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Money is more than memory*

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Abstract

Impersonal exchange is the hallmark of an advanced society and money is one key institution that supports it. Economic theory regards money as a crude arrangement for monitoring counterparts' past conduct. If so, then a public record of past actions—or *memory*—should supersede the function performed by money. This intriguing theoretical postulate remains untested. In an experiment, we show that the suggested functional equivalence between money and memory does not translate into an empirical equivalence: money removed the incentives to free ride, while memory did not. Monetary systems performed a richer set of functions than just revealing past behaviors.

Keywords: Cooperation, intertemporal trade, experiments, institutions, social norms.

JEL codes: C70, C90, D03, E40

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1 Introduction

People have an inclination for cooperation, but such predisposition is weakened when the sphere of interaction expands from personal to *impersonal* as it happens in advanced societies, where interactions often take place among strangers. These cooperation challenges have led to the creation of a variety of institutions.[1]

Our focus is on money, an institution that is ubiquitous across regions, cultures and historical periods, but whose nature continues to be enigmatic. While theory and empirical evidence indicate that monetary exchange grants efficiency gains compared to barter or gift-exchange, the mechanism behind this result remains open to debate. Understanding it can generate valuable insights into the function and (in)stability of traditional currency systems and the usefulness of possible digital alternatives (Camera, 2017).

Here we present a laboratory experiment designed to fill these important gaps. Theory views money as a crude monitoring system—a type of "public memory"—which has no role to play when individuals can rely on shared knowledge of past conduct. An important implication of these theories is that money is subordinate to public monitoring systems, which, if available, would be used to replicate or improve upon monetary trade (Kocherlakota, 1998; Ostroy, 1973; Townsend, 1987). Here we offer an empirical test of this theoretical concept, using a design that is not tied to any specific monetary model. We find evidence that money performs a richer set of functions than just revealing past behaviors. In a set-up where multiple equilibria coexist, we show that the institution of monetary trade is more powerful than reputation-based systems in enabling coordination on more efficient outcomes.

[1]For instance, see Binmore (2011); Bowles and Gintis (2011); Capra et al. (2009); Greif (2006); Kimbrough et al. (2008); McCabe et al. (1998); North (1991); Ostrom (2010).

The experiment consists of a cooperative task involving subjects who interact as strangers for an indefinite number of periods. In each period, subjects meet in pairs, where one has the option to help the other at a cost. Everyone has repeated opportunities to help and to receive help because roles alternate over time. Cooperation requires trusting that help given to a stranger will be returned by a stranger later in the game.

In our Baseline treatment, indefinite repetition gives rise to a social dilemma with two conflicting elements: opportunism, due to the short-run temptation to avoid helping others, and coordination, because many cooperation levels – from 0 to 100 percent – are theoretically feasible. This holds true in all other treatments. In the Money treatment, we add a fixed supply of intrinsically worthless, abstract tokens that can be exchanged for help, in line with a monetary trade strategy. In the Memory treatment, we add a record-keeping system based on numeric balances that rise when help is given, and fall when it is received. This is designed to capture the operating principle behind the theory in Kocherlakota (1998), i.e., there is a technology that allows the public identification of deviations from equilibrium, and it also allows the replication of the monetary trade strategy. In particular, it ensures the key requirement that subjects are strangers, who are unable to observe the past conduct for any of their potential future counterparts.

To identify possible advantages of the institutions we bring to the lab, we create a playing field that is "theoretically level." By design, tokens and record-keeping cannot expand the efficiency frontier as compared to self-enforcing norms alone. Money and Memory allow help to be based on balances in the pair. In this sense, tokens and record-keeping are theoretically similar: record-keeping can be used to replicate a pattern of monetary trade without exchanging tokens, while tokens summarize individual past conduct in lieu of record-keeping. Through this design we can uncover if monetary systems affect behavior differently than systems for collecting and sharing information about past conduct.

The data reveal that institutions like tokens and record-keeping—which do not alter the efficiency frontier—each significantly boosted long-run cooperation as compared to the Baseline. Most importantly, we find that long-run cooperation in Money was significantly higher than in Memory. Tokens encouraged cooperation because subjects took turns at trading them for help. This alternation did not emerge in Memory, where cooperators accumulated large numeric balances, thus allowing free-riders to run large deficits. This suggests that Money facilitates the task of coordinating on credible, incentive-compatible trade patterns, because tokens are available in limited supply. Hence "liquidity" constraints could be instrumental to the superior performance of money. Evidence in favor of this conjecture comes from the Money Unconstrained treatment, where we removed liquidity constraints, so that—as in Memory—help could always be rewarded with a symbolic object. Finally, when tokens and a record-keeping system are both available, participants do not condition help on the opponent's past conduct, as they do in Memory, but follow a monetary trade strategy akin the one seen in Money. This evidence comes from the additional treatment Money+Memory, and reinforces our main result. In sum, our findings indicate that the addition of systems for collecting and sharing information about past conduct provided weaker dynamic incentives to cooperate compared to a monetary system.

Previous experiments with repeated social dilemmas indicate that providing information about opponents' past actions fosters reciprocity and conditional cooperation (Camera and Casari, 2009; Gächter and Hermann, 2011; Milinski et al, 2001; Ule et al, 2009). Our results from Memory are in line with this literature. Notice, however, that the forms of memory that are behaviorally most effective include “second order information”, designed to support the sanctioning of non-punishers (e.g., Bolton et al., 2005; Ule et al, 2009). Our design does not provide this information, because it is not relevant in monetary theory. Empirically, monetary systems are more effective at promoting cooperation and efficiency, even though they do not include second order information. We show that liquidity constraints are crucial for this result. With monetary trade, the only way of having a positive balance is to help someone who has a positive balance. This automatically implies a sanctioning of free riders (who have zero balance) *and also* of those who help them (whose balance remains zero). This double sanctioning mechanism is embedded into monetary trade, but not into our record-keeping system.

The paper proceeds as follows. In Section 2 we provide an overview of the related literature. Section 3 describes the design, Section 4 presents the theory, and Section 5 reports the results. Section 6 offers a concluding discussion.

2 Related studies

Our paper contributes to the experimental literature on money and on memory. These two strands of literature have developed separately.

Laboratory experiments on memory primarily study whether providing information about past actions of counterparts facilitates cooperation. The typical finding is that this information is beneficial to cooperation. Memory is modeled as a “scoring” system, and the interaction takes the form of a finitely repeated helping game.[2] Individuals have a score

[2]Some studies adopted indefinite horizon games such as prisoners’ dilemmas or trust games to investigate the connection between knowledge of opponents’ histories and cooperation (e.g., Bohnet and Huck, 2004; Camera and Casari, 2009). A hybrid design is in Offerman et al (2001), where subjects plays a one-shot, one-sided giving problem, in a sequence of unknown length.

consisting of a summary of their recent actions (e.g., Ule et al, 2009), hence the score typically accounts only for the help given, but not for the help received. In some designs, the score is enriched with additional "second order" information, suggestive of possible motives driving uncooperative actions (i.e. distinguishing opportunistic behavior from punishment of defections, e.g., Bolton et al., 2005; Milinski et al, 2001). The form of memory studied in our experiment does not include this kind of second order information. It also differs in two additional dimensions: the score of a player accounts for all past periods of play (not only the recent ones) and for the help received (not only the help given).

A second question tackled by previous experiments on memory concerns how "rich" the records of past actions must be in order to foster cooperation. An important result is that, while perfect monitoring of individual behavior easily supports cooperation, anonymous public monitoring does not (e.g., Camera and Casari, 2009). Removing anonymity enables relational contracting, which is a strong empirical force behind cooperation. By contrast, our design does not introduce information that allows relational contracting, because our focus is the study of interactions among *strangers*.

