Partners or Strangers? Cooperation, Monetary Trade, and the Choice of Scale of Interaction

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Abstract

We show that monetary exchange facilitates the transition from small to large-scale economic interactions. In an experiment, subjects chose to play an "intertemporal cooperation game" either in partnerships or in groups of strangers where payoffs could be higher. Theoretically, a norm of mutual support is sufficient to maximize efficiency through large-scale cooperation. Empirically, absent a monetary system, participants were reluctant to interact on a large scale; and when they did, efficiency plummeted compared to partnerships because cooperation collapsed. This failure was reversed only when a stable monetary system endogenously emerged: the institution of money mitigated strategic uncertainty problems.

Keywords: Coordination, endogenous institutions, repeated games.
JEL codes: C70, C90, D03, E02, E40

1 Introduction

Large-scale cooperation is central to economic development but challenging to achieve (North, 1991). The problem is that in large groups individuals are strangers, and this limits the ability to reward and punish, which raises vulnerability to exploitation and undermines trust (Milgrom et al., 1990). The fundamental question thus is: how can we expand the scale of interaction without undermining trust and cooperation? The literature has focused on studying the role of enforcement and punishment institutions (Bidner and Francois, 2011; Capra et al., 2009; Greif, 2006; Kimbrough et al., 2008). Here, we consider a primary financial institution: money. We have designed an experiment to uncover whether money can foster an expansion of the scale of interaction and of cooperation.

This question is especially relevant given the exponential rise in digital token alternatives to traditional currency instruments, such as Bitcoin and Ethereum, which have generated renewed interest in better understanding money and the economic problems it ultimately solves (Camera, 2017). Identifying a causal link between the development of monetary systems and economic expansion is one of the open issues because history only provides anecdotal evidence. 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).

In our experiment, players take part in a sequence of pairwise encounters where a good is produced at a cost below its consumption value—hence there are gains from trade. Players face an indefinite sequence of encounters, with roles alternating between producer and consumer (Townsend, 1980). Cooperation amounts to an intertemporal exchange of goods and is efficient, as it

maximizes long-term payoffs. This efficient outcome can be attained through a norm of "mutual support." We let players interact either as partners in fixed pairs, or strangers in large groups where counterparts change at random. This distinction is meaningful because large groups enjoy a return from cooperation that is 50% greater than in partnerships—an increase which proxies for gains from specialization and trade in wider markets. A drawback of large groups is that strangers cannot establish a reputation.

We contrast a CONTROL condition to a TOKENS condition. While in CONTROL consumers have nothing to offer—so producers can only provide goods on a voluntary basis—in TOKENS consumers are endowed with a symbolic object—a token that is intrinsically worthless but storable. Here, a monetary trade convention can spontaneously emerge if consumers can obtain a good only in exchange for a token. By design, nobody is forced to use tokens, so cooperation can still be sustained through a norm of mutual support. However, a monetary trade convention can also spontaneously emerge if there is a shared belief that production will occur only in exchange for a token, in which case tokens will be transferred back and forth among players (Camera and Casari, 2014).

What sets this study apart from other experiments on money is that the scale of interaction is endogenous: players choose between a partnership, or a large group of strangers. While, in principle, a norm of mutual support could promote the formation of large cooperative groups in either condition, the data suggest that the availability of a monetary system played a key role: in fact, forming a large group when a monetary system was unavailable led to efficiency losses. This suggests that a causal link exists between the development of a monetary system and the choice to form large groups. There is also a positive association between group expansion, strength of monetary system,

3

and economic gains.

At the heart of these results lies a tension between higher but riskier payoffs in large groups, and smaller but safer payoffs in partnerships. Though the use of tokens is not required for the creation of large cooperative groups, it facilitates the expansion of the scale of interaction because it mitigates strategic uncertainty problems and reduces the gains from free riding. Strategic uncertainty emerges because the game supports multiple Pareto-ranked equilibria, and this impairs coordination on efficient play (Blonski et al., 2011; Capra et al., 2009; Van Huyck et al., 1990). Adopting a monetary trade convention mitigates this problem because it limits the exposure to potential losses compared to a norm of mutual support. Moreover, such a norm requires a great deal of confidence that others will not succumb to opportunistic temptations as the game progresses—receiving help without giving any. This kind of confidence is not easily established in large groups, because interaction is impersonal and reciprocity impossible (Fehr and Gächter, 2000; Gächter and Hermann, 2011). Relying on monetary exchange helps building confidence because it imposes significant losses on those who adopt exploitative strategies.

The paper proceeds as follows. Section 2 provides some context by discussing the related experimental literature. Section 3 describes the design. Section 4 presents the theory. Section 5 reports the main results and Section 6 offers some final considerations.

2   Related experiments

This study is at the intersection of two strands of experimental literature: cooperation in large and small groups, and the study of money (Table 1). The typical finding when group size is exogenously manipulated, is that coopera-

tion falls as groups get larger (see papers in Table 1, top-left cell). By contrast, experiments that endogenously vary the group size report a positive effect on cooperation (Table 1, top-right cell). This may be driven by self-selection, as participants can form homogeneous groups of cooperators thanks to mechanisms such as "voting with your feet" or ostracism.[1] Our approach sidesteps this shortcoming by studying endogenous group formation without the possibility of self-selection. In our design, subjects choose the group size and then are randomly allocated to groups. This enables us to study how the institutional environment affects subjects' ability to support large-scale cooperation when interactions cannot be restricted to homogenous groups of cooperators.

Table 1: **A map of related experimental literature**

	Exogenous group size	**Endogenous group size**
No monetary institution	Camera et al. (2013a) Carpenter (2007) Diederich et al. (2016) Huck et al. (2004) Isaac and Walker (1988) Nosenzo et al. (2015), etc.	Ahn et al. (2009) Güth et al. (2007) Maier-Rigaud et. al. (2010) Nash et al. (2012) **This study** **(Control condition)**
With monetary institution	Camera et al. (2013a) Duffy and Puzzello (2014)	**This study** **(Tokens condition)**

Our paper also contributes to the growing experimental literature on money as a means of payment, which started with the early contributions of McCabe (1989), Lian and Plott (1998), and Marimon and Sunder (1993). Within this line of research, ours is the first study that addresses the fundamental question of endogenizing the group size. In previous experiments with money, either the

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[1]In these experiments, the choice of group size is intertwined with the choice of group composition, although these are separate issues: one could keep the group size constant, while endogenously altering group composition.

group size is fixed (Camera and Casari, 2014) or it is exogenously manipulated (Camera et al., 2013a; Duffy and Puzzello, 2014). Results form these earlier studies suggest that monetary systems are especially useful in large groups, although the evidence is not conclusive.[2] The original design that we adopt allows us to measure whether the institution of money promotes cooperation on a larger and more efficient scale, when self-selection is impossible.

This paper is part of a broader research agenda about the behavioral importance of monetary systems. In particular, it builds on three earlier works where the group size is exogenously imposed, and the returns from cooperation are independent of group size (Bigoni et al., 2015; Camera and Casari, 2014; Camera et al., 2013a). The present study contains two elements of novelty: the returns from cooperation increase in the scale of interaction, and the group size is determined by a collective choice. This allows us to explore the relation between the emergence of a monetary system, the expansion of markets and economic development, with a political economy angle.

Here, we lay out the distinct objectives and design of these three closely related works, and the additional insights of the present study. Camera and Casari (2014) proves that fiat money can endogenously emerge in the lab; it also shows that money has functions that go beyond pushing forward the efficiency frontier. This is done by adopting a design where—unlike the present study—monetary trade is theoretically inefficient. Results indicate that fiat monetary exchange emerges nonetheless, and it facilitates a coordination on cooperative play that is hardly attained without money.

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[2]Camera et al. (2013a) report that when monetary trade is unavailable cooperation rates decline with the size of the group, but remain constant if participants can engage in monetary trade. Duffy and Puzzello (2014) report that the presence of money enables coordination on a more efficient equilibrium, but a replication of this result has failed (see Camerer et al., 2016).

