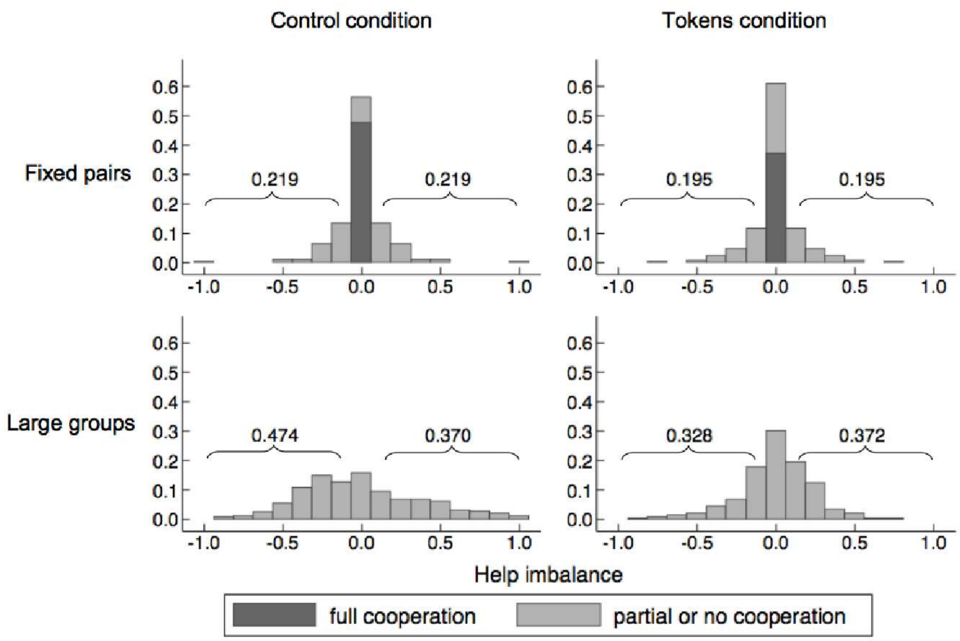


Figure 1: **The distribution of help imbalance.**

Notes: Help imbalance is the difference between how frequently a participant gave and received help in a cycle. Participants who gave more (less) help than they received have a negative (positive) imbalance. Data from rounds 1-16, Training Phase only. Four observations per participant.

The probit regression in Table 1 estimates how the desire to widen the scale of interaction is affected by *help imbalance* and *full cooperation* experienced in partnerships and groups of strangers. The dependent variable takes value 1 when a participant expressed a preference for large groups of 12 and 24 (cycles 5 and 6, respectively) and zero otherwise. This regression reveals that help imbalance in large groups is crucial. Those who received more help than they gave, i.e. the free riders, were more willing to interact in large groups, where they could not be directly targeted for punishment. Instead, those exploited by free riders were more likely to opt for the safety experienced in partnerships.

The share of free riders was similar across conditions (37.0% vs. 37.2%, Figure 1), but more participants were exploited in Control than in Tokens (47.4% vs. 32.8%, Figure 1). This suggests that the differential experience of exploitation was behind the weaker desire to expand the scale of interaction in Control.

Table 1: **Money reduces strategic uncertainty and promotes the formation of large groups**.

Dependent variable: Individual preference for large groups (0=partnerships)	marg. eff.	S.E
Tokens condition x cycle 5 (dummy)	0.112	(0.075)
Tokens condition x cycle 6 (dummy)	0.153*	(0.079)
Cycle 6 (dummy)	-0.086	(0.053)
Training phase		
Help imbalance - partnerships	0.155	(0.149)
Help imbalance - large groups	0.304***	(0.072)
Full cooperation - partnerships (dummy)	-0.183***	(0.062)
Controls	Yes	
N	768	

Notes: Probit regressions on preferences for large groups of 12 and 24 (cycles 5 and 6, respectively). The regression includes controls for order effects in the Training Phase, sex, the number of right answers and response time in a comprehension test on the instructions. Marginal effects are computed at the regressors' mean value (at zero for dummy variables). One observation per person per cycle. Data from rounds 1-16 only.

Large groups never attained full cooperation, while several partnerships attained it (37.0% in Tokens and 47.4% in Control, Figure 1). Those who were in a cooperative partnership were less willing to widen the scale of interaction than those in other partnerships (the regressor "Full cooperation" in Table 1 is negative and highly significant). Partners attained full cooperation more frequently in Control than in Tokens (p-value=0.08; Table A7 in Appendix A), which suggests that the possibility of relying on monetary trade displaced norms of voluntary help (Camera et al., 2013). This is a second reason behind the weaker desire to expand the scale of interaction observed in Control compared to Tokens.

The "Tokens condition" dummies capture the residual difference across conditions in participants' willingness to widen the scale of interaction. The estimated coefficient is positive and significant only for cycle 6, when groups of 24 could be formed, but not for cycle 5, where the size of large groups was 12, as in the Training Phase. A reason may be that participants never experienced interaction in groups of 24 before. In this case the presence of tokens made a difference, because participants realized that monetary trade reduced strategic uncertainty. That is why participants in Tokens condition were more willing to select large groups.

Recall that, by design, cooperative large groups create 50% more surplus than cooperative partnerships, thus raising efficiency from 67% to 100%. But uncooperative large groups may also destroy surplus relative to partnerships. Maximum efficiency could be attained in any condition by simply taking turns at helping others—it did not require the exchange of tokens. By contrast, experimental data reveal different patterns across conditions.

Result 2. *Without tokens, endogenously-formed groups achieved lower efficiency than partnerships. The converse held true with tokens.*

In the experiment, wide disparities emerged between Tokens and Control in the Selection Phase—when the group size was endogenous. In Control, efficiency fell when participants *chose* to widen the scale of interaction. In Tokens, the opposite held true.

Table 2: **How monetary trade and group size influence efficiency.**

Dependent variable: efficiency	coefficient	S.E
Control × large	-0.121*	(0.064)
Tokens × partnership	-0.021	(0.049)
Tokens × large	0.101**	(0.042)
Cycle 6 (dummy)	0.014	(0.036)
Constant	0.566***	(0.034)
N	32	
R-squared	0.343	

Notes: One observation per session per cycles 5 and 6. The default condition is Control, partnerships. Linear regression on realized efficiency on a set of dummy variables that include the interaction between condition and group size. Data from rounds 1-16, Selection Phase only.

The linear regression in Table 2 measures how efficiency varies with group size and availability of tokens. The dependent variable is the realized efficiency in a cycle, in a session. In Tokens large groups attained significantly greater efficiency than partnerships (67.2% vs. 55.4%, two-sided Wald test on the estimated coefficients, p-value=0.027). The opposite was true in Control (45.0% vs. 57.3%). Large groups also attained greater efficiency in Tokens than Control (two-sided Wald test on the estimated coefficients, p-value=0.002). In partnerships, instead, efficiency levels were similar across conditions.

The distribution of efficiency across large groups gives us an additional measure of how monetary trade affected economic performance. In the Tokens condition, 16 large groups were formed in the Selection Phase; half of these groups exceeded the 67% efficiency threshold of partnerships (Figure 2).

Instead, in the CONTROL condition this happens only in 1 of the 5 large groups that were formed. Tokens are intrinsically worthless, so their availability did not raise efficiency *per se*. Tokens merely offered participants an additional way to support cooperation among strangers. In fact, efficiency systematically improved with the intensity of monetary trade (Figure 2). Those groups that established a solid convention of trade attained efficiency above partnerships, while those where the convention of monetary trade failed to take hold, attained efficiency below that of the average partnership.This positive relation holds for the Training and Selection Phases.