Our paper also contributes to the small but growing literature that studies fiat monetary systems in the laboratory. Broadly speaking, there are two strands of this literature: one in which money *must* be used in order to maximize efficiency (Deck et al, 2006; Huber, 2014; Marimon and Sunder, 1993; McCabe, 1989), and another in which money is *not needed* to support efficient outcomes, because players can simply rely on a norm of mutual support or, gift exchange (Camera and Casari, 2014). Results from this second strand of experiments reveal that fiat monetary systems spontaneously emerge in the laboratory (Camera et al., 2013; Duffy and Puzzello, 2014), and support the creation of large economies with higher efficiency as compared to when monetary exchange cannot take place (Bigoni et al., 2019). Our work belongs to this second strand of studies, and complements three related pieces of research on the behavioral importance of monetary systems (Bigoni et al., 2019; Camera and Casari, 2014; Camera et al., 2013), where players cannot observe their counterparts' past conduct. Instead, here we relax this informational constraint. In two of our treatments we introduce an information-sharing system that accurately tallies the help given and received by every player over time. A reputational mechanism can thus emerge. This allows us to investigate a fundamental question in monetary theory: does money act just as a carrier of information about past conduct (Kocherlakota, 1998; Ostroy, 1973)?

To understand the contribution of this study relative to our earlier works, we discuss two issues. First, whether or not a monetary system is theoretically efficient. The designs in Camera et al. (2013) and Camera and Casari (2014) are tailored to ascertain whether or not monetary exchange emerges even when it is Pareto-inferior to non-monetary equilibrium. Here instead, monetary exchange is by design efficient. Second, how the emergence and stability of a monetary system depends on the size of the trading group. Camera et al. (2013) and Bigoni et al. (2019) tackle this problem by considering endogenous versus exogenous variation in groups size. By contrast, here we work with a fixed group size to hold informational and coordination frictions constant.

This work is also related to experiments on cooperation in repeated social dilemmas, which one can classify along three dimensions: the time horizon of interaction, the task in the stage game, and the matching protocol in the supergame. Our paper studies an *indefinitely* repeated social dilemma, which supports a richer set of equilibria compared to interaction that is one-shot or with a commonly known number of periods (Dal Bó, 2005;

Palfrey, 1994). Now consider the task in the stage game. Experiments on indefinitely repeated games have focused on tasks in which all subjects make a decision in every period, e.g., prisoners' dilemmas, voluntary contribution mechanisms, Bertrand duopolies, or trust games (see Bigoni et al., 2012; Engle-Warnick and Slonim, 2006; Kurzban and Houser, 2005; Roth and Murnighan, 1978). By contrast, we employ a helping game, in which a dictator takes a decision while the counterpart remains passive (Nowak and Sigmund, 1998). We use this task because it is simple to understand and focuses participants' attention on the intertemporal aspect of the interaction. This task is also at the core of a large class of models used to study money (see Kocherlakota, 1998).

The third dimension is the matching protocol. Typically, experiments on indefinitely repeated games involve interactions in fixed pairs (e.g., Blonski et al., 2011; Dal Bó and Fréchette, 2011). This allows the study of cooperation in small societies, where people know others' past behavior. Instead, we consider societies of strangers, where subjects cannot rely on reciprocity and cannot build reputation. This design is better suited to study cooperation when interactions are anonymous, as prescribed by the standard monetary model. There is a related literature on this theme and on institutions that can foster cooperation, such as personal punishment, information-sharing systems, and communication in environments with multiple Pareto-ranked equilibria (Camera and Casari, 2009; Capra et al., 2009).

3 Experimental design

The experiment has three main treatments: Baseline, Money and Memory (Table 1). Two additional treatments, Money+Memory and Money Unconstrained, serve as a robustness check and are discussed in Sections 5.1-5.3. In all treatments subjects face a cooperative task that is repeated an indefinite number of periods, where every period subjects encounter a random counterpart and play in pairs. Interactions are anonymous, and communication is ruled out. The design in Baseline is described next.

Variable	Treatment				
	Baseline	Memory	Money	Money+ Memory	Money Unconstr.
Group size	8	8	8	8	8
Token supply	0	0	4	4	≥ 4
Record-keeping	No	Yes	No	Yes	No
Sessions	4	5	4	4	2
Subjects	96	120	96	96	48
Supergames	20	25	20	20	10
Periods (avg.)	111.5	117.6	116.2	114.0	110.0

Table 1: Sessions and treatments

Notes: Sessions' dates (dd-mm-yy): Baseline, 6-2-12 (two), 24-1-14, 20-2-12; Money, 7-2-12 (two), 24-1-14, 16-2-12; Memory, 13-2-12 (two), 21- & 23-1-14, 27-1-14; Money Unconstrained, 12-6-14 (two); Money+Memory,2-3-18, 5-3-18, 7-3-18, 9-3-18. We run the 2012 sessions at Purdue University (VSEEL lab) and the 2014 and 2018 sessions at Chapman University (ESI lab). The sessions on 20-2-12, 16-2-12, 27-1-14, and 9-3-18 were run with experienced subjects (=experienced sessions): subjects were informed that all session participants had previously participated in a session with the same treatment.

3.1 Interaction in a period

Each period players meet in pairs to play a "helping game" (Table 2). In the pair, one player is a producer, and the other a consumer. The producer has a good, which he can consume or transfer to the other who values it more (help).

	Producer	
	Y	Z
Consumer	$d - l, d$	$g, 0$

Table 2: Payoffs in the stage "game" in Baseline and Memory

In BASELINE the consumer has no action to take (Table 2). The producer chooses either outcome Z (=*Help*) or Y (= *Do not help*). Payoffs to consumer and producer are, respectively, g and 0 if the producer helps; otherwise, they are $d - l$ and d, with $g > 2d - l > 0$. In the experiment $d = 6$, $l = 2$, $g = 20$ and each point is worth \$0.03. Surplus in a pair is maximum when the producer helps, which generates $g - (2d - l) = 10$ points. We refer to this outcome as the (socially) efficient outcome or, alternatively, *cooperation*. The dominant action is not to help, which we call *defection*. At the end of the interaction actions and outcome in the pair are observed by both agents.

3.2 The supergame

A session comprises a sequence of five separate supergames. In a supergame, subjects interact within a fixed group of eight subjects, for an indefinite number of periods. A group is comprised of four producers and four consumers with deterministically alternating roles. At the start of every period each consumer meets a producer at random. According to this matching protocol, there is only a 0.25 probability to be in the same pair in two consecutive periods.[3] Participants can never identify their opponent. Hence, subjects interact as strangers because opponents change at random and are anonymous.

The duration of the supergame is determined by a random continuation rule (Roth and Murnighan, 1978). A supergame has 20 fixed periods after which the game continues into an additional period with probability $\beta = 0.75$, which we interpret as the discount factor of a risk-neutral subject. The design guarantees an interaction of finite but uncertain duration; the expected duration is 23 periods; from period 20, in each period the supergame is expected

[3]There are 4! ways to match four producers to four consumers; in 3! of such pairings consumer j meets producer i. In each period one pairing is chosen with equal probability.

to go on for 3 additional periods. In the experiment a computer randomly selects an integer number between 1 and 100, using a uniform distribution, and the supergame ends when a number greater that 75 is selected. At the end of each period all participants in the group observe the number drawn, which informs them about the end or continuation of the supergame, and can also serve as a public coordination device. Subjects also observe whether or not outcomes were identical in all four pairs (a binary variable, "yes" or "no;" see the instructions in Appendix C). This statistic provides a form of anonymous public monitoring, which is introduced to ensure that the minimum discount factor supporting full cooperation in sequential equilibrium is constant across treatments (see Section 4).[4]

Each session involves twenty-four subjects, divided into three groups in each supergame for a total of fifteen groups per session. Supergames terminate simultaneously for all groups. As explained in the instructions, each group is constructed so that no two subjects can interact in more than one supergame.

3.3 Money and Memory treatments

The Money treatment adds indivisible, intrinsically worthless electronic objects called "tokens," which neither yield nor can be redeemed for points or dollars. In period 1 of each supergame, every consumer is endowed with one token, hence there are four tokens per group; this supply is known and remains fixed throughout the supergame. Tokens can be transferred from consumer to producer, one at a time, and can be carried over to the next period but not to the next supergame. Participants can hold any positive balance of tokens.

[4]The chosen form of public monitoring may also simplify coordination tasks as compared to other forms, such as revealing the frequency of actions in the group. A red flag is a signal less open to interpretation than a frequency-based signal. It is conceivable that outcomes could differ without public monitoring. Camera and Casari (2009) investigate this issue and find that making outcomes public does not alter cooperation rates, as compared to the case in which this information is concealed.