The article in Camera et al. (2013a) studies cooperation under *exogenous* variation of group size, from two to thirty-two players, with and without tokens. The paper finds that without tokens cooperation falls as groups get larger, while with tokens it remains stable. Unlike the present study, subjects experienced just one group size before being forced into a large group, so they could not assess how size affects cooperation. Another fundamental difference with the present study is that subjects neither had the option to expand the group size, nor the incentives to do so because the returns from cooperation could not increase as groups got larger. Here, instead, the transition from partnerships to large groups is endogenous and is also theoretically socially efficient. This allows us to study how the emergence of a monetary system affects the scale of interaction as well as realized efficiency. The theoretical advantage of a monetary strategy over a grim strategy is that it facilitates cooperation in large groups by reducing strategic uncertainty. The empirical results support this theoretical intuition.

Bigoni et al. (2015) investigates a mechanism that—according to current thinking in monetary theory (Kocherlakota, 1998; Ostroy, 1973)—could possibly explain these earlier results: do tokens act just as carriers of information about past conduct? The design thus introduces a treatment characterized by a *reputational mechanism* which, theoretically, should prove superior to a monetary system in supporting efficient play. In fact, the experiment does not provide support for this view because cooperation rates are substantially lower with a reputation mechanism than with tokens, suggesting that money is not just a carrier of information about past conduct.

3 Experimental design

The experiment has two conditions. We first fully explain the CONTROL condition, and then discuss the changes introduced in the TOKENS condition.

Control condition. In the CONTROL condition, participants play a "helping game" in pairs composed of a producer and a consumer. Each producer starts with 6 consumption units (CUs) and can choose to help ("give help") or not ("no help"). The consumer has 3 CUs. Helping yields a payoff of 0 CUs to the producer and a payoff of $k > 9$ CUs to the consumer; the net benefit from help is $k - 9$ CUs. The value of the parameter k depends on the size of the economy, as explained below.

Participants play this game repeatedly, in "cycles" of uncertain duration. A cycle consists of at least sixteen rounds, after which we implement a continuation probability of 75%.[3] Hence, cycle duration is the same for everyone in the same cycle of a session, although it can vary across cycles and sessions. 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. 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

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[3]A cycle lasted an average of 19 rounds, the longest session lasted 127 rounds. The length of the cycle was not pre-selected in advance. We introduce a discontinuity in continuation probability in round 16 for two reasons. First, it provides each subject with a minimum degree of experience, and minimizes the confounding effect of heterogeneous duration across cycles and treatments, while keeping cycles' duration ex-ante unknown to participants and experimenters alike. Second, it allows us to implement a perfect stranger matching across cycles, which is key to avoid reputation spillover effects and to maintain a close connection with the theory. Experimental results appear robust to changing the number of initial fixed rounds (Camera et al., 2013b).

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 anonymous public monitoring: at the end of each round, participants can see whether or not the outcome was identical in every pair of their group ("yes" or "no"). Participants can view a record of this feedback—as well as own payoffs, roles, actions and outcomes—for all past rounds of the cycle. Public monitoring makes small and large groups more comparable because it ensures that the crucial parameter that theoretically supports full cooperation is independent of group size (see Section 4). To minimize reputational spillovers, no information is made available about outcomes outside the participant's group. We also adopt a perfect stranger matching procedure across cycles, ensuring that no one interacts with someone met in previous cycles (except possibly in cycle 6).[4]

Benefits from cooperation are greater in large groups ($k = 18$) than in partnerships ($k = 15$); see Table 2.

Table 2: **Payoffs in partnerships and large groups**

	Producer				**Producer**	
	No help	*Give help*			*No help*	*Give help*
Consumer	$3, 6$	$15, 0$	**Consumer**		$3, 6$	$18, 0$
	(a) Partnerships				(b) Large groups	

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. Hence, by design, the return

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[4]This is feasible because of deterministic alternation of roles. For details about matching across cycles see the online Appendix.

from cooperation is 50% greater in large groups compared to partnerships: full cooperation creates a per-capita *surplus* of 3 CUs in partnerships and 4.5 CUs in large groups, relative to 4.5 CUs when no one cooperates.[5]

However, expanding the scale of interaction is not necessarily beneficial, because surplus creation depends on the cooperation rate achieved in the group. We assess a group's success in creating surplus by measuring economic *efficiency*, which is 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. It is invariably zero when no one cooperates, 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.

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. To control for order effects, group size in the Training Phase followed either the order 2-12-2-12 or 12-2-12-2 (4 sessions per order, per treatment). Instead, the scale of interaction in the Selection Phase is endogenous. Before the start of cycles 2-5, participants express a preference for partnerships or groups of 12; before cycle 6, they choose partnerships or a group of 24. The majority of choices determines the group size for everyone in the session: the choices made before cycles 2-5 were all counted to select the group size for cycle 5, while the group size for cycle 6 was determined only by the choices made before that cycle.[6]

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[5]While the assumption that large markets have higher returns than small markets is uncontroversial, the specific wealth multipliers of 1.67 and 2 employed in the experiment are discretionary, although well within the range in the experimental literature. Public good experiments typically use multipliers between 1.2 and 2.5, trust games generally ranging between 3 and 6. As in any experiment, the quantitative results are of course tied to the exact parameter values.

[6]The design exhibits an asymmetry in the number of choices expressed for groups of size 2 vs. 12 as compared to 2 vs. 24. Alternative designs could eliminate this asymmetry but

Tokens condition. In this condition we add symbolic, 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 3).

Table 3: **The stage game in the Tokens condition**

Producer

		No help	*Give help*	*Sell help*
	Do nothing	$3, 6$	$k, 0$	$3, 6$
Consumer	*Transfer a token*	$\circledast\!\!\rightarrow$ $3, 6$	$\circledast\!\!\rightarrow$ $k, 0$	$\circledast\!\!\rightarrow$ $k, 0$
	Buy help	$3, 6$	$\circledast\!\!\rightarrow$ $k, 0$	$\circledast\!\!\rightarrow$ $k, 0$

Notes: In the experiment $k = 15$ in partnerships and $k = 18$ in large groups, and actions had neutral labels. "$\circledast \rightarrow$" indicates the transfer of a token from consumer to producer.

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.

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present other drawbacks. For instance, subjects could have made just one choice before cycle 5 (12 vs. 2) and one before cycle 6 (24 vs. 2), and no choices in previous cycles. Relative to our design, which induces subjects to think more thoroughly about the choice over group size, this alternative reduces the focus on the importance of the choice of group size. Another alternative is to elicit two choices before each cycle 2-4 (one for 12 vs. 2, the other for 24 vs. 2), and then to elicit a single choice before cycle 5 and before cycle 6. We believe this alternative lowers subjects' understanding of the task, relative to our design, and is more likely to generate noisy choices.

Choices are made simultaneously and without communication. Actions had neutral labels: terms like "buy" and "sell" were never used in the instructions (for details see online Appendix).

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 – whether help was given, whether a token was transferred – but not the action of the counterpart. Consider that there are multiple combinations of actions that lead to help jointly with the transfer of a token (Table 3).

If a consumer has no tokens, he has no actions to take, and the producer can only choose whether to help unconditionally or not: hence the decision situation is identical to the CONTROL condition. Token holdings are partially observable by the counterpart: in every pair, each player can see if the counterpart has either 0 or at least one token; the exact number is unobservable in order to preserve anonymity and to reduce the cognitive load.

Experimental procedures. The experiment involved 384 undergraduate volunteers, each of whom participated in only one session between 9/2014 and 10/2014. 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 about 2 hours (including instructions, quiz and payments) 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.

The experiment was programmed using the software z-Tree (Fischbacher, 2007) and ran in the Economic Science Institute's laboratory at Chapman

12

University. No eye contact was possible. We collected participants' demographic data 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 online Appendix). 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.