Linear regressions on average payoff per-round attained by participants in large groups (Selection Phase) show a positive and significant effect of the intensity of monetary trade at the group and at the individual level (Table 3). The dependent variable is the average payoff per-round for a participant in a large group (0,1, or 2 observations per participant). The regressors include two variables related to the intensity of monetary trade: at the group and individual level.[1]

6 Conclusions

Economies prosper when their members move beyond local exchange and cooperate with outsiders in the creation of wealth. But widening the scale of cooperation presents formidable challenges: interaction becomes impersonal and reciprocity unfeasible, as trust and social norms are weakened. This paper studies a setup where money is theoretically inessential, and shows that

[1]The intensity of monetary trade at the group level is measured as the overall frequency of the actions "sell help" and "buy help"; at the individual level it is measured as the frequency of the actions "sell help" and "buy help" in all rounds in which monetary trade was possible (i.e. the consumer had at least one token).

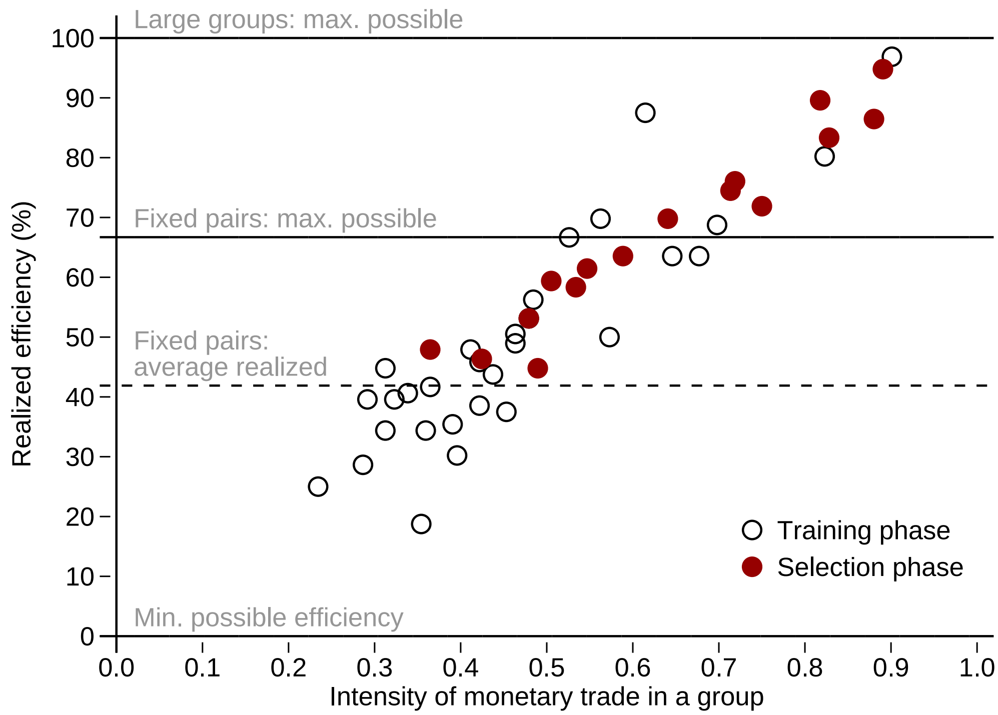


Figure 2: **A strong monetary trade convention boosts efficiency in large groups.**

Notes: One observation per group, per cycle. The intensity of monetary trade is the overall frequency of the actions “sell help” and “buy help.” Minimum efficiency (0%) is obtained when help is never given. Maximum efficiency in fixed-pairs is 67%, which is obtained when help is always given; in large groups it is 100%. Realized efficiency in partnerships (41.7%) is computed aggregating data from the Training and Selection Phases (dashed line). Data from rounds 1-16, Tokens condition only.

Table 3: **Intense monetary trade raises payoffs in large groups.**

Dependent variable: average per round profit	coefficient	S.E
Intensity of monetary trade		
at the group level	3.421***	(0.189)
at the individual level	0.916***	(0.207)
Cycle 6 (dummy)	-0.075	(0.045)
Controls		
Constant	3.782***	(0.507)
N	240	
R-squared	0.413	

Notes: Linear regression on data for large groups in the Selection Phase, TOKENS condition. The dependent variable is the average payoff per-round for a participant in a large group. Among the regressors we include a dummy taking value one for cycle 6. The regression includes controls for order effects in the Training Phase, sex, the number of right answers and response time in a comprehension test on the instructions. Standard errors are robust for clustering at the session level. Data from rounds 1-16 only.

stable monetary systems are behaviorally crucial to expand the scale of cooperation.

In the experiment, participants faced an intertemporal cooperative task and could restrict interaction to partnerships, or expand it to large groups of strangers where the returns from cooperation were higher. When participants could trade symbolic tokens for cooperation, large groups spontaneously emerged, and created more surplus than partnerships. Instead, large groups rarely emerged without money and, when they did, free-riding prevailed.

Two remarks are in order. First, the exchange of symbolic objects is not necessary to sustain cooperation, in groups of any size. Second, the mere presence of tokens in the economy does not mechanically guarantee a cooperative outcome; participants in large groups must also be able to establish a strong convention of monetary trade.

This study also uncovers a key behavioral advantage of monetary exchange in promoting large-scale cooperation: it offers protection from strategic uncertainty. Participants are unsure about what others will choose because of equilibrium multiplicity ranging from zero to full cooperation. This uncertainty is the central stumbling block to widening the scale of cooperation. Participants realize that opportunistic temptations are stronger in large groups, because free-riders cannot be directly targeted for punishment, which raises uncertainty. In contrast, partners can rely on reciprocity and reputation and can also more easily coordinate on a common strategy compared to strangers in a large group. Choosing the scale of interaction thus hinges on the perceived trade-off between a partnership's low but predictable payoff, and the possibly higher but unpredictable payoff of large groups.

A monetary trade convention reduces strategic uncertainty because it prevents free-riders from exploiting cooperators: producers help only in exchange for a token, and only consumers who helped in the past have a token. Hence, the monetary strategy supports full cooperation through a unique rule of behavior on- and off-equilibrium. In contrast, a social norm of mutual help has two separate components: cooperation and punishment, and a coordination challenge arises from the existence of multiple ways to punish.

This insight open new avenues for theoretical advances. A monetary trade convention empirically outperforms a social norm of mutual help because it facilitates off-equilibrium coordination. However, the problem of coordination out-of-equilibrium is typically neglected in the theory. This consideration could lead to new theoretical models with sharper empirical predictions.

References

T.K. Ahn, R. M. Isaac, and T. C. Salmon, 2009. Coming and going: Experiments on endogenous group sizes for excludable public goods. *J. Pub. Econ.* 93(1-2), 336-351.

M. Bigoni, G. Camera, and M. Casari, 2013. Money is More than Memory. Working Paper No. 14-17, Economic Science Institute, Chapman University.

G. Camera, and M. Casari, 2014. The Coordination Value of Monetary Exchange: Experimental Evidence. *Am. Econ. J.: Micro.*, 6(1), 290-314.

G. Camera, M. Casari, and M. Bigoni, 2013. Money and trust among strangers. *Proc. Natl. Acad. Sci. U.S.A.*, 110(37), 14889-14893.

P. Dal Bó, and G. Fréchette, 2011. The Evolution of Cooperation in Infinitely Repeated Games: Experimental Evidence. *Am. Econ. Rev.*, 101(1), 411-429.

J. Duffy and D. Puzzello, 2014. Gift Exchange versus Monetary Exchange: Theory and Evidence. *Am. Econ. Rev.*, 104(6), 1735-1776.

G. Ellison, 1994. Cooperation in the Prisoner's Dilemma with Anonymous Random Matching, *Review of Economic Studies*, 61(3), 567-588.

E. Fehr and S. Gächter, 2000. Fairness and retaliation: the economics of reciprocity, *Journal of Economic Perspective*, 14(3), 159-181.

U. Fischbacher, 2007. Z-Tree: Zurich toolbox for ready-made economic experiments, *Experimental economics*, 10(2), 171-178.

G. Fréchette and S. Yuksel, 2014. Infinitely Repeated Games in the Laboratory: Four Perspectives on Discounting and Random Termination. Working paper, New York University.