Producer

		Y	Z	Z *for* 1	Z *for* 0
	give 0	$d-l,d$	$g,0$	$d-l,d$	$g,0$
Consumer	*give* 1	$d-l,d^\star$	$g,0^\star$	$g,0^\star$	$d-l,d^\star$
	1 *for* Z	$d-l,d$	$g,0^\star$	$g,0^\star$	$d-l,d$
	1 *for* Y	$d-l,d^\star$	$g,0$	$d-l,d^\star$	$d-l,d^\star$

Table 3: The augmented stage game in MONEY

Notes: $\star$ denotes the transfer of a token from consumer to producer. In the experiment $d=6, l=2, g=20$.

The introduction of tokens expands the actions sets relative to BASELINE because the stage game now includes the possibility to *trade* using a direct mechanism; Table 3 explains how. A consumer can either keep her tokens (=*give* 0), transfer one to the producer (=*give* 1), transfer one conditionally on receiving help (=1 *for* Z) or on *not* receiving it (=1 *for* Y). The producer can still help (= Z) or not (= Y), but now can also choose to help conditionally on receiving either one (=Z *for* 1) or no tokens (=Z *for* 0). Each pair of choices is associated with a unique outcome, which is reported in Table 3 along with the relevant payoffs.[5] Subjects choose simultaneously and without prior communication, hence, they cannot signal a desire to cooperate by requesting or offering a token. In particular, nothing prevents producers from unilaterally providing help, if they wish to do so.[6]

[5]The instructions discuss the outcomes for each choice combination. Before starting to play, subjects had to correctly answer twenty-five multiple-choice questions, including questions about the association between choices and outcomes.

[6]There are three main differences with respect to the designs in Camera et al. (2013) and Camera and Casari (2014): here subjects deterministically alternate between the consumer and producer roles (rather than randomly), token holdings are unrestricted (rather than bounded), and consumers and producers have four choices rather than three.

Several remarks are in order. First, the possibility of conditioning the outcome on the counterpart's choice might facilitate coordination on cooperation. The producer can choose to help conditional upon receiving a token, and the consumer can choose to transfer one token conditional upon being helped. Helping only in return for a token is a form of monetary exchange, which can also be achieved by choosing the actions Z and *give* 1.

Second, to avoid biasing the results in favor of the emergence of monetary exchange the design includes actions that are antithetical to monetary exchange. By choosing Z *for* 0, the producer commits to execute Z only if the consumer chooses *give* 0. By choosing 1 *for* Y, the consumer commits to transfer a token if the producer *avoids* Z. Hence, tokens may take on a negative connotation as subjects could use them to tag defectors by giving tokens to those who do not help. Given this richer action set, the addition of tokens might increase coordination problems, relative to BASELINE.

Third, subjects cannot create or borrow tokens, so a consumer without tokens has no action to take, as in BASELINE. This possibility of being "liquidity constrained" is at the heart of monetary economics, and also key in our study as it allows us to investigate whether removing such constraints helps to improve overall efficiency. Subjects are informed whether a token transfer is feasible in their pair; the design minimizes the chance that this information might indirectly identify the opponent. Before making a choice, subjects can see if the opponent's tokens balance is positive or zero, but not the number of tokens held. The restriction to transfer one token at a time is not theoretically binding in monetary equilibrium.

Finally, though subjects can hold any number of tokens, a balance of one token per consumer is all that is needed for the monetary system to function efficiently because with deterministic alternation of roles there is no precautionary motive to hold tokens (Section

4). This explains our calibration of the tokens' supply: with less than four tokens monetary exchange would be sometimes unfeasible; adding tokens cannot increase the cooperation frequency, and in fact would undermine it by reducing the endogenous value of tokens.

The Memory treatment retains the Baseline stage game and adds an information-sharing system, or (*public*) *record-keeping*. This treatment is designed to capture the operating principle behind the theory in Kocherlakota (1998), i.e., there is a technology that allows public monitoring of deviations from equilibrium, and it also allows the replication of the monetary trade strategy without exchanging physical objects. In particular, we must ensure that access to information is limited in the sense that a player "is unable to observe the entries in the record [i.e., the past conduct] for any of his potential future trading partners."[7] In that paper, this follows from the assumption of an infinite population, which ensures a very strict type of informational isolation: agents are complete strangers (as demonstrated in Aliprantis et al., 2007, Theorem 9). In the experiment, we are constrained to finite populations, hence, subjects can meet repeatedly within each supergame. Our Memory treatment is designed to capture with finite populations the stranger's interaction of the infinite-population model. In order for subjects to interact as strangers, we should not provide full individual histories; doing so would undermine anonymity and allow identification of past counterparts. For this reason, we provide only a summary statistic, which is sufficient to replicate the monetary strategy. As a result, as in Kocherlakota (1998), players *cannot* have public information about every interaction since the beginning of the game.

To do so, in our Memory treatment the information-sharing system assigns to each

[7] This assumption is in Kocherlakota (1998, p. 239). It implies that although a complete historical record of players' past actions exists, there are limitations about what players can see about *future* opponents, of which there is an infinite number. In addition, it is assumed impossible for two individuals in a pair to have had any direct or indirect contact in a past round (p. 236).

subject a numeric *balance* (“personal index,” in the instructions), which tallies the help given and received in the past. The initial balance is 1 for consumers, 0 for producers. As in Money, balances are intrinsically worthless and subjects only see if the opponent’s balance is positive or not. The difference with Money is that balances are automatically updated at the end of each interaction, based only on the producer’s action. If Y is chosen, then balances in the pair do not change. If Z is chosen, then the producer’s balance increases by one and the consumer’s falls by one; balances can be negative. If subjects condition their help on balances in their pair, then Memory simplifies coordination tasks relative to Money because choice sets are smaller (as in Baseline) and balance updates are automatic. In this manner, the Memory design ensures that the information about the counterparts’ past actions is comparable to that available in Money, as the interaction remains anonymous. An alternative design with precise numeric balances could have the drawback of allowing identification of past opponents—altering the interaction from impersonal to personal.[8]

Procedures. We recruited 456 subjects through announcements in undergraduate classes, at Purdue and Chapman University. The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007). Instructions were read aloud at the start of the experiment and left on the subjects’ desks. No eye contact was possible among subjects. Average earnings were \$27.94 per subject (min = \$18.84, max = \$40.94). On average, a session lasted 115 periods for a running time of about 82 minutes (min = 57 minutes, max = 108 minutes excluding instruction reading, a quiz, and payments).

[8]Neither the personal index nor tokens can improve subjects’ recollection of their own past choices (e.g., the accounting system in Basu et al., 2009) relative to Baseline, since an electronic record of subjects’ own past decisions was visible on their screen in all treatments.

3.4 Design choices: additional considerations

Four features of our design must be discussed in more detail: size of interaction group, framing of instructions, payoff matrix, and anonymous public monitoring.[9] Here we illustrate how these features were informed by the evidence collected through the companion studies in Camera and Casari (2014); Camera et al. (2013), and by theoretical considerations.

First, players interact in groups of eight. This could be perceived as being "too small" to study monetary exchange among strangers. The choice of group size is dictated by practical constraints and by the need to eliminate reputational spillovers from one supergame to the next. Working with groups of eight allows us to implement perfect-stranger matching across cycles, with 24 participants per session, which is in line with the standards adopted in most economic experiments. Would the pattern of monetary exchange differ, had we implemented larger groups? Evidence from a previous study suggests this is not so. Camera et al. (2013) varied the size of economies from 4 to 8, to 32, with a design similar to our MONEY treatment. They found that variation in group size neither affected the frequency of monetary exchange, nor the efficiency levels. In light of this, we do not anticipate that increasing the groups' size would alter the main message of this paper.