Design choices and possible alternatives. Here we provide a few additional considerations about the specific design adopted in this experiment, based on results from complementary studies within this line of research.

A first consideration is about the choice of information structure. One may argue that the TOKENS condition adds information about individual past conduct that is unavailable in CONTROL; treatment effects may thus be driven by the richer information structure and not by the possibility of monetary exchange. This important issue is the focus of a companion study (Bigoni et al., 2015). There, a third experimental condition introduces a public record of past individual actions which, theoretically, should supersede the function performed by tokens. The data reveal that cooperation rates in this condition are substantially lower than in TOKENS, providing evidence that monetary systems perform a richer set of functions than just revealing past behaviors. A similar result also emerges in Camera and Casari (forth.), which shows that information about past conduct alone is ineffective in overcoming cooperation challenges in indefinitely repeated games among strangers.

A second consideration concerns the action space in TOKENS. One may be concerned that the three alternatives available to the subjects in this design may bias the subjects' behavior in favor of the emergence of monetary

13

exchange. Bigoni et al. (2015) addresses this possible concern, with a design including additional actions that are *antithetical* to monetary exchange. The consumer can give a token only if the producer *does not* help, while the producer can commit to help only if he *does not* receive a token. Hence, tokens may take on a negative connotation as subjects could use them to tag defectors. Even under this expanded action set, we observe that subjects learn to use tokens as a medium of exchange, neglecting these additional actions.

A third consideration relates to subjects' experience with monetary systems in their daily lives. One may surmise that subjects accustomed to deal with money outside the lab automatically coordinate on using tokens as media of exchange in the experiment. Evidence from earlier studies on the endogenous emergence of monetary systems does not support this view. In fact, the experimental data reveal that subjects need to have repeated exposure to the Tokens condition in order to discover how tokens can function as money, so that it takes time for a widespread monetary convention to emerge (Bigoni et al., 2015; Camera and Casari, 2014; Camera et al., 2013a). The four cycles of Training Phase in this design are meant to facilitate this process.

4   Theoretical considerations

Why should players form large groups? By design, cooperating in large groups is more rewarding than in partnerships, so full cooperation in large groups is Pareto-efficient. In this section we demonstrate that, according to standard theory, full cooperation is an equilibrium in the Control condition both in partnerships and in large groups (Section 4.1). This suggest there is no reason to expect higher cooperation rates in partnerships than in large groups. In fact, large groups theoretically support full cooperation for *lower* discount fac-

14

tors than partnerships due to the higher returns from cooperation and public monitoring. Evidence from previous experiments on repeated games among partners suggests that lower threshold discount factors facilitate cooperation (Dal Bó and Fréchette, 2011). Therefore, if Pareto dominance is a relevant equilibrium selection criterion, subjects should choose large groups over partnerships. These considerations suggest a first testable hypothesis:

Hypothesis 1. *Players in the* CONTROL *condition will select large groups over partnerships.*

We proceed by showing that a fully cooperative equilibrium exists also in the TOKENS condition (Section 4.2). This equilibrium can be equivalently sustained with and without using tokens as money. In particular, using tokens as money does not alter the return from cooperation relative to the CONTROL condition, neither in partnerships nor in large groups. These additional considerations suggest a second testable hypothesis:

Hypothesis 2. *The availability of tokens will not alter the selection of the scale of interaction.*

Finally, since each condition supports multiple equilibria, we go beyond the canonical theoretical analysis by studying the impact of strategic uncertainty (Section 4.3). We demonstrate that in the CONTROL condition strategic uncertainty may prevent coordination on the efficient equilibrium, but that the use of tokens as money can resolve this problem. Based on this refinement to standard theory, we surmise that if strategic uncertainty motivates choices, then the use of money might tilt the selection of interaction scale toward large groups, in contrast to the hypothesis stated above.

4.1 Control condition

Here we show that in our experiment full cooperation can be supported as a sequential Nash equilibrium in groups of any size. To do so, we consider a "grim" trigger strategy specifying actions—for a player who is a producer—based on two "states": (i) Cooperation: the player selects "give help"; (ii) Punishment: the player selects "no help." The strategy specifies that the player starts in the cooperation state and permanently transitions to the punishment state if anyone in the group defects (including the player himself). 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 thanks to the availability of anonymous public monitoring (Kandori, 1992, Proposition 1). The strategy is constructed so that after any history of play, conduct in the continuation game is part of an equilibrium of the original game (Abreu et al., 1990). The central feature of this norm is that the entire group participates in punishing defections so in equilibrium no one defects. We have the following:

Proposition 1. *If $\beta \geq \beta^* := \dfrac{6}{k-3}$, then full cooperation is part of a sequential Nash equilibrium, where $\beta^* = 0.4$ in large groups and $\beta^* = 0.5$ in partnerships.*

Proof. See the Appendix A. □

If participants are risk-neutral, then the fully cooperative equilibrium exists in the CONTROL condition, in groups of any size, because in the experiment $\beta = 0.75$. The threshold β^* depends only on the differences in returns from cooperation and not on the group size because of public monitoring. Moreover, the derivation of this threshold fully takes into account the discontinuity in continuation probability that characterizes our design.

16

The result in Proposition 1 is based on the assumption that all players coordinate on a punishment strategy that is immediate, indiscriminate and unforgiving. However, previous experimental results show that trigger strategies of this kind are uncommon among partners and even more so among strangers (Camera et al., 2012; Dal Bó and Fréchette, 2011; Fudenberg et al., 2012). Furthermore, the condition $\beta \geq \beta^*$ does not guarantee that full cooperation will be realized because many equilibria exist in the game. For these reasons, below we will refine this standard theoretical prediction by incorporating the concept of risk-dominance into the analysis (Section 4.3).

4.2   Tokens condition

All the equilibria that exist in the CONTROL condition also exist 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 1 (**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 1. Here, help is only given quid-pro-quo in exchange for one token.[7] Otherwise, help is not given. In monetary equilibrium all encounters support trade due to the deterministic alternation between roles,

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[7]Transferring more than one token is unnecessary to attain full cooperation, and is also impossible in monetary equilibrium because each consumer has just one token. These considerations, and a desire to minimize the cognitive load for participants, explain why in our design consumers could transfer only one token per round.

so equilibrium payoffs are identical to those attained under the social norm. It follows that if the social norm of cooperation is an equilibrium, then monetary trade is also an equilibrium.

Proposition 2. *If* $\beta \geq \beta^* = \dfrac{6}{k-3}$, *then the monetary trade strategy in Definition 1 supports full cooperation in equilibrium.*

Proof. See Appendix A. $\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad$ □

To sum up, adding tokens neither precludes the adoption of the social norm of cooperation, nor forces the use of tokens. If the discount factor β supports the fully cooperative equilibrium without using tokens, then this is also sufficient to support full cooperation by exchanging tokens. Adding tokens simply expands the strategy set, but it neither eliminates equilibria, nor expands the set of payoffs compared to the CONTROL condition.

4.3 Strategic uncertainty: the role of tokens

Previous experimental results suggest that tokens positively influence outcomes: Camera and Casari (2014) report that tokens facilitated coordination on cooperative play in stable groups of four players; Camera et al. (2013a) report that when the group size was *exogenously* increased, cooperation rates declined without tokens, but this no longer occurred when subjects could exchange tokens. What is the theoretical mechanism behind these results? In this section we show that the use of tokens reduces the strategic uncertainty that exists in large groups. As a consequence, the use of tokens may promote the choice of large groups over partnerships.