J. W. Friedman, 1971. A non-cooperative equilibrium for supergames. *Rev. Econ. Stud.*, 1-12.

S. Gächter and B. Herrmann. 2011. The limits of self-governance when cooperators get punished: Experimental evidence from urban and rural Russia. *European Economic Review* 55(2), 193-210.

A. Greif, 2006. The Birth of Impersonal Exchange: The Community Responsibility System and Impartial Justice. *J. Econ. Persp.*, 20(2), 221-236.

M. Kandori, 1992. Social Norms and Community Enforcement, *Review of Economic Studies*, vol. 59(1), 63-80.

E. O. Kimbrough, V. Smith and B. J. Wilson, 2008. Historical Property Rights, Sociality, and the Emergence of Impersonal Exchange in Long-Distance Trade. *Am. Econ. Rev.*, 98(3), 1009-1039.

P. Lian, and C. R. Plott, (1998) General equilibrium, markets, macroeconomics, and money in a laboratory experimental environment. *Econ. Th.*, 12 (1), 21-75.

R. S. Lopez, 1971. *The Commercial Revolution of the Middle Ages, 950-1350.* Englewood Cliffs, N.J., Prentice-Hall.

K. A. McCabe, 1989. Fiat money as a store of value in an experimental market. *Journal of Economic Behavior & Organization*, 12(2), 215-231.

P. R. Milgrom, D. C. North, and B. R. Weingast, 1990. The Role of Institutions in the Revival of Trade: The Law Merchant, Private Judges, and the Champagne Fairs. *Econ. and Pol.*, 2(1), 1-23.

D.C. North, 1991. Institutions. *J. Econ. Persp.*, 5(1), 97-112.

T. R. Palfrey, and H. Rosenthal, 1994. Repeated Play, Cooperation and Coordination: An Experimental Study. *Rev. Econ. Stud.*, 61(3), 545-565.

C. Plott, 2001. Equilibrium, Equilibration, Information and Multiple Markets: From Basic Science to Institutional Design. Nobel Symposium on Behavioral and Experimental Economics, Stockholm, Sweden.

A. E. Roth, and K. Murnighan. 1978. Equilibrium behavior and repeated play of the prisoner's dilemma. *J. Math. Psy.*, 17, 189-98

R. Townsend, 1980. Models of money with spatially separated agents. In: Kareken, J., Wallace, N. (eds.) *Models of monetary economies*, 265-303. Minneapolis: Federal Reserve Bank of Minneapolis.

J. Van Huyk, R. Battalio, and R. Beil, 1990. Tacit Coordination Games, Strategic Uncertainty, and Coordination Failure. *Am. Econ. Rev.* 80, 234-248.

R. A. Weber, 2006. Managing Growth to Achieve Efficient Coordination in Large Groups. *Am. Econ. Rev.*, 96(1), 114-126.

Appendix

Proof of Lemma 1. To prove the result we consider the two cases $t \geq T$ and $t < T$ separately.

Let v_s denote the equilibrium payoff at the start of round $t \geq T$ to a player who is in role $s = 0, 1$ (0 identifies a producer). It holds that

$$v_s := \frac{\beta^{1-s}}{1-\beta^2} \times k \qquad \text{for } s = 0, 1.$$

The payoff is time invariant due to the stationary alternation between roles.

Now consider round $t < T$. Given the proposed strategy those who are initial consumers earn k on odd dates ($t = 1, 3, \ldots$) and zero otherwise; initial producers earn k on even dates ($t = 2, 4, \ldots$) and zero otherwise. Hence, knowing whether $T - t$ is odd or even matters. For $j, h = 1, 2 \ldots$ and $s = 0, 1$ it holds that

$$v_s(t) = \begin{cases} k \times \dfrac{T-t}{2} + v_s & \text{if } T - t = 2h \\ k \times \dfrac{T-t+1}{2} + \beta v_s & \text{if } T - t = 2h - 1. \end{cases}$$

The continuation payoff $v_s(t)$ has two components. The first sums up the round payoffs for all $t \leq T - 1$. The second sums up the round payoffs for all $t \geq T$. It should be clear that $v_s(t)$ is increasing in T for $s = 0, 1$ and it achieves a minimum when $T - t = 1$. Hence, the equilibrium payoff to a player in role $s = 0, 1$ on any date $t \geq 1$ is given by (1). We have $v_1(t) > v_0(t)$ for all t because $v_1 > v_0$ for all $\beta \in (0, 1)$. □

Proof of Lemma 2. The result is obtained by manipulation of the equations in (3). Note that

$$v_0 - \hat{v}_0 = \frac{\beta}{1-\beta^2} \times k - \frac{d + \beta(d-l)}{1-\beta^2} = \frac{\beta}{1-\beta^2} \times (k - 2d + l) - \frac{d}{1+\beta}$$

Now define

$$\begin{aligned} \Delta_0(t) &= v_0(t) - \hat{v}_0(t) \\ &= \begin{cases} (k-2d+l) \times \frac{T-t}{2} + v_0 - \hat{v}_0 & \text{if } \; T-t = 2h \\ (k-2d+l) \times \frac{T-t+1}{2} + \beta(v_0 - \hat{v}_0) & \text{if } \; T-t = 2h-1, \\ v_0 - \hat{v}_0 & \text{if } \; T-t \leq 0. \end{cases} \end{aligned}$$

It is immediate that $\Delta_0(t = T - 2h) > \Delta_0(t \geq T)$; note that $k - 2d + l > 0$ by assumption. Also, $\Delta_0(t = T - 2h + 1) > \Delta_0(t \geq T)$; to prove it insert $h = 1$ (the most stringent case), rearrange the inequality, and then insert the expression for $v_0 - \hat{v}_0$, to obtain the inequality $k - 2d + l > -d$.

Given that the minimum value of $\Delta_0(t)$ is achieved for $T - t \leq 0$, then (2) holds for all t whenever

$$\begin{aligned} 0 \;\; &\leq \;\; v_0 - \hat{v}_0 = \frac{\beta}{1-\beta^2} \times (k - 2d + l) - \frac{d}{1+\beta} \\ &\Leftrightarrow \;\; \beta \geq \beta^* := \frac{d}{k-d+l}. \end{aligned}$$

Note that $\beta^* < 1$ because $k - 2d + l > 0$ by assumption. □

Proof of Proposition 2. Conjecture that monetary trade is an equilibrium. Consider a player with $s = 0, 1$ tokens at the start of a round. In equilibrium, a consumer has a token and a producer has none. Hence, the probability that a consumer with a token meets a producer without tokens is 1. Denote by $v_s(t)$ the equilibrium continuation payoff. Because the consumption pattern is the same as under the social norm, in monetary equilibrium it holds that $v_s(t)$ corresponds to the functions defined in (1).

Now consider deviations. We start by proving that a consumer does not deviate in equilibrium, refusing quid-pro-quo exchange for help. In round $t \geq 1$ let $\beta_t = 1$ if $t < T$ and $\beta_t = \beta$ otherwise. Denote by $\tilde{v}_1(t)$ the payoff in t to a consumer who defects by refusing to spend money in t. Using recursive arguments we have

$$\begin{aligned}\tilde{v}_1(t) &= d - l + \beta_t[d + \beta_{t+1}v_1(t+2)] \\ &< k + \beta_t[0 + \beta_{t+1}v_1(t+2)] = v_1(t).\end{aligned}$$

The inequality holds for any β_t because $k > d + d - l$ by assumption. To understand the inequality consider the first line. Defecting in t generates payoff $d-l$ instead of k, and in $t+1$ the player will be a producer *with* money, reverting back to playing the monetary strategy (unimprovability criterion). Hence, she will refuse to sell for another token because she already has one; this is optimal because (i) acquiring an additional token costs her d and (ii) she has already one token to spend. hence, in $t+2$ the player becomes a consumer with money and the distribution of tokens is back at equilibrium. In summary, following a unilateral deviation in t by a consumer, the group is back on the equilibrium path in round $t+2$.