Second, one may wonder whether the subjects' life experience with money foster cooperation in MONEY relative to the MEMORY by facilitating the adoption of a monetary trade convention. To address this concern, the instructions employ an abstract and neutral language, which has no references to money or trade. Participants' roles are "red" and "blue" (not "seller" and "buyer"), the possible outcomes are "Y" and "Z" (not "cooperate" and "defect" or "buy" and "sell"), and tokens are called "tickets" (not "coins" or "francs") and

[9] We thank anonymous Reviewers for raising these issues.

cannot "be redeemed for dollars." As a further check, subjects are given the option to engage in actions that are antithetical to monetary exchange (see Table 3).

In fact, participant's familiarity with money outside the lab is unlikely to be responsible for the experimental results about tokens. Short of running an experiment in small-scale primitive societies, support for this claim may come from comparing inexperienced subjects' behavior across treatments, at the inception of the game. If familiarity with real-world money is what drives subjects' choices, then monetary equilibrium should easily emerge and immediately prevail over alternative outcomes. This implies that efficiency in the first supergame should be higher when tokens are present than when they are not. Instead, in previous studies the opposite pattern emerges (Camera and Casari, 2014; Camera et al., 2013): subjects "discovered" monetary exchange in the experiment. As we will see, the same happens in this experiment: the exchange of tokens is very limited at the beginning, and becomes widespread only following considerable experience within the laboratory.

Third, we adopt a matrix with asymmetric payoffs. It is well known that payoff variations may alter behavior (Dal Bó and Fréchette, 2018). For example, the payoff asymmetry between producers and consumers may affect choices due to fairness considerations (Fehr and Gächter, 2000). Results from previous experiments do not support this hypothesis: the emergence of monetary exchange is robust to manipulating the payoff matrix so that defection payoffs are symmetric or the cost-to-benefit ratio of cooperation is different (Bigoni et al., 2019; Camera and Casari, 2014; Camera et al., 2013). The emergence of monetary exchange is also robust to increasing the imbalance of cumulative payoffs, which emerge when consumer/producer roles alternate randomly rather than deterministically, as in this paper. More importantly, one may worry that in Memory and Money accumulation of high balances may attract envy. This is precisely the reason why in our design cumulative earnings and balances are unobservable and nearly impossible to infer, as individuals are strangers whose histories remain private throughout the experiment. This design feature minimizes the potential impact of fairness considerations.

Fourth, our design includes anonymous public monitoring of defections. This gives the best shot at memory to outperform money. To see why this is so, consider the operating principle behind the theory in Kocherlakota (1998). There, the efficient outcome is supported using the following two-state informational structure. If there is a deviation, then every individual is unmistakably identified either as (a) deserving of a reward (cooperation) or (b) deserving of punishment (defection). State (b), if reached, is absorbing. State (b) characterizes the initial deviator, as well as anyone who *has knowledge* of the presence of a deviator (independent of whether they have always cooperated or not). State (a) comprises everyone else who does not fit into state (b). In this context, memory is a technology that plays two roles: (i) it allows such an identification of individuals as either being in state (a) or (b); and (ii) it allows the replication of the monetary trade strategy. With this technology, one can construct the following punishment: someone who deviates from full cooperation will be permanently punished immediately after his deviation. The same punishment is applied to all those who have knowledge of a deviation. As a consequence, *all those* who have defected, or have knowledge of a defection, will never receive a reward in the continuation game (this is called autarchy in the original paper). It is this grim punishment which theoretically deters the deviation from happening in the first place. Given this, the most powerful and simplest “memory technology” is one in which knowledge of a defection is immediately shared with everyone in the group, which is precisely what we do in our set-up.

4 Theoretical considerations

We start by showing that a social norm of cooperation supports full cooperation as an equilibrium in all treatments. Proofs and mathematical details of this section are reported in Appendix A. Consider the following strategy:

Definition 1 (**Cooperative strategy**). *As a producer, the player* cooperates *(selects Z) as long as she has not observed a* defection *(Y). If a defection is observed, then the player defects forever after.*

Common adoption of this strategy gives rise to a *social norm*, which consists of a rule of cooperation and a rule of punishment that sanctions any publicly observed uncooperative action with full defection. The norm exploits the availability of anonymous public monitoring, and is constructed so that after any history of play, conduct in the continuation game is part of some equilibrium of the original game (Abreu et al., 1990), either full cooperation or full defection. Hence, the game has a simple recursive structure as in the public observability case studied in Kandori (1992). If players are sufficiently patient, then the punishment threat can adequately deter any defection, and full cooperation is a sequential equilibrium.

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

The threshold value β^* is the cost-benefit ratio of cooperating: the producer's cost from helping is divided by the consumer's surplus from being helped. The condition $\beta \geq \beta^*$ is sufficient and necessary for existence of cooperative equilibrium but does not guarantee that it will be realized instead of another outcome with lower efficiency. In fact, thanks to public monitoring of defections, multiple equilibria exist ranging from full defection to full cooperation. Full defection is always an equilibrium because it consists of an infinite repetition of the static Nash equilibrium strategy (Y). Full cooperation is socially efficient because it maximizes surplus in all meetings.

In Appendix A we prove that full cooperation is an equilibrium by checking two conditions. First, in equilibrium, no producer should prefer to defect. Second, given that everyone else follows the candidate strategy in Definition 1, no producer should prefer to cooperate off-equilibrium. The latter condition is immediately verified: any equilibrium defection is publicly observed, hence, everyone defects forever after and there is a loss from cooperating. The first condition requires checking that a producer cannot improve her payoff by moving off equilibrium (unimprovability criterion). The design parameters yield $\beta^* = 0.375$, so under reasonable assumptions about subjects' risk attitudes cooperation is an equilibrium in every treatment as the continuation probability from period 20 on is 0.75.

It is important to note that we have derived conditions sufficient for the existence of the efficient equilibrium using the public trigger which is available in our design. The natural question is what would happen if anonymous public monitoring were not available. The analysis in Camera and Casari (2014, online appendix) deals precisely with that case. The upshot of that analysis is that, with private monitoring, we would obtain a threshold lower bound discount factor above β^* because a deviation could not immediately trigger a permanent state of full defection. This delay in punishment would raise the payoff from moving off the equilibrium path.[10]

[10] Punishment under private monitoring takes the form of a contagious process of defections, which spreads through the economy via the random matching process. Camera and Gioffré (2014) provides a general characterization of this process, with and without tokens.

4.1 Equilibrium with tokens

Adding tokens adds actions and expands strategy sets but does not expand the set of equilibrium payoffs because the efficient outcome is an equilibrium without tokens. In fact, adding tokens does not eliminate *any* of the equilibria possible in Baseline as players can always adopt strategies that ignore tokens (which have no intrinsic value). Tokens can be used to support the efficient outcome. Following monetary theory, below we consider a strategy that conditions cooperation on the transfer of a token, letting H (=positive) and L (=zero) denote the observable *balances* in a pair.

Definition 2 (**Monetary trade strategy**). *In any period and after any history: a consumer with balance L has no action to take, and a consumer with balance H transfers one token upon receiving help. A producer with balance L helps upon receiving a token, and a producer with balance H does not help.*

If everyone adopts this strategy, then tokens are exchanged quid-pro-quo for help, and become a medium of exchange. This outcome is called *monetary trade*. In equilibrium all encounters are *trade meetings* in which the consumer "buys" help by giving her only token to the producer who has no tokens, as in a Turnpike model (Townsend, 1980). The monetary trade strategy is cognitively simple: it is history-independent and does not require any change in behavior as a reaction to a defection. Off-equilibrium a producer may have tokens or a consumer may have none, in which case tokens are not exchanged and help is not given. Because the theory tacitly assumes that agents coordinate on the best available equilibrium, tokens are irrelevant.[11] Consumption patterns in monetary equilibrium mirror those under the social norm—so payoffs coincide with v_0 for a producer and v_1 for a consumer—and the efficient outcome is supported on the same parameter set of the social norm.

[11] In monetary theory, tokens are said to be theoretically relevant (or, essential) only if monetary trade expands the set of equilibrium payoffs (see Kocherlakota, 1998, p. 232).

Proposition 2. *If $\beta \geq \beta^*$, then monetary trade supports full cooperation as an equilibrium.*

In monetary equilibrium a producer who refuses to help is "punished" by not receiving a token. The player will not be able to consume next period, much as it happens under the social norm, albeit for different reasons. This explains why the lower bound β^* is the same as under the social norm.