To study how strategic uncertainty affects the ability to support the efficient outcome, we take two steps. First, we demonstrate that in the CONTROL

18

condition strategic uncertainty may prevent coordination on the efficient equilibrium in large groups (but not in partnerships). Then, we show that the use of tokens as money can resolve this problem. The theoretical argument is built along the lines of the study in Blonski et al. (2011), which adapts the static concept of risk-dominance to an infinitely repeated prisoner's dilemma in fixed pairs. We study risk dominance for the grim strategy and for the monetary trade strategy, by considering each strategy in isolation against the alternative strategy "always defect." Our focus on comparing two equilibria, instead of three or more, reflects the standard approach in the literature. In doing so, we assume that a player who is unsure about the strategy choice of others adopts the "principle of insufficient reason," placing equal weight on each strategy choice.

The main result can be summarized as follows.

Proposition 3. *The monetary strategy is risk dominant in large groups, while the grim strategy is not.*

Proof. See Appendix A $\qquad\qquad\qquad\qquad\qquad\qquad\qquad\square$

Start by noting that, without tokens, strategic uncertainty is not a problem in partnerships because there is just one player (the producer) who takes an action in each round. In round 1, the action of the producer fully reveals her strategy, "grim" or "always defect." Therefore, the counterpart faces no strategic uncertainty when she becomes a producer in round 2. The initial producer can thus select the efficient equilibrium by cooperating. This is the central difference between our helping game in fixed pairs and the prisoner's dilemma studied in Blonski et al. (2011).

Now consider large groups without tokens. Here there is strategic uncertainty because many producers simultaneously choose between "grim" and

19

"always defect" in round 1. Grim is risk dominant if initial producers are at least indifferent to choosing the competing strategy. The payoff expected from choosing grim depends only on the likelihood that full cooperation is the outcome in round 1, by the end of which all strategic uncertainty is resolved: given public monitoring, if there is full cooperation, then this will also be the outcome in every future round, otherwise there will be full defection forever after. Consider an initial producer who chooses the grim strategy. Suppose she believes that every other initial producer selects grim with probability $0 < p < 1$. If there are $n - 1$ other producers, then the probability of full cooperation in round 1 is p^{n-1}, which is decreasing in n. It follows that, for any p, risk-dominance as a strategy selection criterion requires a greater threshold discount factor β^{**} to support cooperation, as compared to the threshold β^* implied by standard theoretical arguments. In particular, if we assume that $p = 0.5$ as per the principle of insufficient reason, then $\beta^{**} \simeq 0.98$ for groups of 12, and $\beta^{**} \simeq 0.99$ for groups of 24. As a consequence, the grim strategy is not risk dominant in large groups in our design. The message is that strategic uncertainty is likely to impair coordination on the efficient equilibrium in the CONTROL condition.

Monetary trade can resolve this problem because it *is* risk dominant in large groups. In the TOKENS condition, let the choice be between "monetary trade" and "always defect" in round 1. Initial consumers have always an incentive to select monetary trade since tokens do not bestow benefits *per se*. So consider initial producers. As before, suppose an initial producer believes that every other initial producer selects monetary trade with probability $0 < p < 1$. If $p = 0.5$, then $\beta^{**} \simeq 0.63$ for groups of 12, and $\beta^{**} \simeq 0.64$ for groups of 24. The reason why the threshold discount factor β^{**} needed to support cooperation is not as high as without tokens is that miscoordination on monetary trade does

20

not trigger the permanent and indiscriminate form of punishment associated with grim. Hence, though strategy miscoordination does reduce payoffs of monetary traders, adopting the monetary trade strategy can be profitable even if not everyone else does the same.

These considerations suggest that the addition of tokens can be very helpful to widen the scale of cooperation, improving payoffs. This, of course, may occur only if tokens are used as money in the experiment. In that case, the emergence of a monetary system might induce subjects to choose large groups over partnerships in the TOKENS condition. This contrasts with Hypothesis 2, which is based on standard theoretical arguments that do not account for the role of strategic uncertainty.

5   Results

We report four main results, which are based on subjects' behavior in the Selection Phase (cycles 5 and 6). Before presenting them, we provide an overview of behavior in the Training Phase. To balance the number of observations across cycles and session, the analysis focuses on rounds 1-16 of a cycle. The four results reported are robust to considering all rounds.

5.1   Training Phase

Average cooperation rates were higher in partnerships than in large groups (69.4% vs. 50.0%, p-value = 0.016 in CONTROL; 67.6% vs. 48.8%, p-value = 0.023 in TOKENS; see also the regression in Table 4, Model 1).[8] However, in the Training Phase, partnerships did not create more surplus than large

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[8]p-values presented in this paragraph are based on two-sided Wilcoxon matched-pairs signed-rank tests with exact statistics, taking two (matched) observations per session: $N_1 = N_2 = 8$.

groups because, by design, they had lower returns from cooperation (efficiency was 46.2% vs. 50.0% in CONTROL, and 46.1% vs. 48.8% in TOKENS; p-value > 0.1 under both conditions, see also Table 4). Given this evidence, there is no clear social benefit from enlarging the scale of interaction, and hence no reason to expect that a majority of participants would express a preference for large groups in either condition.

Table 4: **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 is the per-round average cooperation or efficiency in each cycle of a session. Training Phase only (cycles 1-4). The default condition is CONTROL and partnerships. Linear regressions 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). The difference between coefficients for *Tokens × large* and *Control × large* is statistically insignificant in Model 1 (two-sided Wald test, p-value=0.770), and in Model 2 (two-sided Wald test, p-value =0.739). The difference between coefficients for *Cycle 2* and *Cycle 4* is statistically significant in Models 1 and 2 (two-sided Wald test, p-values=0.020 and 0.037, respectively). Symbols ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5% and 10% level, respectively.

A second important consideration is that a monetary trade convention emerged in the experiment, but its development required some time and experience. In the Training Phase, holding group size constant, aggregate coopera-

tion rates and efficiency were similar in CONTROL and TOKENS; this evidence is provided by the first three coefficients in the regressions in Table 4.[9] However, there were important differences in individual actions across Conditions. In TOKENS, whenever monetary trade was feasible (i.e. the consumer had at least one token), consumers overwhelmingly chose "buy help" (81.8%) and producers mostly chose "sell help" (63.4%). Instead, help was rarely given to consumers without tokens (18.3%); this contrasts with behavior observed under the same decisional situation in CONTROL, where "give help" was the predominant choice (59.7%). Simply put, in TOKENS producers were reluctant to help without being concurrently compensated with a token. These results are in line with previous experiments (Bigoni et al., 2015; Camera et al., 2013a), thus providing a reassuring replication of earlier results obtained under different experimental protocols, payoffs, and continuation probability (Camerer et al., 2016).

In what follows, we report how these differences in Training Phase behavior across conditions influenced participants' desire to widen the scale of interaction in the Selection Phase.

5.2 The choice of scale of interaction

The experimental evidence does not support either of the theoretical hypotheses about the endogenous scale of interaction, while it is in line with the competing, behavioral hypotheses.

Result 1. *Without tokens, participants infrequently form large groups.*

Result 2. *The availability of tokens promotes the formation of large groups.*

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[9]In addition, for each group size we obtain a p-value > 0.1 for both cooperation rate and efficiency, based on two-sided Wilcoxon-Mann Whitney ranksum tests with exact statistics, taking one observation per session with $N_1 = N_2 = 8$.

Participants in TOKENS selected to interact in large groups more frequently than in CONTROL (Table 5).

Table 5: **Share of preferences for large groups.**

	CONTROL	TOKENS
Overall (cycles 2-6)	0.421	0.546
Selection Phase only		
—*Cycle 5 (groups of 12)*	0.432	0.573
Large groups formed in	2 of 8 sessions	6 of 8 sessions
—*Cycle 6 (groups of 24)*	0.354	0.542
Large groups formed in	1 of 8 sessions	4 of 8 sessions

By the end of the Training Phase, all subjects have experienced two cycles of interactions in two different partnerships and in two different groups of 12. Therefore, to analyze preferences for large and small groups, we focus on the choices expressed in the Selection Phase, comprising cycles 5 and 6. Overall, the share of preferences for large groups is 55.8% in TOKENS and 39.3% in CONTROL; the difference is statistically significant according to a two-sided Wilcoxon-Mann Whitney test (p-value = 0.030, $N_1 = N_2 = 8$) and to the regression in Table 6 (p-value 0.014 on "Tokens condition" coefficient).