Now we prove that if $\beta \geq \beta^*$, then a producer in equilibrium would not want to deviate in any t, refusing to help for a token. Denote by $\tilde{v}_0(t)$ the payoff in t to a producer who defects by refusing to accept money in t. Using recursive arguments, we have

$$\begin{aligned}\tilde{v}_0(t) &= d + \beta_t[d - l + \beta_{t+1} v_0(t+2)] \\ &< 0 + \beta_t[k + \beta_{t+1} v_0(t+2)] = v_0(t).\end{aligned}$$

The inequality holds for any $\beta_t \geq \beta^*$ because $k > d + d - l$ (if $\beta_t = 1$); if $\beta_t = \beta$, then we need $\beta \geq \beta^*$. The first line of the inequality shows that defecting in t generates payoff d instead of 0. In $t + 1$ the player is a consumer *without* money; she cannot buy help—since everyone follows the monetary strategy—and earns $d - l$. In $t + 2$ she is a producer without money and the distribution of tokens is back at equilibrium. Hence, after a unilateral deviation in t by a producer, the group is back in equilibrium in round $t + 2$. □

Supplementary information
not for publication

A Additional tables

		Producer		
		No help	*Give help*	*Sell help*
	Do nothing	$d-l, d$	$k, 0$	$d-l, d$
Consumer	*Transfer a token*	$d-l, d^\star$	$k, 0^\star$	$k, 0^\star$
	Buy help	$d-l, d$	$k, 0^\star$	$k, 0^\star$

Table A1: The stage game in the Tokens condition

Notes: The notation $\star$ indicates that the producer receives a token from the consumer. In the experiment $d = 6, l = 3, k = 15$ in partnership and $k = 18$ in large groups.

Table A2: **How money and group size influence efficiency.**

	Model 1		Model 2	
	Dep. var. = Cooperation		Dep. var. = Efficiency	
	coefficient	S.E	coefficient	S.E
Control × large	-0.194***	(0.040)	0.037	(0.035)
Tokens × partnership	-0.018	(0.040)	-0.012	(0.035)
Tokens × large	-0.206***	(0.040)	0.025	(0.035)
Cycle 2	0.180***	(0.040)	0.155***	(0.035)
Cycle 3	0.212***	(0.040)	0.167***	(0.035)
Cycle 4	0.275***	(0.040)	0.230***	(0.035)
Constant	0.527***	(0.037)	0.325***	(0.033)
N	64		64	
R-squared	0.633		0.463	

Notes: One observation per session, Training Phase only (cycles 1-4). The default condition is CONTROL, partnerships. Linear regressions on the average per-round cooperation rate and efficiency, on a set of regressors that include the interaction between the Condition and group size. Data from rounds 1-16 only. Except for *constant*, all regressors are dummy variables. The difference between coefficients for *Tokens × partnership* and *Tokens × large* is statistically significant in Model 1 (two-sided Wald test, p-value<0.001), but not in Model 2 (two-sided Wald test, p-value =0.289).

Table A3: **Preferences for large groups.**

	CONTROL	TOKENS
Share of preferences for large groups:		
Overall (cycles 2-6)	0.421	0.546
In the Selection Phase		
—*Cycle 5 (groups of 12)*	0.432	0.573
Large groups were formed in	2 in 8 sessions	6 in 8 sessions
—*Cycle 6 (groups of 24)*	0.354	0.542
Large groups were formed in	1 in 8 sessions	4 in 8 sessions

Table A4: **How money affects preferences for large groups.**

Dependent variable: preference for large groups (yes=1)	marg. eff.	S.E
Tokens condition (dummy)	0.176**	(0.072)
Cycle 6 (dummy)	-0.055	(0.034)
Controls		
N	768	

Notes: Probit regression on the preferences for large groups in cycles 5 and 6. The regression includes controls for order effects in the Training Phase, sex, and for the number of right answers and the response time in a comprehension test on the experimental instructions. Marginal effects are computed at the mean of the value of regressors (at zero for dummy variables).

Table A5: **Help imbalance and group size.**

Dependent variable:	Control		Tokens	
no help imbalance (yes=1)	marg. eff.	S.E	marg. eff.	S.E
Large group (dummy)	-0.402***	(0.034)	-0.315***	(0.032)
Cycles 3 and 4 (dummy)	0.119**	(0.056)	0.153***	(0.047)
Controls				
N	768		768	

Notes: Probit regression on the presence of a help imbalance. The regression includes controls for order effects, sex, and for the number of right answers and the response time in a comprehension test on the experimental instructions. Marginal effects are computed at the mean of the value of regressors (at zero for dummy variables). Data from rounds 1-16, Training Phase only.

Table A6: **Help imbalance across conditions.**

Dependent variable:	partnerships		Large groups	
no help imbalance (yes=1)	marg. eff.	S.E	marg. eff.	S.E
Tokens condition (dummy)	0.049	(0.032)	0.134***	(0.036)
Cycles 3-4 (dummy)	0.215***	(0.051)	0.030	(0.029)
Controls				
N	768		768	

Notes: Probit regression on the presence of a help imbalance. The regression includes controls for order effects, sex, and for the number of right answers and the response time in a comprehension test on the experimental instructions. Marginal effects are computed at the mean of the value of regressors (at zero for dummy variables). Data from rounds 1-16, Training Phase only.

Table A7: **Full cooperation across conditions.**

Dependent variable:	partnerships		Large groups	
full cooperation (yes=1)	marg. eff.	S.E	marg. eff.	S.E
Tokens condition (dummy)	-0.096*	(0.054)	-0.003	(0.018)
Cycles 3-4 (dummy)	0.373***	(0.053)	0.025	(0.017)
Controls				
N	768		768	

Notes: Probit regression on the experience of full cooperation. The regression includes controls for sex, and for the number of right answers and the response time in a comprehension test on the experimental instructions. Marginal effects are computed at the mean of the value of regressors (at zero for dummy variables). Data from rounds 1-16, Training Phase only.

B Instructions

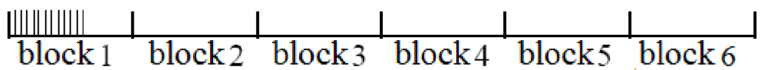
We include copies of the instructions for Control and Tokens conditions, for the case where the Training Phase had group size ordering 2, 12, 2, 12. Instructions for the case where the Training Phase had group size ordering 12, 2, 12, 2 are identical with the obvious change in ordering.

Instructions for the Control condition

This is an experiment in decision-making. You will earn money based on the decisions you and others make in the experiment, and you will be paid in cash at the end of the experiment. Different participants may earn different amounts.

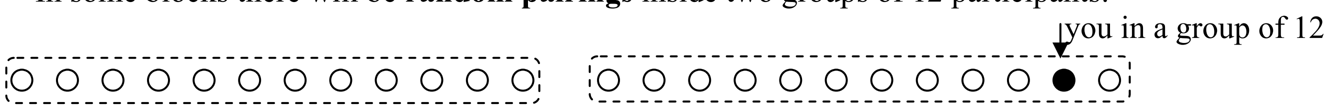
Overview of the experiment

The experiment is divided into **six blocks**. Each block is a separate section with **many periods**:

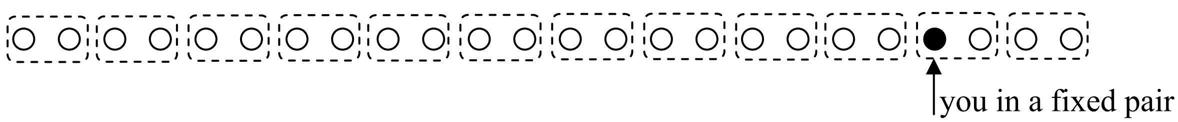


There are 24 participants. At the start of each **block**, a computer program will form groups. In each period of the block you will be paired with someone **in your group** to interact with him or her.