4.2 Equilibrium with record-keeping

Adding record-keeping leaves unaltered the action sets compared to Baseline. It enriches the strategy sets because producers can now condition actions on observed balances, denoted L (0 or below) and H (1 or above). This eliminates none of the equilibria that are possible in Baseline because players can always adopt strategies that ignore balances. Yet, there are ways in which balances can be employed to support full cooperation. In particular, subjects can replicate the monetary trade strategy without the need to exchange symbolic objects. Hence, equilibrium outcomes exist in which the balances L and H convey the same information about past actions as under monetary exchange.[12]

Definition 3 (Trade strategy). *In any period and after any history, the player takes an action only as a producer. If her balance is L, then she helps only consumers with balance H. In all other circumstances, she does not help.*

This strategy supports full cooperation because, as in Definition 2, help is *conditioned* on balances in the pair. Producers help only to increase their balance above zero, and do so only if the consumer's balance is H. It follows that the trade strategy supports full cooperation when $\beta \geq \beta^*$. Hence, record-keeping does not expand the set of equilibrium payoffs.

[12] Off-equilibrium, information about past actions *might* differ across treatments. For instance, in Money a consumer with balance L might have helped without receiving a token, while in Memory producers who help always increase their balance. The balances neither allow identification of past opponents, nor support reputation-based strategies. Even if other ways exist to use record-keeping to support efficient play, these different strategies cannot improve equilibrium payoffs over the trade strategy.

However, there are two differences between using the trade strategy in Money and in Memory, which may alter behavior in ways that are not predicted by the theory. First, the record-keeping system may simplify coordination on efficient play as only producers make choices and balances are automatically updated. Instead, in a monetary system, producer and consumer must coordinate on trading a token for help. Second, producers should not help consumers with balance L, but the incentives to do so may differ across treatments. They are strong under monetary trade because a producer's balance cannot increase by helping a consumer without tokens. This is not so in Memory: helping always increases the producer's balance. Hence, producers may be tempted to help someone with balance L, when in fact they should punish. That is to say, trade is a best response in and off-equilibrium in Money—due to the presence of liquidity constraints—but it may not be a best response off-equilibrium in Memory because consumers are liquidity unconstrained. In section 5.3 we will develop a theoretical model that incorporates these considerations to shed light on some of the empirical findings that are listed in the following section.

5 Results

We report a series of results that address the following questions: if defections are public, is full cooperation easy to sustain? When monetary trade is possible, do subjects attain different cooperation rates than when it is not? Does a system designed to maintain and share information about past conduct supersede the function performed by money?

All analyses consider only the first twenty periods in each supergame and take as unit of observation, unless otherwise noted, the average choice of each subject in a supergame. Results 1-5 and 7 rely on sessions run with inexperienced subjects, while Result 6 explicitly addresses the issue of experience.

Result 1. *In the long-run, cooperation and efficiency were greater in* Money *and* Memory *compared to* Baseline.

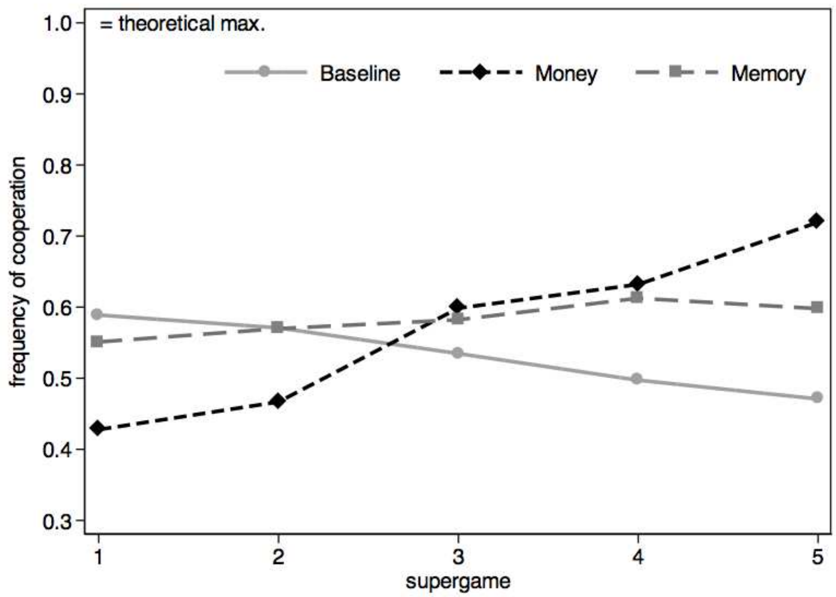

Figure 1: Relative frequency of cooperation by treatment

Support for Result 1 is provided by Figure 1 and Tables 4-5. By design, aggregate efficiency is proportional to average cooperation. The minimum and maximum cooperation rates observed in a supergame are 10% and 85% in Baseline, 23% and 95% in Money, 19% and 100% in Memory (inexperienced subjects only). Average cooperation rates in Baseline range from 59% in the first supergame to 47% in the last supergame. This declining trend is statistically significant at the 5% level as illustrated by the panel regression in Table 4, model 1. The dependent variable is the average cooperation frequency of a subject in a supergame; the regression controls for individual characteristics.

Dependent variable: *Individual rate of cooperation*	Model 1 (Baseline) Estimate	S.E.	Model 2 (All treatments) Estimate	S.E.
Supergame	-0.031**	0.016	-0.031**	0.013
Money			-0.266***	0.069
Money × Supergame			0.106***	0.018
Memory			-0.102	0.063
Memory × Supergame			0.045***	0.016
Constant	0.308**	0.127	0.438***	0.103
Controls	Yes		Yes	
N. of obs. (N. of subjects)	360 (72)		1200 (240)	
R-squared within	0.064		0.126	
R-squared between	0.259		0.212	
R-squared overall	0.179		0.172	

Table 4: Cooperation rate.

Notes: One obs. per subject per supergame. Inexperienced sessions, all supergames. Panel regression with random effects at the individual level and robust standard errors (S.E.) adjusted for clustering at the session level. The estimated coefficients for *Money* and *Memory* are significantly different (p- value$<$ 0.001). The estimated coefficients for *Money* × *Supergame* and *Memory* × *Supergame* are significantly different (p- value$<$ 0.001). The sum of the coefficients *Supergame* and *Memory* × *Supergame* is weakly significant (p-value = 0.099). The sum of the coefficients *Supergame* and *Money* × *Supergame* is significant (p-value $<$ 0.001). In this and all subsequent regressions, *Controls* include the following individual characteristics: gender, major, two measures of understanding of the instructions (response time and number of wrong answers in the quiz) and session location (Purdue, Chapman).

Now consider Money and Memory. In the last supergame, average cooperation are 72% and 60%, respectively. The differences in cooperation with Baseline are statistically significant according to a linear regression (1% and 5% level, respectively; Table 5). Contrary to Baseline, in Money there is a significant positive trend with experience (Table 4, model 2). The Memory treatment exhibits a weaker positive trend (significant at a 10% level).

One may be tempted to chalk up Result 1 as an artefact of subjects being in the habit of relying on record keeping and monetary exchange in everyday life. Yet, two observations suggest this result has a deeper connotation. First, neither Money nor Memory expand the efficiency frontier—the efficient outcome is attainable in Baseline—nor constrains subjects to adopt a trade strategy, nor precludes cooperation through a social norm. In fact, adding tokens and balances expands action set, strategy sets, and the equilibrium set relative to Baseline. Hence, if anything, the enriched stage games in Money and Memory should increase coordination difficulties, not reduce them (Capra et al., 2009; Weber, 2006).[13]

Dependent variable: *Individual frequency of cooperation*	Supergame 1		Supergame 5	
	Estimate	S.E.	Estimate	S.E.
Money	-0.151*	0.069	0.307***	0.078
Memory	-0.059	0.065	0.132**	0.047
Constant	0.342*	0.155	0.456***	0.069
Controls	Yes		Yes	
N	240		240	
R-squared	0.212		0.207	

Table 5: Treatment Effects on Cooperation in Supergames 1 & 5.