Table 6: **How money affects preferences for large groups.**

Dependent variable: preference for large groups (yes=1)	marg. eff.	S.E
TOKENS condition (dummy)	0.177**	(0.072)
Cycle 6 (dummy)	-0.055	(0.034)
Controls	Yes	
N	768	

Notes: One observation per person per cycle. Data for Selection Phase only (cycles 5 and 6). Panel probit regression on the preferences for large groups, with standard errors robust for clustering at the session level. 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). Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

The result is robust to separately considering cycle 5 and cycle 6 according to two regressions based on the same specification as the regression in Table 6 (the coefficients on the Tokens dummy are significant at the 5 percent level, in both cycle 5 and 6). However, according to two-sided Wilcoxon-Mann Whitney tests run separately for cycle 5 and 6, the treatment effect is significant only for cycle 6 (p-values = 0.109 and 0.065, respectively, for cycle 5 and cycle 6).

Next we analyze the choice of group size and investigate the determinants of these choices. Is it monetary exchange that induced a preference for large groups, or the experience of higher cooperation levels? We can exclude differences in cooperation rates as the main explanation: as noted above, in the Training phase cooperation levels were not statistically different between TOKENS and CONTROL. Therefore it must be the exposure to monetary exchange itself that induced different choices over group size. In what follows we investigate how.

Two elements of the experience during the Training Phase determined an

individual's disposition to widen the scale of interaction: experiences of full cooperation (the subject always receives help as a consumer, and always gives help as a producer) and exploitation by free-riders (the subject gives more help than he receives). Below we quantify these two elements, and we explain how they affect the individual's choice of group size in the Selection Phase.

We measure exploitation in the Training Phase by the endogenous variable *help imbalance*, calculated as the difference between how frequently a participant received and gave help in a cycle, normalized for the number of rounds. Figure 1 shows that help imbalance goes from -1 to 1: it is negative for someone who gave help more frequently than she received it, positive otherwise. In particular, help imbalance takes value -1 for an unconditional cooperator who always gave help as producer, but never received help as consumer; this corresponds to an average payoff of 1.5 CUs per round. Conversely, a free-rider who never helped as producer, but always received help as consumer, has an imbalance of 1; this corresponds to an average payoff of $3+k/2$ CUs per round. The help imbalance is 0 for someone who gave and received help in equal amounts, over the course of a cycle; this occurs when the participant experienced full cooperation (denoted by the dark bars in Figure 1), partial but proportionate cooperation (e.g., the participant helped three out of eight times as a producer, and received help three out of eight times as a consumer), or no cooperation at all. As a result, the average payoff associated with 0 help imbalance ranges between $1.5+k/2$ (full cooperation) and 4.5 (no cooperation) CUs per round.

Participants are unsure which strategy others will use. This *strategic uncertainty* (Heinemann et al., 2009; Van Huyck et al., 1990) 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 imbal-
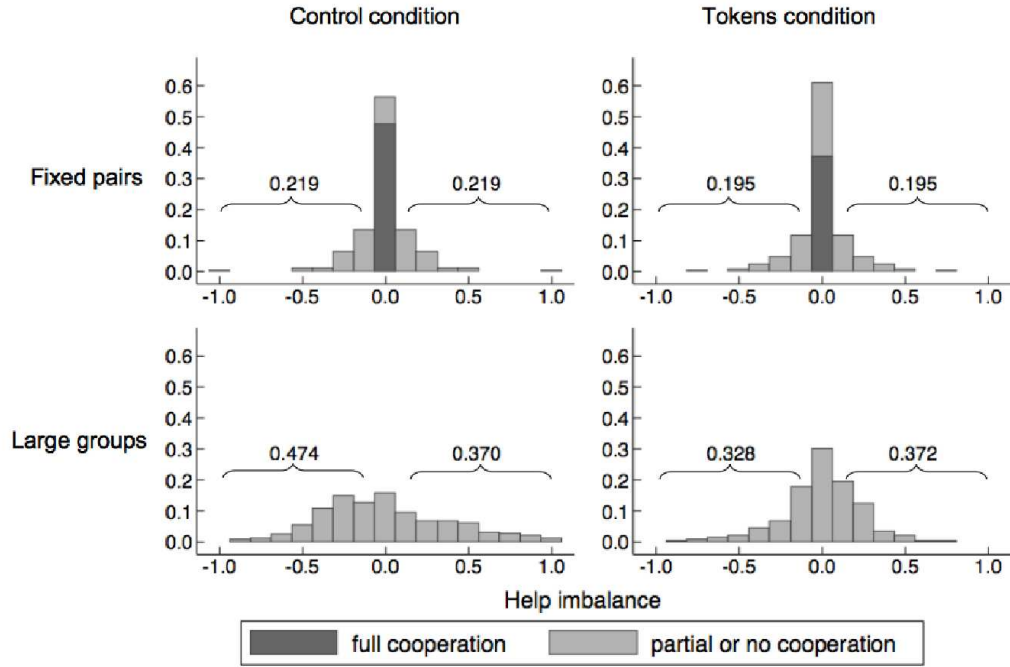
26

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, normalized for the number of rounds. Unconditional co-operators who always gave help as producers, and never received help as consumers, have an imbalance of -1; conversely, free-riders who never helped as producers, and always received help as consumers, have an imbalance of 1. An imbalance of 0 indicates the a participant gave and received help in equal amounts. Data from rounds 1-16, Training Phase only; four observations per participant.

ance 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: in CONTROL we have 0.563 vs. 0.156, respectively; in TOKENS we have 0.609 vs. 0.299 (p-value = 0.008 in each treatment—two-sided Wilcoxon matched-pairs signed-rank tests with exact statistics, two matched observations per session: $N_1=N_2=8$); additional evidence is provided

by the "Large groups" coefficient in Table B1, in the online Appendix.

A widespread adoption of monetary exchange offers protection against exploitation hazards because a participant must transfer a token to receive help, and the only way to obtain tokens is to help others. There is evidence that the possibility to trade tokens for help *quid-pro-quo* reduced this exploitation hazard in the experiment. We more frequently observe zero help imbalance in TOKENS than in CONTROL, especially in large groups where it was almost twice as frequent (0.299 vs. 0.156, p-value = 0.0026—two-sided Wilcoxon-Mann Whitney ranksum test with exact statistics, one observation per session: $N_1 = N_2 = 8$); Table B2 in the online Appendix provides further evidence.

Were the more cooperative type of participants more likely to choose a large group? The probit regression in Table 7 estimates how the desire to widen the scale of interaction is affected by various factors in the Selection Phase, when participants had already experienced small and large groups. 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.

Table 7: **Money 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)	0.115	(0.075)
TOKENS condition x cycle 6 (dummy)	0.156*	(0.080)
Cycle 6 (dummy)	-0.087*	(0.052)
Training phase		
Help imbalance - partnerships	0.135	(0.146)
Help imbalance - large groups	0.312***	(0.072)
Full cooperation - partnerships (dummy)	-0.183***	(0.062)
Controls	Yes	
N	768	

Notes: One observation per person per cycle. Panel probit regressions on preferences for large groups of 12 and 24 expressed in the Selection Phase (cycles 5 and 6, respectively), with standard errors robust for clustering at the session level. 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). Data from rounds 1-16 only. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

This regression reveals that free riders, i.e. those who received more help than they gave, were more willing to interact in large groups. Instead, those exploited by free riders were more likely to opt for the safety experienced in partnerships. This may seem surprising but consider, first, that participants could not self-select into homogenous groups of cooperators, and, second, that in large groups free riders could not be directly targeted for punishment.