- In some blocks there will be **random pairings** inside two groups of 12 participants:



- In other blocks there will be **fixed pairings** because groups will have only 2 participants:



Groups change in each block so that **you cannot interact with anyone for more than one block**, except, possibly, block 6.

How do you earn money in a period?

You will earn points that depend on your choices and the choices of others **in your group**. Points will be converted into dollars at the end of the session in a manner that we explain later.

In each period you interact with another participant called your **"match."** If you are not in a fixed pair, then your match is **a random person** from your group. Your match will always remain anonymous.

In each pair, one person will be **red** and the other **blue**. The **red** person must choose to execute either outcome **Y** or **Z.** This choice determines the point **earnings** in the pair; the earnings also depend on whether you are in a fixed pair or not, as shown in the following tables:

In a **fixed pair**:

- if **Y** is the outcome: **red** earns **6** points and **blue** earns **3** points.
- if **Z** is the outcome: **red** earns **0** points and **blue** earns **15** points.

In a **random pair:**

- if **Y** is the outcome: **red** earns **6** points and **blue** earns **3** points.
- if **Z** is the outcome: **red** earns **0** points and **blue** earns **18** points.

What happens in each period?

Each **period** has the following timeline:

1. You see your color and you are paired with another participant.
2. You may be called to make a choice.
3. You observe the outcome.
4. The block may continue or may end.

We now discuss these points in detail.

1. Your color and your match

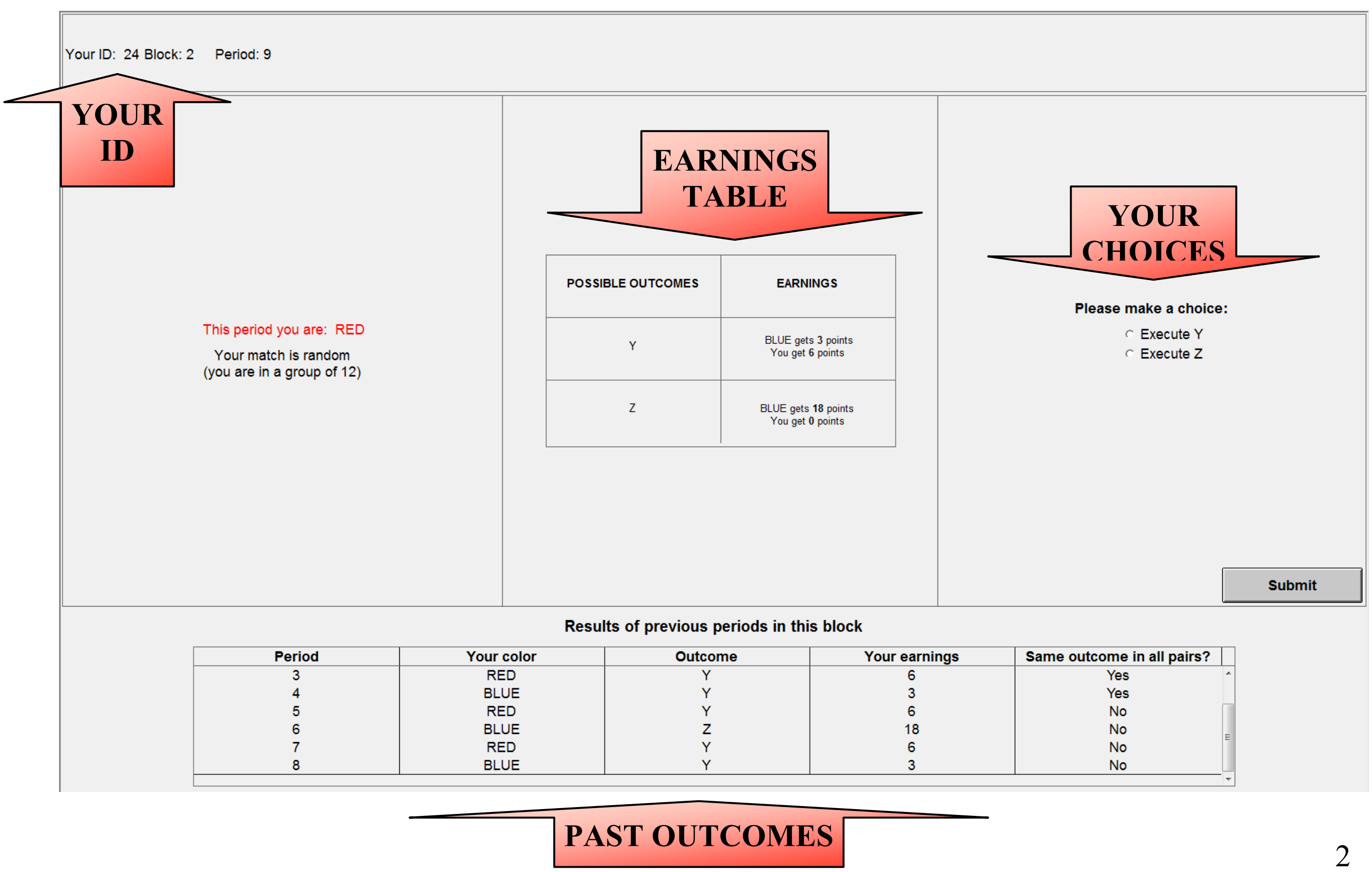
In each period, half of the persons in your group are **red** and the others **blue**. Your initial color is random and then your color alternates from period to period:

- If you are **blue**, then next period you will be **red**;
- If you are **red**, then next period you will be **blue**.

Your match has always a color different than yours. If there are **fixed pairs**, then your match remains the same in each period of the block. Otherwise, your match changes from period to period with a probability greater than 80% because your match can be **anyone from your group** who has a color different than yours. You will never know who you meet.

2. Your choices

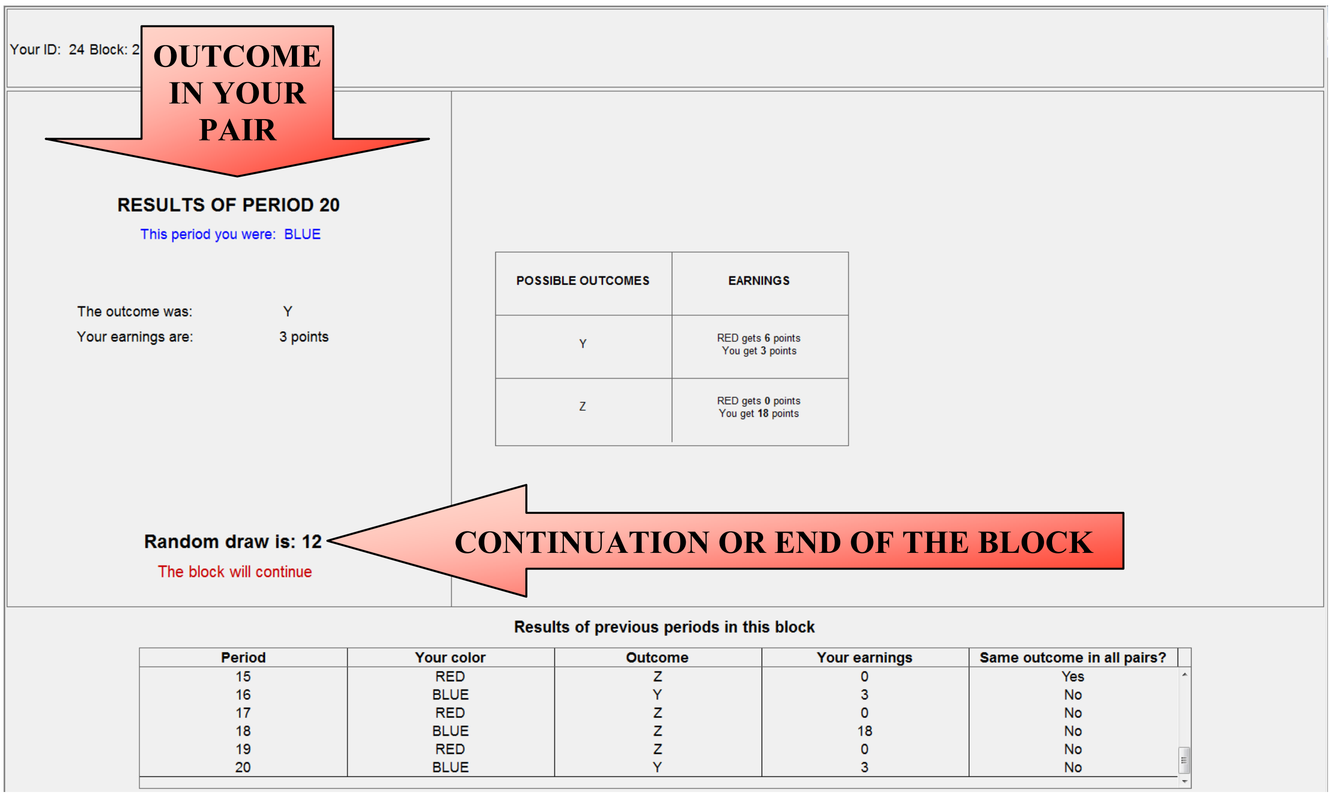
- If you are **blue**, then you have no choice to make.
- If you are **red** , then you must select one of the following two options (see figure below):
  - o **Execute Y**
  - o **Execute Z**