Notes: One observation per subject. Inexperienced sessions. Robust standard errors adjusted for clustering at the session level. In supergame 1 the estimated coefficients for *Money* and *Memory* are significantly different (p-value: 0.002). In supergame 5 the estimated coefficients for *Money* and *Memory* are significantly different (p-value: 0.014).

Second, Money and Memory supported lower overall cooperation than Baseline in supergame 1 (Table 5). This inferior short-run performance suggests that pre-existing "monetary habits" are not the primary reason for the experimental results. In fact, it suggests that subjects *developed* a monetary trade convention over the course of the session, with the intent to coordinate on a cooperative outcome.

Result 2. *In the long-run* Money *supported higher cooperation and efficiency than* Memory*.*

Figure 1 and Tables 4-5 provide support. In the last supergame the average cooperation level in Money is significantly different from Memory (Table 5).

[13]The result is not an artifact of having anonymous public monitoring. In an experiment with a similar design but private monitoring, Camera and Casari (2014) report that monetary trade emerges and promotes cooperation.

In the experiment, tokens became a fiat money in a manner consistent with monetary equilibrium. Tokens were by design intrinsically worthless, and—unlike the transfer of balances in Memory—their exchange was not forced. This finding about the endogenous emergence of monetary systems is in line with earlier studies (Camera and Casari, 2014; Camera et al., 2013). An original feature of this study is that tokens could be used in a way *opposite* to monetary trade. Consumers could transfer a token only to a producer who *refused* to help (Section 3), but this behavior was not observed. When token exchange was feasible, many producers offered help only in exchange for a token (63%) and consumers offered a token only in exchange for help (82%). When consumers had no tokens, producers refused to help 72% of the times.

One cannot exclude that the behavior in Memory is consistent with some inefficient equilibrium being played. Indeed, the design admits multiple equilibria, with frequencies of cooperation ranging from 0 to 100 percent. What we do observe is that efficiency is lower in Memory than in Money even if subjects could adopt the trade strategy – which supports efficient play – equally easily in both treatments. A possible explanation for Result 2 is that subjects did not exploit record-keeping to replicate a monetary trade pattern.

Result 3. Money *supported trade but* Memory *did not.*

Support for Result 3 comes from Figures 2-3 and Table 6. Full cooperation in Money and Memory can be achieved through the trade strategy. This means that after any history, in either treatment producers with low balances (=L) help only consumers with high balances (=H). If everyone adopts this strategy, subjects alternate between giving and receiving help, so would have 0 balances as producers and 1 as consumers (Figure 2, left panel).

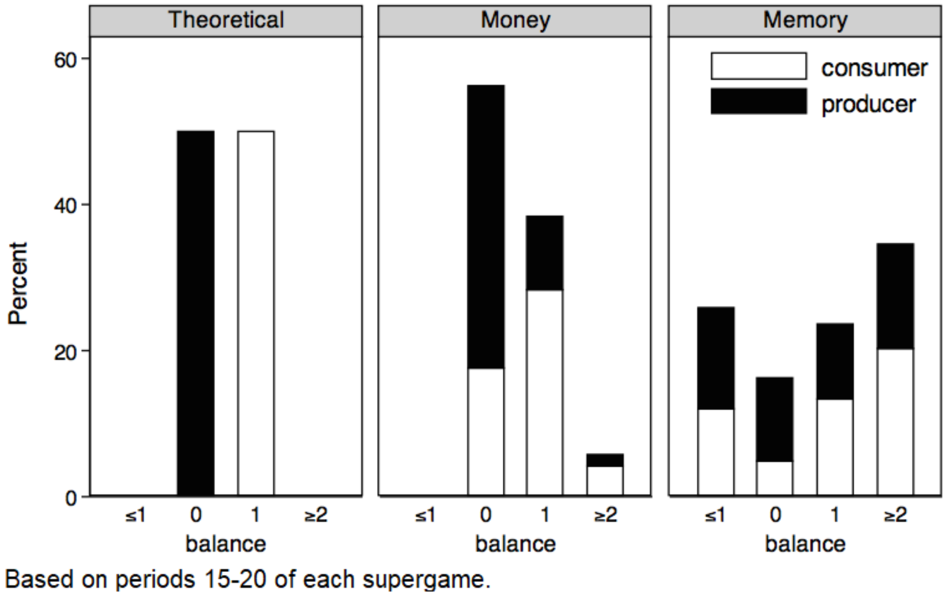

Figure 2: Distribution of balances in Money and Memory

In the experiment, the distribution of balances approximates the 50/50 theoretical distribution only in Money, where about 56% of subjects hold 0 tokens and 38% hold 1 token (Figure 2, center panel). This distributional pattern completely breaks down in Memory, where only 16% of subjects have a 0 balance and 23% have balances 1 (Figure 2, right panel). This is direct evidence that subjects did not adopt a trade strategy, nor a strategy compatible with efficient play.[14]

To study deviations from the trade strategy we look at the empirical distribution of balances. In equilibrium, trade requires help to be given in every encounter. Instead, off-equilibrium help should be given only when the producer has balance L and the consumer has H; otherwise, help should not be given, especially if the consumer has balance L.

[14]By design, efficient play is identical in Baseline and in Memory, as the action sets in these treatments are identical. These two treatments differ only in the existence of balances. But balances in Memory change automatically, so in the efficient equilibrium all producers have 0 balances, and all consumers 1 balances, exactly as in Money if they exchanged balances back and forth.

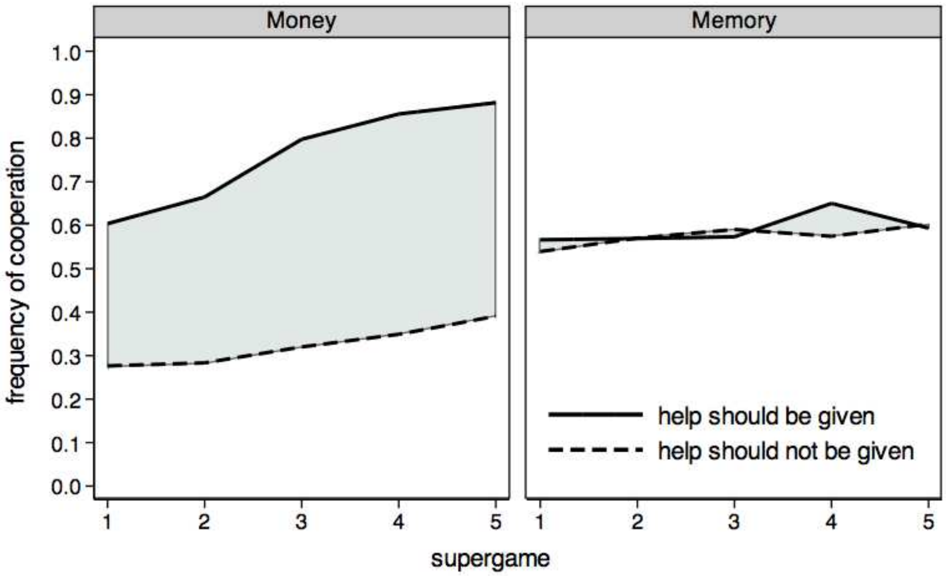

Figure 3: Trade emerges in Money but not in Memory.

Figure 3 shows the empirical frequency of cooperation when help should and should not be given under the trade strategy (solid vs. dashed line). Theoretically, the solid line should be at 100%, and the dashed line at 0%, if everyone followed the trade strategy. In Money, the aggregate cooperation frequency is consistent with the widespread adoption of the trade strategy: the distance between the solid vs. dashed line in Figure 3 (left panel) amounts to 49 percentage points in the last supergame. By contrast, in Memory the two lines tend to overlap even in the last supergame (Figure 3, right panel).

The data reveal that subjects do not use record-keeping in the same manner they use tokens. Producers do help more frequently consumers with balance H rather than L. However, in Memory producers do not condition their help on their own balance as they should, if they followed a trade strategy, while in Money they do, helping more frequently if *their* balance is L rather than H.