Support for these findings comes from the estimated coefficients on *help imbalance* experienced during the Training Phase in partnerships and groups of strangers, and *full cooperation* in partnerships. The regression reveals that help imbalance in large groups is crucial. 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 different experience of exploitation weakened the desire to expand the scale of interaction in CONTROL.

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 7 is negative and highly significant). Partners attained full cooperation more frequently in CONTROL than in TOKENS (the difference, however, is not significant according to a two-sided Wilcoxon-Mann Whitney test, and marginally significant according to the regression in Table B3 in the online Appendix), which suggests that the possibility of relying on monetary trade displaced norms of voluntary help (Camera et al., 2013a).[10] 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 in Table 7 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

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[10]In TOKENS there is more than one way to support full cooperation—monetary exchange or the trigger strategy—which may give rise to coordination issues. Among partners, a single round of miscoordination would prevent monetary exchange, if it leaves the consumer without tokens. This can explain why full cooperation in CONTROL was initially more frequent than in TOKENS. Subjects did converge on a norm of monetary exchange in TOKENS, but only with experience: in cycle 4 the frequency of full cooperation in partnerships was similar in CONTROL and TOKENS, 62.5% v.s. 60.4%.

were more willing to select large groups.

5.3 Efficiency

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 3. *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 8: **How monetary trade and group size influence efficiency.**

Dependent variable: efficiency	coefficient	S.E
CONTROL condition × Large	-0.121**	(0.056)
TOKENS condition × Partnership	-0.021	(0.030)
TOKENS condition × Large	0.101	(0.064)
Cycle 6 (dummy)	0.014	(0.021)
Constant	0.566***	(0.024)
N	32	
R-squared	0.343	

Notes: One observation is the average efficiency in a cycle of a session, Selection Phase only (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. Standard errors are robust for clustering at the session level. Data from rounds 1-16. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

The linear regression in Table 8 measures how efficiency varies with group size and availability of tokens. The dependent variable is 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=0.059). The opposite is true in CONTROL (45.0% vs. 57.3%, p-value=0.049). Large groups also attained greater efficiency in TOKENS than CONTROL (two-sided Wald test on the estimated coefficients, p-value=0.016). In partnerships, instead, efficiency levels were similar across conditions.

Result 4. *Strong monetary systems raised efficiency in large groups compared to partnerships. Weak monetary systems reduced it.*

As efficiency is proportional to cooperation rates, it is interesting to see how cooperation rates differed across conditions, in the Selection Phase. When we pool together data for cycles 5 and 6, we find that average cooperation rates in large groups were 67.6% in TOKENS vs. 47.2% in CONTROL. As a comparison, cooperation rates in partnerships were quite similar, with 83.1% in TOKENS vs. 86.0% in CONTROL.

To assess the significance of these differences consider the regression in Table 9, based on the specification in Table 8. Partnership's cooperation rates are statistically similar across conditions. In each condition, cooperation rates fall as we move from Partnerships to Large groups. However, the decline is significantly larger in CONTROL as compared to TOKENS; the *Control condition × Large* and *Tokens condition × Large* coefficients are both negative, but are significantly different at the 5 percent level (Wald test, p-value=0.019).

Table 9: **Cooperation in the Selection Phase.**

Dep. var. = Cooperation	coefficient	S.E
Control condition $\times$ Large	-0.404***	(0.063)
Tokens condition $\times$ Partnership	-0.033	(0.045)
Tokens condition $\times$ Large	-0.183**	(0.068)
Cycle 6	0.032	(0.022)
Constant	0.842***	(0.034)
N	32	
R-squared	0.616	

Notes: One observation is the average cooperation in each cycle of a session. Selection Phase only (cycles 5 and 6). The default condition is CONTROL, partnerships. Linear regression on average cooperation on a set of dummy variables that include the interaction between condition and group size. Standard errors are robust for clustering at the session level. Data from rounds 1-16. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

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 happened only in one of the five large groups that were formed (a group of size $N = 12$).
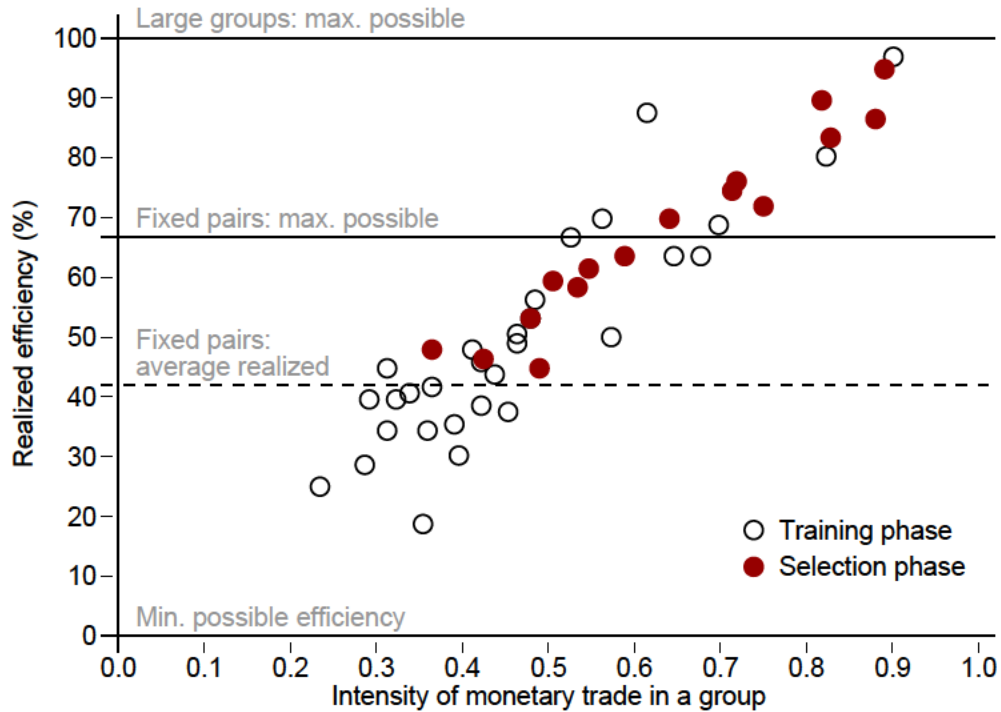
Figure 2: **A strong monetary system raised efficiency in large groups.**

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.

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.

Table 10: **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	3.419***	(0.203)
at the individual level	0.919***	(0.215)
Cycle 6 (dummy)	-0.079**	(0.033)
Controls	Yes	
Constant	3.819***	(0.499)
N	240	
R-squared (within)	0.095	
R-squared (between)	0.403	
R-squared (overall)	0.413	

Notes: One observation per person per cycle. Selection Phase only (cycles 5 and 6). Out of 16 possible opportunities to form large groups, 10 were realized (see Table 5 in Supplementary Material). Panel 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. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

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 10). 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.[11]

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[11]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 feasible.

6 Conclusions

We have shown that well-functioning monetary institutions can cause a group of people to transition from engaging in low-value personal exchanges in partnerships, to pursue high-return impersonal exchange in large groups. We also investigated, theoretically and empirically, the mechanism that enables this transition.

In an experiment where participants could rely on the institution of money, large groups spontaneously emerged, cooperated more, and created more surplus than partnerships. In contrast, large groups rarely emerged without a monetary institution and, when they did, free-riding prevailed because defectors could not be identified. In each treatment, the decision to form large groups involved every session participant, and it did not hinge on self-selection effects because defectors could not be excluded from the group. This setup differs from the typical experiments about endogenous group formation, where inclusion or exclusion rules for single individuals make self-selection possible.