To make your choice, select the relevant option and click the “Submit” button**.** You can review results of **past periods of the block** by scrolling down the table at the bottom of the screen. Each line reports your color, the **outcome** Y or Z in your pair and your **earnings** in a past period**.** The last column reports whether the outcome was the same in all pairs of your group.

3. Outcome of choices

The results for the period will be displayed after everyone makes a choice (see figure below). You will see the **outcome** and the points you **earned**. You can write the results on your record sheet. Results from past periods will again be visible at the bottom of the screen.



4. Ending of a block

Each block has many periods but their number is **unknown** because it is **random**. Hence:

- We never know for sure which period will be the last in a block.
- Some blocks may end up being longer and others shorter.

Each block will have at least 16 periods. From period 16 on, at the end of each period a computer selects with equal probability a number between 1 and 100. If the number selected is less than or equal to 75, then the block will continue. Otherwise, the block will end. This number is the same for every participant.

So: starting in period 16, the block has always a chance to continue. The results screen will inform you whether the block continues or not: you will see the randomly selected number.

Note: The number of past periods does not influence the chance that a block will end. In every period, **every number** between 1 and 100 has an equal chance of being selected. Hence, the chance that a block will end, say, after period 20, is 25%, which is identical to the chance that the block will end after period 16. As soon as a block ends, different groups are formed and a new block starts.

Will there be fixed pairs or random pairs?

In blocks 1 and 3 you will be in a fixed pair. In blocks 2 and 4 you will be randomly paired inside a group of 12 participants. **Recall**: participants that you meet in a block cannot be met in future blocks.

At the end of each of the first four blocks you will be asked to **express your preference** for your match to be either fixed, or randomly assigned from a group of 12 persons. Your preferences will **not** be revealed to others. A computer program will tally all the preferences expressed and in block 5 the program will either form fixed pairs or groups of 12 based on the most preferred option (or "flip a coin," in case of a tie).

Finally, before block 6 starts you will be asked to express a preference for your match to be either fixed, or randomly assigned from **one large group with all 24 participants** (in which case your match most likely changes every period). Once again, the computer program will implement the most preferred option.

Payments

When the session ends, **one** of the six blocks completed will be randomly selected. The points you have earned **in that block** will be converted into dollars: **1 point is worth 20 cents** ($0.20).

To choose the block we publicly roll a six-faced "virtual" die at http://www.bgfl.org/virtualdice.

The numbers on the die's faces identify the blocks. Each block is equally likely to be selected.

Final reminders

- The session is divided into six separate blocks; each block has many periods.
- In each period you meet an anonymous match. If pairs are fixed, your match is the same for the entire block. Otherwise, your match **changes** from period to period with more than 80% probability.
- If you are **red**, then you must choose between outcome Y and Z.
- The points you earn depend on the **outcome** in your pair, Y or Z, and whether pairs are fixed or not.
- Each block has an **uncertain** number of periods. Starting in period 16, there is **always** a 75% chance of an additional period, and a 25% chance of ending.
- In the last two blocks pairs are fixed or random depending on the majority of preferences.
- You **cannot** interact with anyone for more than one block except, possibly, in the last block.

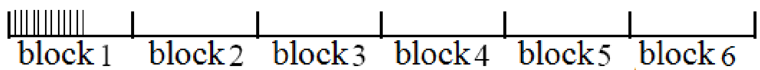
Before we start the experiment, you will be asked to answer ten questions designed to verify your understanding of the instructions. You will receive $0.25 for each question you answer correctly. If you have a question at any time, then please raise your hand and someone will come to answer it.

Instructions for the Tokens condition

This is an experiment in decision-making. You will earn money based on the decisions you and others make in the experiment, and you will be paid in cash at the end of the experiment. Different participants may earn different amounts.

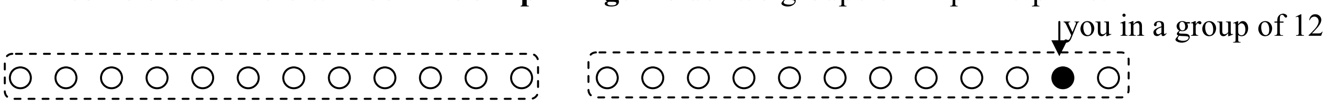
Overview of the experiment

The experiment is divided into **six blocks**. Each block is a separate section with **many periods**:

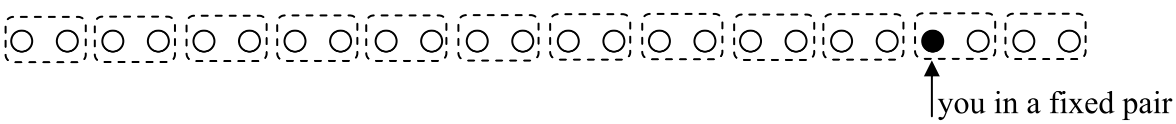


There are 24 participants. At the start of each **block**, a computer program will form groups. In each period of the block you will be paired with someone **in your group** to interact with him or her.

- In some blocks there will be **random pairings** inside two groups of 12 participants:



- In other blocks there will be **fixed pairings** because groups will have only 2 participants:



Groups change in each block so that **you cannot interact with anyone for more than one block**, except, possibly, block 6.

How do you earn money in a period?

You will earn points that depend on your choices and the choices of others **in your group**. Points will be converted into dollars at the end of the session in a manner that we explain later.

In each period you interact with another participant called your **"match."** If you are not in a fixed pair, then your match is **a random person** from your group. Your match will always remain anonymous.

In each pair, one person will be **red** and the other **blue**. The **red** person must choose to execute either outcome **Y** or **Z.** This choice determines the point **earnings** in the pair; the earnings also depend on whether you are in a fixed pair or not, as shown in the following tables:

In a **fixed pair**:

- if **Y** is the outcome: **red** earns **6** points and **blue** earns **3** points.
- if **Z** is the outcome: **red** earns **0** points and **blue** earns **15** points.

In a **random pair:**

- if **Y** is the outcome: **red** earns **6** points and **blue** earns **3** points.
- if **Z** is the outcome: **red** earns **0** points and **blue** earns **18** points.

Tickets

In the first period of each block everyone who is **blue** will receive **1 ticket.** Tickets:

- do not yield points or dollars
- cannot be carried over to the next block
- cannot be redeemed for points or dollars
- can be exchanged with your match as explained below.

What happens in each period?

Each **period** has the following timeline:

1. You see your color and you are paired with another participant.
2. You may be called to make a choice.
3. You observe the outcome.
4. The block may continue or may end.