Table 6 reports the marginal effects of balances in a pair on the probability of observing cooperation in the pair, in the two treatments. If subjects adopt the trade strategy in each treatment, then the probability of observing cooperation should be higher in *trade meetings*—where the producer's balance is L and the consumer's is H—than in all other meetings. The data from the Money treatment are in line with this prediction: help is given with a significantly higher frequency in trade meetings than in all others (Table 6, p-value < 0.001 for all comparisons). However, this is not so in Memory, where the estimated marginal effect of being in a trade meeting is significantly smaller than the estimated marginal effect when both producer and consumer have balance H (p-value $= 0.003$). In addition, the estimated marginal effect of being in a trade meeting is much smaller in Memory than in Money (p-value < 0.001 in a regression with pooled data from both treatments). This is evidence that the trade strategy is used in Money but not in Memory.

Dependent variable: *Cooperation outcome in a pair*	Money		Memory	
	Estimate	S. E.	Estimate	S. E.
Supergame	0.099***	0.016	0.138***	0.029
Period	-0.002***	0.001	-0.005***	0.002
Balance: Producer, Consumer				
L, H	0.440***	0.035	0.095***	0.016
H, L	-0.118**	0.055	-0.006	0.032
H, H	0.203***	0.063	0.191***	0.024
Controls	Yes		Yes	
N. of obs. (N. of subjects)	3600 (72)		4800 (96)	

Table 6: How balances in a meeting affect cooperation.

Notes: One observation per subject per period. Inexperienced sessions. Marginal effects from a logit regression. Robust standard errors adjusted for clustering at the session level.

Result 3 is especially remarkable given that trade in Memory requires less coordination than in Money, where consumer and producer must mutually agree to a trade. This is not so in Memory, as only the producer takes an action, while balances are automatically updated. So, why did trade emerge in Money and not in Memory?

Result 4. Money *removed the incentives to free ride but* Memory *did not.*

Support for Result 4 comes from Figure 4, which shows that the introduction of Money and Memory altered the distribution of earnings because it redistributed surplus from frequent defectors to frequent cooperators.

In each supergame, we classified subjects into five categories according to their frequency of cooperation as producers (horizontal axis) and computed the associated average earnings across all periods, regardless of their role, consumer or producer (vertical axis). The share of observations in each bin is reported by the corresponding marker. In Baseline, about 39% of subjects are frequent cooperators and 28% are frequent defectors; those who earned the most on average are the frequent defectors (circles).

Introducing the record keeping technology lowered the incentives to defect relative to cooperation. The association between income and cooperation remains U-shaped and frequent defectors still earn the most (squares). By contrast, the use of tokens as money generates a dramatic shift in incentives: average individual earnings and cooperation frequency exhibit a positive, monotone association (diamonds). Frequent defectors are now the category that earned the least, and account for only 8% of the subject population. This result confirms the findings reported in Camera and Casari (2014). In short, a monetary system endogenously emerged in the Money treatment and the use of money removed the incentives to free ride. In contrast, in the Memory treatment subjects failed to remove incentives to free-ride, which is a likely reason why efficiency is lower in Memory than in Money.

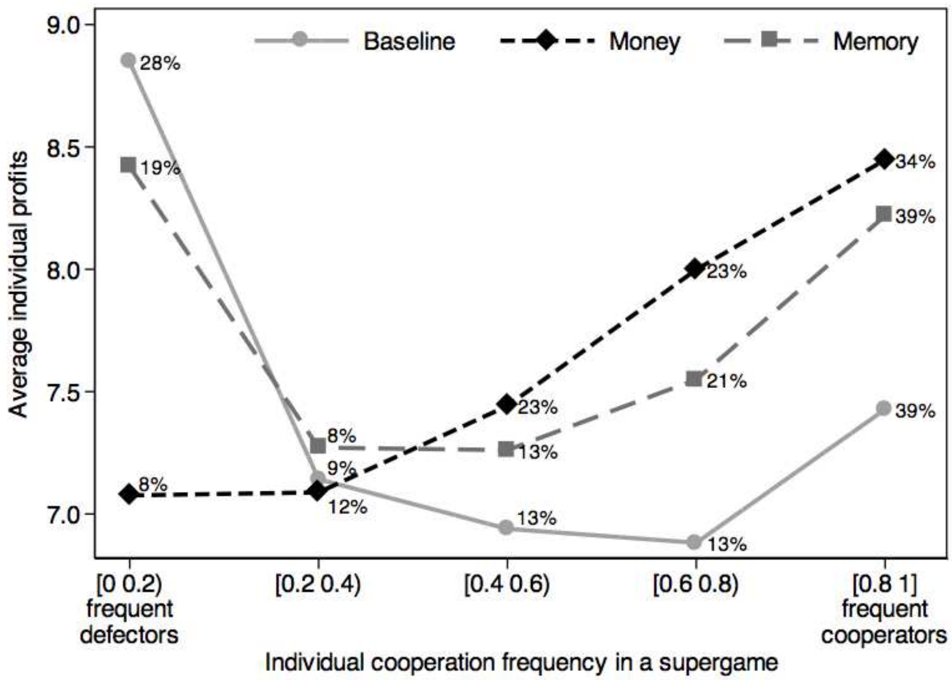

Figure 4: Cooperation frequency versus profits

5.1 Combining Money and Memory

To further check the robustness of our results, it is helpful to consider a situation in which both money and memory are simultaneously available.[15] In the Money+Memory treatment, subjects have access to two possible trading instruments: one is tokens – which are in scarce supply – and the other is numeric balances – as in the Memory treatment. Theoretically the use of memory should supersede the use of money, i.e. in this treatment tokens should not be used. Moreover, if a behavioral complementarity exists between the two institutions, then cooperation rates could be even higher relative to a situation where each institution is present in isolation.

Result 5. *In* Money+Memory *monetary trade emerged; cooperation and efficiency were lower than in* Money*.*

[15]We thank an anonymous reviewer for suggesting this additional treatment.

The data reveal that subjects coordinate on using tokens as a trading instrument, and do not exploit the additional information provided by the record-keeping system. When the consumer had tokens, help was exchanged for a token in 67% of the encounters, which is not far from the 71% observed in MONEY.

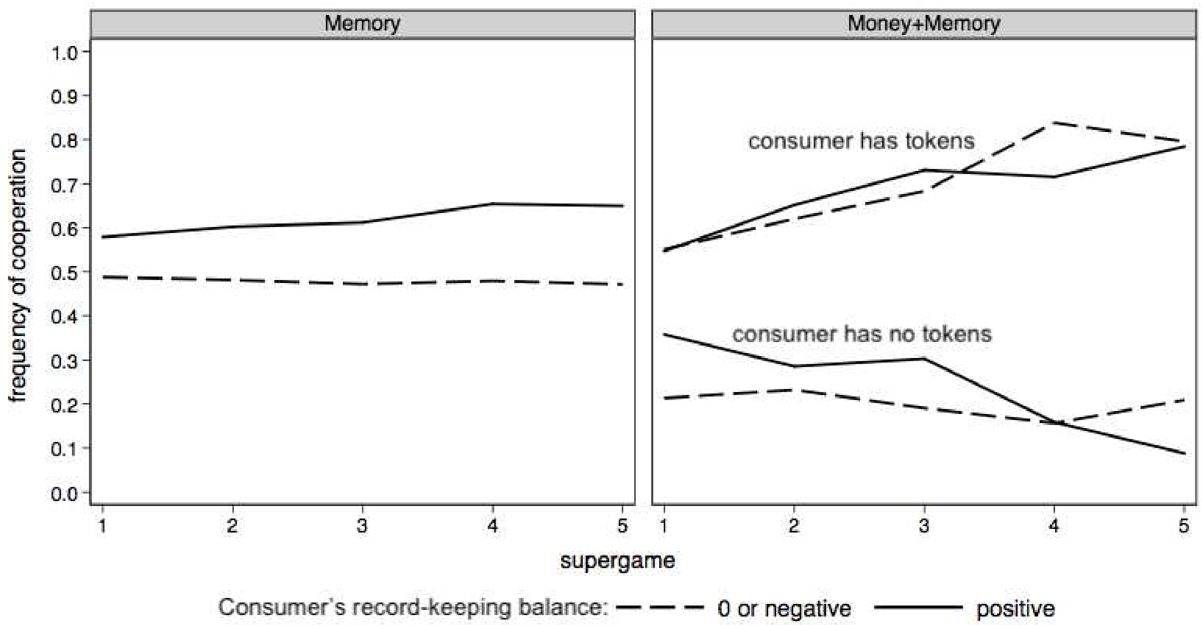

Figure 5: Cooperation when tokens and record keeping are both available.