So, why did a monetary institution promote large-scale cooperation? Simply put, it offered protection from strategic uncertainty. Strategic uncertainty becomes a central stumbling block to widening the scale of cooperation when self-selection mechanisms are unavailable. Consider that our experimental setup exhibits equilibrium multiplicity ranging from zero to full cooperation. Partners can easily coordinate on a high-payoff strategy by relying on reciprocity and reputation. Instead, in large groups opportunistic temptations are stronger because free-riders cannot be directly targeted for punishment. This contributes to raising strategic uncertainty as participants are unsure about what others will choose. Selecting a 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.

Were cooperative types of participants more likely to choose large groups? The answer is no: preferences for large groups were especially strong among free riders, and were especially weak among cooperators who were their victims. This finding is perhaps surprising vis-a-vis the extant literature, where the driving force behind endogenous group formation is self-selection. For example, if subjects can "vote with their feet," then they can congregate into homogenous cooperative groups. Under our design with random allocation of participants to large groups, the mechanism at work is completely different. In this manner we uniquely contribute to the literature about endogenous group formation by studying an empirically-relevant mechanism for collective choice that is not based on segregation.

These considerations explain why a monetary trade convention was so effective in supporting the transition to large-scale interaction. Money 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, money makes cooperators less reluctant to venture into groups of strangers. The experimental data offer strong evidence about this mechanism. A unique result is that only those experimental societies that were able to establish a strong convention of monetary trade managed to transition to a large and successful group. In fact, we find that poorly functioning monetary institutions proved to be a liability to large groups, lowering payoffs below those achieved in partnerships, and even if partnerships were designed to be less efficient.

These findings provide novel insights into the role played by monetary systems within the architecture of modern economic systems. They also bring forth new questions. For example, would subjects in an experiment collectively decide to adopt a monetary system, if given the choice? We also need to better

understand how monetary systems would interact with self-selection mechanisms: would we observe the emergence of separate groups, some using money and others relying on non-monetary institutions? We leave these questions to future research.

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A Appendix

A.1 Proof of Proposition 1

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 a sequential Nash 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.*

Let the payoff matrix in the stage game be defined below.

Producer

	No help	*Give help*
Consumer	$d - l, d$	$k, 0$

In the experiment $d = 6$, $l = 3$ and $k = 15$ in partnerships and 18 in large groups. In order to prove Proposition 1, we show that, if $\beta \geq \beta^* = \dfrac{d}{k - d + l}$, then the strategy in Definition 1 supports full cooperation in 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.

43

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 $h = 1, 2 \ldots$,*

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

and

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

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 if $T - t$ 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 $\beta \in (0, 1)$. $\qquad \square$

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

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

45

Now define

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

It is immediate that $\Delta_0(t = T - 2h) > \Delta_0(t \ge T)$; note that $k - 2d + l > 0$ by assumption. Also, $\Delta_0(t = T - 2h + 1) > \Delta_0(t \ge T)$; 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 $T - t \le 0$, then (2) holds for all t whenever

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

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

$\square$

Given that everyone else adopts 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 $\beta \ge \beta^*$ 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.

A.2 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. Recall that, according to the monetary trading strategy, equilibrium deviations do not trigger a switch in behavior. However, they alter the tokens' distribution, possibly only temporarily. To find a sufficient condition for the existence of a monetary equilibrium, we consider the best-case scenario where the distribution of tokens goes back to equilibrium in the second round of play after the defection. This will happen if, in the round after the deviation, the deviator meets the same counterpart again. Here, the incentive to deviate is the largest for a producer because the system is back in equilibrium two rounds after a unilateral deviation occurs.

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 moves off equilibrium and 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, after a unilateral deviation in t by a consumer, in the best-case scenario 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 adopts 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 $t + 2$.

A.3 Proof of Proposition 3

The payoff matrix in a round is

Outcome:	C	D
Consumer's payoff:	0	d
Producer's payoff:	g	$d - l$

with $d = 6$, $l = 3$, $g = 15$ in fixed pairs and 18 in large groups. The possible group size is $2n$, with $n = 1, 6, 12$.

Following the risk dominance concept in Blonski et al. (2011)—an indefinitely repeated prisoner's dilemma game in fixed pairs—consider uncertainty over two competing strategies: "grim" and "always defect" in CONTROL; "monetary trade" and "always defect" in TOKENS.

A.3.1 Control condition: the grim strategy is not risk dominant

Consider uncertainty over two competing strategies: "grim" (G) and "always defect" (AD). Initial producers select a strategy in round 1 and maintain it for the rest of the supergame. Initial consumers take no action in round 1, so we set them free to select G or AD in round 2. Given public monitoring, all uncertainty about future play is resolved at the end of round 1. If no-one (someone) defected then every producer cooperates (defects) in every future round. Hence, the choice of strategy G dominates AD in round 2 (weakly, if someone defects in round 1). The full cooperation payoff to a consumer, v, is larger than the full defection payoff, $\hat{v}$, since

$$\hat{v} := \frac{d - l + \beta d}{1 - \beta^2} \qquad \text{and} \qquad v := \frac{g}{1 - \beta^2},$$

with $\hat{v} < v$ since by assumption $2d - l < g$. Therefore, we say that a strategy is risk dominant if it makes an initial producer at least indifferent to choosing the competing strategy.

Large groups: there is strategic uncertainty in the first round because an initial producer is not sure what strategy the other $n - 1$ initial producers will select. Suppose that every initial producer believes that in round 1 there is probability p that C is the outcome in any given pair; D is the outcome with

48

the complementary probability. The probability p is easily mapped into beliefs about strategy selection: the player believes that every other initial producers plays G with probability p, and AD otherwise.

Given public monitoring, all uncertainty about future outcomes is resolved by the end of the round 1: either C will be the outcome in every meeting, or D will be the outcome in every meeting. The central question is how likely it is that full cooperation will emerge. Since the probability p of outcome C is independent across meetings, the initial strategic uncertainty increases with the group size $2n$. Fix an initial producer, and suppose he selects G. The probability that there is full cooperation in round 1 is p^{n-1}, i.e., the joint probability that C is selected by *all other* $n - 1$ producers. Here, full cooperation occurs forever after. With complementary probability $1 - p^{n-1}$ there is some defection in round 1, and full defection forever after.

Denote V_G and V_{AD} the expected payoffs for an initial producer who chooses strategy G and AD where

$$
\begin{aligned}
V_{AD} &= d + \beta \hat{v}, \\
V_G &= 0 + p^{n-1} \beta v + (1 - p^{n-1}) \beta \hat{v}.
\end{aligned}
$$

Consider V_{AD}: the initial producer defects so all future producers will defect (if they chose G or AD). Therefore, in round 2 the initial producer is a consumer with payoff $\beta \hat{v}$. Consider V_G: the initial producer cooperates but the continuation payoff depends on the outcome in all other round 1 meetings. With probability p^{n-1} every other producer is also a grim player so the continuation payoff is βv; otherwise, if some initial producer defects, the full defection continuation payoff is $\beta \hat{v}$. The key observation is that all strategic uncertainty is resolved by the end of round 1. We say that G is risk dominant if

$$
\begin{aligned}
V_G \geq V_{AD} &\Rightarrow p^{n-1} \beta (v - \hat{v}) - d \geq 0, \\
&\Rightarrow \beta^2 d (1 - p^{n-1}) + \beta p^{n-1} (g + l - d) - d \geq 0, \\
&\Rightarrow \beta \geq \beta^{**}(n)
\end{aligned}
$$

with

$$
\beta^{**}(n) := \frac{p^{n-1}(d - g - l) + \sqrt{p^{2(n-1)}(g + l - d)^2 + 4d^2(1 - p^{n-1})}}{2d(1 - p^{n-1})} \in (0, 1).
$$

A special case is $p = 0.5$, which may be motivated by the "principle of insufficient reason" for someone who is unsure about which of the two strategies other initial producers will choose. If so, then $\beta^{**}(6) = 0.976$ and $\beta^{**}(12) = 0.99$.