We now discuss these points in detail.

1. Your color and your match

In each period, half of the persons in your group are **red** and the others **blue**. Your initial color is random and then your color alternates from period to period:

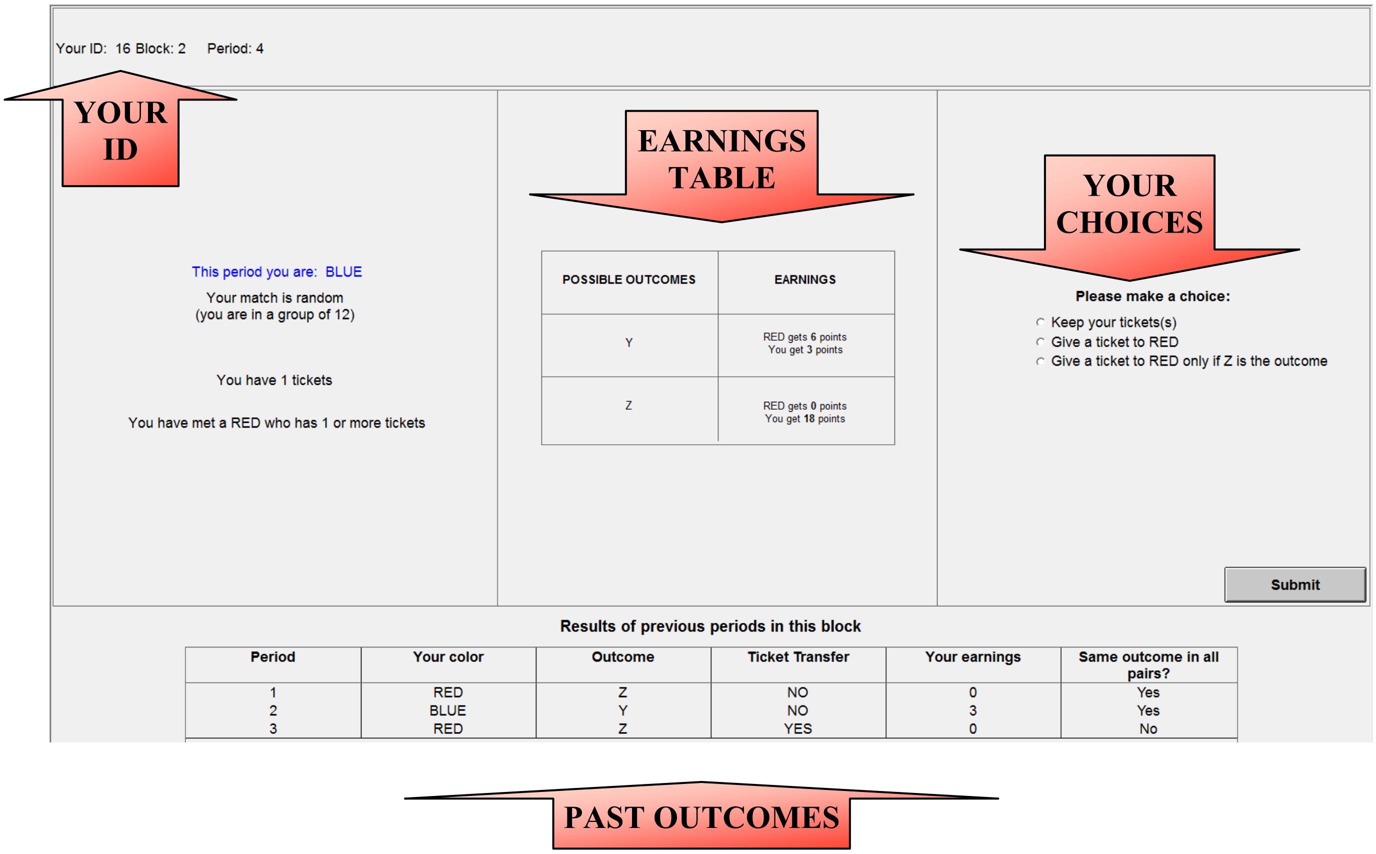
- If you are **blue**, then next period you will be **red**;
- If you are **red**, then next period you will be **blue**.

Your match has always a color different than yours. If there are **fixed pairs**, then your match remains the same in each period of the block. Otherwise, your match changes from period to period with a probability greater than 80% because your match can be **anyone from your group** who has a color different than yours. You will never know who you meet.

2. Your choices

- If you are **blue,** in general you must choose one of three options (see figure below):
  - ***Keep your ticket(s)***
  - ***Give a ticket to red***
  - ***Give a ticket to red only if Z is the outcome.***
    This option **guarantees** that your ticket goes to **red only if red does not** choose outcome **Y**.

Note: If you are **blue** and **do not** have a ticket, then you have **no choice** to make.

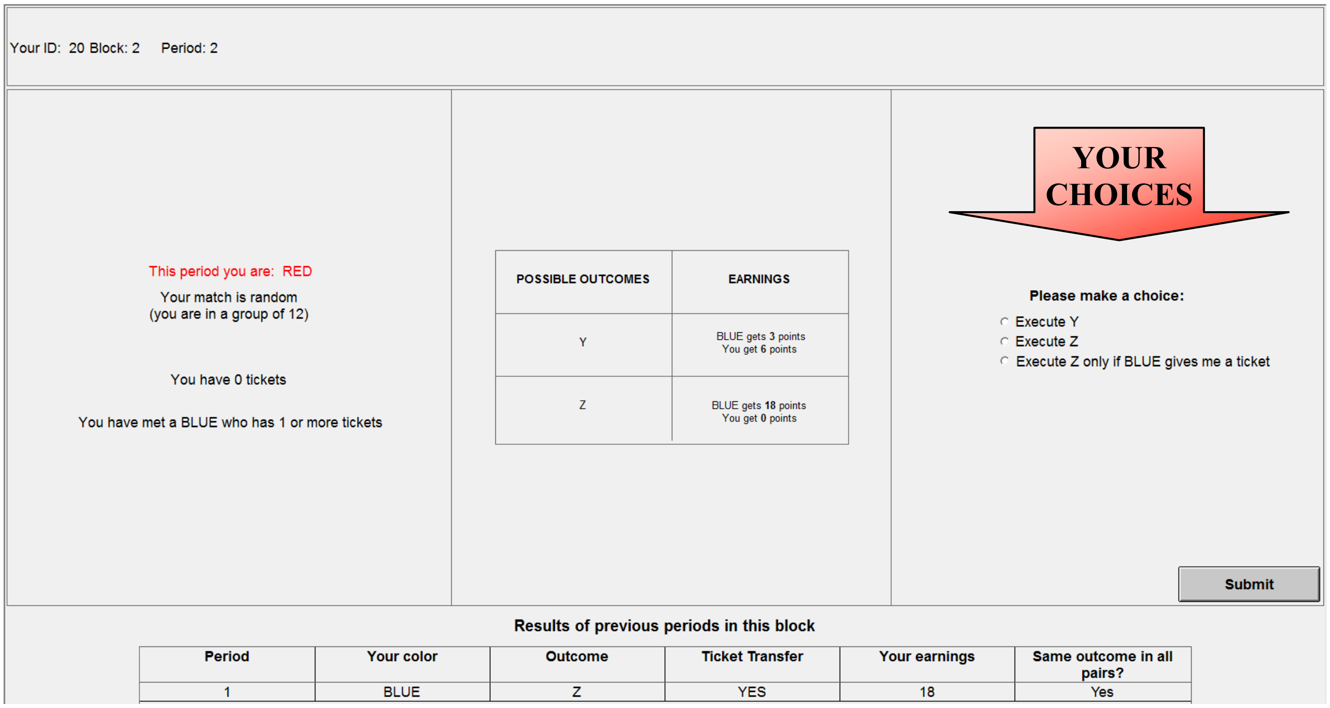


To make your choice, select the relevant option and click the "Submit" button**.** You can review results of **past periods of the block** by scrolling down the table at the bottom of the screen. Each line reports your color, the **outcome** Y or Z in your pair, if there was a ticket transfer in your pair, and your **earnings** in a past period**.** The last column reports whether the outcome was the same in all pairs of your group.