Since in the experiment $\beta^{**} > \beta = 0.75$ strategy AD is risk dominant. In CON-TROL strategic uncertainty prevents large groups from attaining the efficient outcome.

Fixed pairs: The analysis for the case of fixed pairs is an adaptation of the analysis above. The important difference is the absence of strategic uncertainty since there is just one player who takes an action in each round (the subject who is a producer in that round). In a sense, here the player who is a producer in round 1 gets to select the equilibrium and can therefore select the efficient equilibrium by cooperating in round 1. The reason is as follows: if the initial producer cooperates in round 1, then this reveals that she has selected strategy G. Therefore, the initial consumer faces no strategic uncertainty. In fact, choosing strategy G is always a best response for the player who is a producer in round 2 (even if the initial producer defects, as we noted above). Hence, adopting strategy G is optimal for the initial producer because there is no uncertainty over the strategy selected by the counterpart. This is the central difference between our helping game and the PD game in fixed pairs—it simplifies coordination on the efficient outcome in fixed pairs. Technically if $n = 1$, then $p^{n-1} = 1$ and hence $V_G \geq V_{AD}$ implies $\beta \geq \beta^* = \dfrac{d}{g + l - d} = 0.5$ since $g = 15$ in fixed pairs.

A.3.2 Tokens condition: monetary trade is risk dominant

When tokens are available we let "Monetary Trade" (MT) compete against AD. The main difference relative to CONTROL is that initial consumers must also select a strategy, since they have one token each and so their action set is non-empty. Note that MT is a history-independent strategy, unlike grim. The main implication is that histories of play in this scenario cannot affect future play and that the inefficient full defection outcome can arise only if all initial producers select AD.

It should be clear that since tokens are intrinsically worthless, MT is risk dominant for initial consumers, no matter the uncertainty over strategy selection by others. Offering a token *quid-pro-quo* for help can only increase an initial consumer's payoff from $d - l$ to g, without lowering her continuation payoff even if everyone else selects AD. It follows that initial strategic uncertainty matters only for initial producers, who give up d to receive an intrinsically worthless token from a consumer. We therefore say that MT is risk dominant if it leaves the representative initial producer at least indifferent to choosing the competing AD strategy.

Fixed pairs: the immediate implication is that strategic uncertainty is not an issue in fixed pairs. The initial producer can select the efficient equilibrium by choosing the MT strategy, knowing that MT is risk dominant for the initial consumer. Indeed, if both choose MT, then the efficient equilibrium is attained. Here the initial producer earns payoff $\dfrac{\beta g}{1 - \beta^2}$. Instead, if either player chooses AD, then the inefficient equilibrium is attained. Here, the initial producer earns payoff $\dfrac{d + \beta(d - l)}{1 - \beta^2}$, which is lower than the efficient equilibrium payoff if $\beta \geq \beta^* = \dfrac{d}{g + l - d} = 0.5$. Since $\beta = 0.75$ in the experiment, strategic uncertainty is not an issue in fixed pairs and monetary trade has no advantage over grim.

Large groups: to maintain comparability with the analysis in the CONTROL condition, let us consider uncertainty *over outcomes* in a meeting. The main difference is that the outcome in a meeting now involves not only C or D but also whether a token is transferred from consumer to producer or not, i.e., whether there is "trade" or no "trade." Let an initial producer believe that trade occurs with probability p in any given pair of round 1. In round 1, this probability p easily maps into beliefs about strategy selection. We have already established that MT is risk dominant for initial consumers. Hence, to simplify matters let us suppose that initial consumers assign probability one to MT being selected by those who are consumers in round 1. This implies that if an initial producer selects MT with probability p, then trade occurs with probability p in her round 1 match.

Hence, if we consider the initial round of play we have the following.

- Initial consumer (who has one token): if she chooses AD, then her payoff is $\dfrac{d - l + \beta d}{1 - \beta^2}$. As noted above, choosing MT is optimal because this gives her at least a chance to earn $g > d - l$ in round 1 and do no worse in future rounds than by choosing AD.

- Initial producer (who has no token): if she chooses AD, then she will never trade so we have the same expression as before, i.e.,

$$V_{AD} = \frac{d + \beta(d - l)}{1 - \beta^2}.$$

Instead, if she selects MT she expects to trade with certainty in round 1, since all initial consumers select MT (given the considerations above).

The continuation payoff, however, depends on what strategy was selected by all other initial producers. The payoff at the start of the game can be written as

$$V_{MT} = 0 + p^{n-1}\frac{\beta g}{1 - \beta^2} + (1 - p^{n-1})\beta V_1,$$

where V_1 denotes the expected payoff if not everyone trades in round 1, which we now calculate.

The problem in calculating V_1 is that, unlike CONTROL, strategic uncertainty in TOKENS gets resolved in round 1 *only if* trade occurs in every meeting—an outcome that can be publicly observed. In that case, the continuation payoff for an initial producer (who also chose MT) is $\frac{\beta g}{1 - \beta^2}$. However, strategic uncertainty remains if not everyone trades in round 1, because the distribution of outcomes is not made public. Hence, if full cooperation is not realized in round 1, then we must account for uncertainty over outcomes in all future rounds. The probability of trading in such future meetings depends on the distribution of tokens, which evolves at random and is unobserved by players. To see this, note that if someone does not adopt MT, then tokens will not be exchanged in some pairs so as play progresses some producers will have a token, while some consumers will not. Hence, monetary trade may fail to occur even in meetings between players who have each selected MT. Assessing this trading uncertainty is problematic because the distribution of tokens evolves based on random meetings. For an initial p, we can find a long-run probability trading in a meeting using a technique similar to the one adopted to calculate payoffs off monetary equilibrium in Bigoni et al. (2015). As these calculations are lengthy and elaborated for participants, we adopt a more reasonable, heuristic approach. We simply suppose that if monetary trade does not occur in all initial meetings, then an initial producer will naively assign the same probability p of trading in any future meeting in which she is either a producer without tokens, or a consumer with a token.

Given this heuristic approach, consider a player who initially selected strategy MT, when strategic uncertainty was not resolved in round 1. Let V_0 and V_1 denote the expected utilities at the start of any round after the first, if the player is, respectively, a producer without a token and a consumer with a token. We have

$$
\begin{aligned}
V_0 &= p(0 + \beta V_1) + (1 - p)[d + \beta(d - l + \beta V_0)], \\
V_1 &= p(g + \beta V_0) + (1 - p)[d - l + \beta(d + \beta V_1)].
\end{aligned}
$$

The player expects not to trade with probability $1 - p$. As this implies no change in her token inventory, the player cannot trade in the next round, either. If she is a producer who does not sell, then she will have no token to spend next round, as a consumer. If she is a consumer who does not buy, then she keeps the token and will not need to sell next round. Hence, it takes two rounds to have a new chance to trade.

Rewrite

$$
\begin{aligned}
V_0[1 - (1-p)\beta^2] &= p\beta V_1 + (1-p)[d + \beta(d-l)], \\
V_1[1 - (1-p)\beta^2] &= pg + p\beta V_0 + (1-p)(d - l + \beta d).
\end{aligned}
$$

Substituting we have

$$
\begin{aligned}
V_1 &\left[1 - (1-p)\beta^2 - \frac{(p\beta)^2}{1 - (1-p)\beta^2}\right] = pg \\
&+ \frac{\beta p(1-p)[d + \beta(d-l)]}{1 - (1-p)\beta^2} + (1-p)(d - l + \beta d).
\end{aligned}
$$

The monetary trade strategy is risk dominant for initial producers if $V_{MT} \geq V_{AD}$. Given $p = 0.5$, we have $V_{MT} \geq V_{AD}$ for all $\beta \geq 0.63$ approximately if $n = 6$, and $\beta \geq 0.64$ approximately if $n = 12$. Hence a (long-run) 50-50 chance to trade in a round still supports the efficient equilibrium in the TOKENS conditions, because it makes monetary trade risk-dominant.