- If you are **red,** in general you must choose one of three options (see figure below):
  - ***Execute Y***
  - ***Execute Z***
  - ***Execute Z only if blue gives me a ticket.***

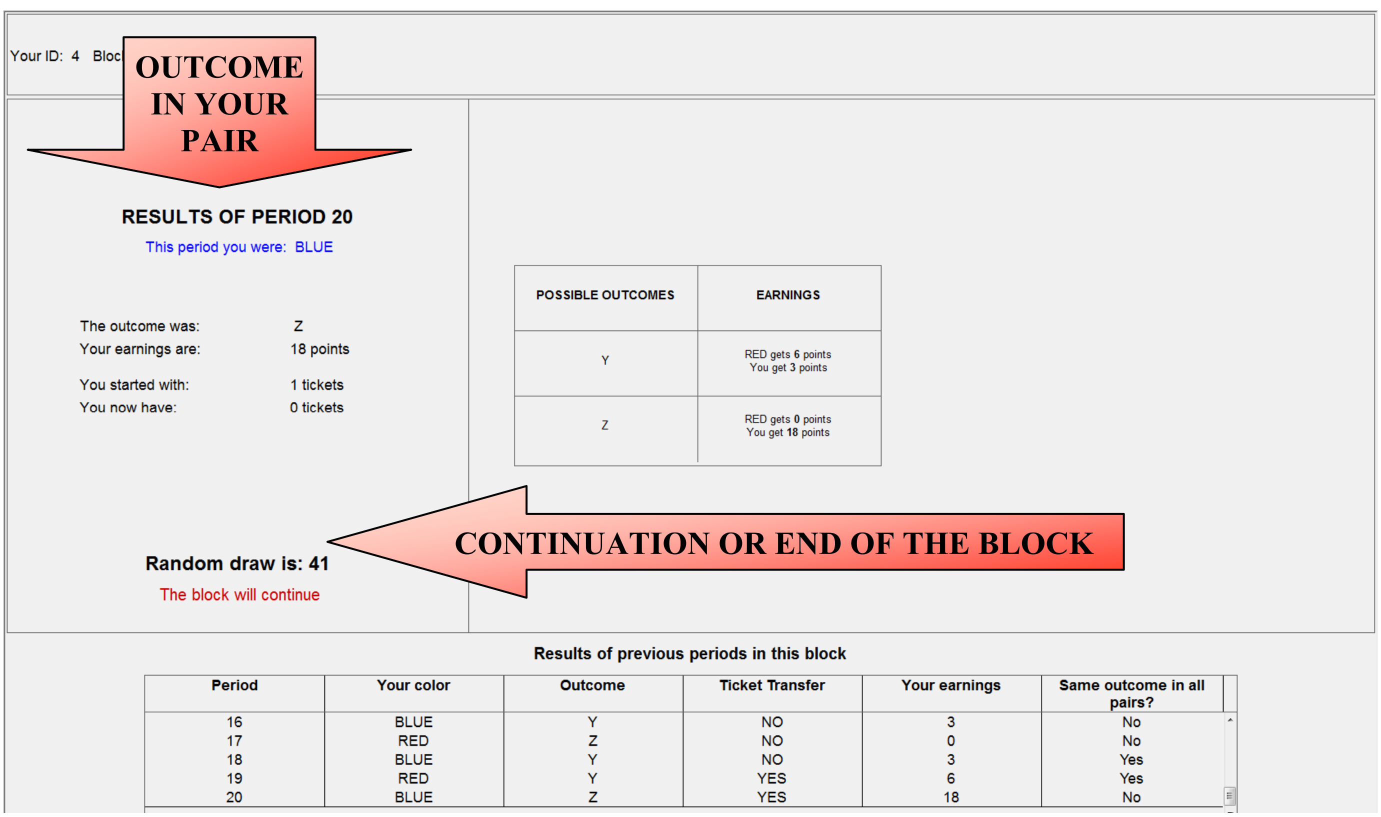
    Choosing this last option **guarantees** that:
    - If **blue** chooses any option involving "*Give a ticket,*" then the outcome is **Z** and you receive a ticket from **blue**.
    - Otherwise, the outcome is **Y** and you do not receive a ticket.

Note: If your **blue** match does not have a ticket, then you **do not** have the third option.



3. Outcome of choices

The results for the period will be displayed after everyone makes a choice (see figure below). You will see the **outcome**, if a ticket was transferred, and the points you **earned**. You can write the results on your record sheet. Results from past periods will again be visible at the bottom of the screen.



4. Ending of a block

Each block has many periods but their number is **unknown** because it is **random**. Hence:

- We never know for sure which period will be the last in a block.
- Some blocks may end up being longer and others shorter.

Each block will have at least 16 periods. From period 16 on, at the end of each period a computer selects with equal probability a number between 1 and 100. If the number selected is less than or equal to 75, then the block will continue. Otherwise, the block will end. This number is the same for every participant.

So: starting in period 16, the block has always a chance to continue. The results screen will inform you whether the block continues or not: you will see the randomly selected number.

Note: The number of past periods does not influence the chance that a block will end. In every period, **every number** between 1 and 100 has an equal chance of being selected. Hence, the chance that a block will end, say, after period 20, is 25%, which is identical to the chance that the block will end after period 16. As soon as a block ends, different groups are formed and a new block starts.

Will there be fixed pairs or random pairs?

In blocks 1 and 3 you will be in a fixed pair. In blocks 2 and 4 you will be randomly paired inside a group of 12 participants. **Recall**: participants that you meet in a block cannot be met in future blocks.

At the end of each of the first four blocks you will be asked to **express your preference** for your match to be either fixed, or randomly assigned from a group of 12 persons. Your preferences will **not** be revealed to others. A computer program will tally all the preferences expressed and in block 5 the program will either form fixed pairs or groups of 12 based on the most preferred option (or "flip a coin," in case of a tie).

Finally, before block 6 starts you will be asked to express a preference for your match to be either fixed, or randomly assigned from **one large group with all 24 participants** (in which case your match most likely changes every period). Once again, the computer program will implement the most preferred option.

Payments

When the session ends, **one** of the six blocks completed will be randomly selected. The points you have earned in that block will be converted into dollars: **1 point is worth 20 cents** ($0.20).

To choose the block we publicly roll a six-faced "virtual" die at http://www.bgfl.org/virtualdice.

The numbers on the die's faces identify the blocks. Each block is equally likely to be selected.

Final reminders

- The session is divided into six separate blocks; each block has many periods.
- In each period you meet an anonymous match. If pairs are fixed, your match is the same for the entire block. Otherwise, your match **changes** from period to period with more than 80% probability.

- If you are **red**, then you must choose between outcome Y and Z.
- The points you earn depend on the **outcome** in your pair, Y or Z, and whether pairs are fixed or not.
- Tickets **neither** yield points or dollars, **nor** will be redeemed for points or dollars.
- Each block has an **uncertain** number of periods. Starting in period 16, there is **always** a 75% chance of an additional period, and a 25% chance of ending.
- In the last two blocks pairs are fixed or random depending on the majority of preferences.
- You **cannot** interact with anyone for more than one block except, possibly, in the last block.

Before we start the experiment, you will be asked to answer ten questions designed to verify your understanding of the instructions. You will receive $0.25 for each question you answer correctly. If you have a question at any time, then please raise your hand and someone will come to answer it.

Alma Mater Studiorum - Università di Bologna
DEPARTMENT OF ECONOMICS

Strada Maggiore 45
40125 Bologna - Italy
Tel. +39 051 2092604
Fax +39 051 2092664
http://www.dse.unibo.it