Figure 5 reveal that producers did not take into account the consumer's record-keeping balance. In MONEY+MEMORY, when the consumer had no tokens, help was given in 20% of the encounters, which is even below the 26% observed in MONEY; again, the Figure confirms that the consumer's record did not play a role in the producer's decision to help. This is evidence that, given the choice between two institutions, money and memory, subjects coordinated on using money as a trading instrument.

Cooperation rates are systematically below the MONEY treatment, even if subjects had access to a richer information set about the actions of the counterparts. This difference is amplified, rather than reduced, as subjects gain experience across supergames. In cycle 5, the overall cooperation frequency was 62%, which is in between Memory (60%) and Money (72%). Figure B.5 and Table B.5 (in Appendix B) provide additional evidence. These results suggest that a coordination issue emerged when subjects faced the choice between two alternative institutions, and provide no support in favor of the hypothesis that these institutions can complement each other.

5.2 The effect of experience

Figure 1 suggests that experience with the task affects cooperation. The open question is whether Memory can outperform Money in cooperation frequency if subjects have gained enough experience. All of the results above are based on the behavior of subjects that had no previous experience with the game. Now, we present evidence based on sessions run with subjects who had participated in an earlier experiment under the same treatment.

Result 6. *Data from experienced subjects confirm and reinforce Results 1-5.*

By the last supergame of the experienced sessions, cooperation in Baseline fell to 28.8%, in Money rose to 94.6%, and in Memory reached 55.4%. These levels are significantly different one from another according to a probit regression (see Appendix B.1). This finding reinforces Results 1-2. Experience with the task also helps to firmly establish the use of the trade strategy in Money but not in Memory, which strengthens the finding for inexperienced subjects (Result 3). Consider the distance between the solid and dashed lines in a graph made with data from experienced sessions and similar to Figure 3; by the last supergame, we have a distance of 85 percentage points in Money and of 8 points in Memory (see Appendix B.1). Experience also wiped out free riding behavior in Money, in line with

Result 4 for inexperienced subjects. About 89% of subjects were frequent cooperators and no one cooperate in less than 40% of the encounters.

In Money+Memory, cooperation rates are in between those in Money and in Memory (85.8% in supergame 5). The difference with Memory is statistically significant at the 1% level, while the difference with Money is not (see Table B.4 in Appendix). This confirms Result 5.

5.3 How liquidity constraints affect incentives

By design, the information available in Memory allows to replicate the trade strategy in Money without having to exchange tokens. Empirically, we observe that subjects did not use that strategy. How can we explain this result? In Money the general use of the trading strategy removes the incentive to free-ride because a producer who exits a meeting without a token necessarily becomes a "liquidity constrained" consumer in the next round. Hence, refusing to help is unprofitable because producers do not help liquidity constrained consumers (see Figure 4, Money treatment). Removing liquidity constraints, as done in Memory, raises the incentive to free-ride relative to Money because free-riders can always "pay" by accumulating negative balances.

We provide a simple theoretical framework to formalize this idea. Consider the indefinitely repeated helping game when there are two types of players, *rational* and *behavioral*, in proportion $1 - p$ and p, respectively. At the beginning of the game, rational players choose between two strategies—free-riding and monetary trade—selecting the one with the highest long-run payoff. A free rider never helps, while a monetary trader helps only if her balance is L and the consumer's is H. Behavioral players, instead, follow a fixed rule of thumb, which does not necessarily maximize an objective function. As consumers, they offer a token in exchange for help, whenever possible. As producers, they behave either as *altruists*, who cooperate unconditionally because of pro-social preferences, or as *quid-pro-quo* players, who cooperate only if doing so raises their balances. Unlike rational traders, quid-pro-quo players help any consumer who can "pay" with a unit of balances—even those with balance L.

The presence of behavioral players raises the incentive to free-ride. The free-riding payoff is below the payoff from choosing the trading strategy if

$$\beta \geq \frac{d}{(1-p)(g-d+l)} \qquad \Rightarrow \qquad \beta \geq \frac{\beta^*}{1-p}.$$

It follows that the larger is the share of behavioral types p, the higher is the critical β needed to support a trading equilibrium. With our parameters, the above inequality holds only if behavioral players represent less than half of the population. The formal proof is in Appendix 5. Notice that in MONEY the share p includes only altruists because quid-pro-quo players never help consumers with balance L (they have nothing to offer). This is the reason why the incentives to trade are higher in MONEY than in MEMORY.

The empirical implication of this theory is that removing liquidity constraints in MONEY should impair cooperation and reduce trade, similarly to what happens in MEMORY. To test this conjecture, we designed the new treatment MONEY UNCONSTRAINED, where a consumer who wants to trade but has no token, can freely create one. Hence, trade is always feasible and balances can go negative as in MEMORY.

Result 7. *In* MONEY UNCONSTRAINED *monetary trade did not emerge; cooperation and efficiency were lower than in* MEMORY*.*

Figures B.5, B.6 and Table B.5 (in Appendix B) offer evidence. In MONEY UNCONstrained, tokens are exchanged in 59% of encounters, but these exchanges do not map into the monetary trade strategy. We find a weak tendency to help more frequently consumers who have tokens, but we also see that producers *do not* condition help on their own balance—in contrast with monetary trade. This behavior is in line with what we observed in Memory (see Table B.6, Appendix B), but contrasts with what we see in Money, where producers without tokens cooperated more frequently than those with tokens (Table 6).

To refine this aggregate view, we now estimate the share of subjects who fit the description of behavioral types. For each subject we ran a one-sided binomial test considering the choices as producer in the entire session. The null hypothesis is that the probability to help a consumer with balance L is ≥ 0.75. We classify the subject as behavioral if we cannot reject the null with $\alpha = 0.05$. The unit of observation is a subject in a session, i.e., we assume that the type is constant across cycles. This estimate relies on the assumption that each encounter between the subject and a consumer L is an independent trial.

We first consider Money, where only altruists help consumers with balance L. Here, 22% of subjects fit this description. We then look at the Memory and Money Unconstrained treatments, to estimate the share of behavioral types, which we expect to be higher as it now includes also the quid-pro-quo players. In Money Unconstrained 33% of subjects are classified as behavioral, and this share increases to 45% in Memory. In line with our simple behavioral model, the incentives to trade as opposed to free riding are higher in Money, where quid-pro-quo types do not help free-riders.

6 Conclusion

We have offered an empirical test of a long-standing theoretical assertion: money is just an imperfect substitute for knowledge of past conduct (Kocherlakota, 1998; Ostroy, 1973; Ostroy and Starr, 1990; Townsend, 1987). This assertion is typically taken to imply that there should be no benefit from using money in societies where past actions are made public. In an experiment, we compared the role performed by money as opposed to an institution that reveals past conduct in the group (memory). Our setup is designed to test the principle of operation behind the theoretical assertion that money has no role to play when past conduct is observable, and not to test a specific monetary model. This is done by constructing laboratory economies where payoff maximization requires the exchange of intertemporal gifts, but participants can neither rely on reciprocity nor relational contracting. The methodological advantage of experimental over field evidence lies in the ability to decouple institutions that are often intertwined (e.g., money and credit). By selectively adding and removing an institution we can establish causal links between that institution and economic performance.

The experimental evidence provided reinforces the long-held view that monetary systems are key to support impersonal exchange, intertemporal trade and, consequently, large-scale cooperation. At the same time, our study highlights original aspects of the institution of money that have been ignored or poorly understood. It suggests that well-functioning monetary systems are not simply rudimentary arrangements for monitoring past conduct, but play a role that is richer than previously thought. The study demonstrates that the use of money imposes a punishment discipline among strangers, which is unattainable otherwise.

Our study makes three contributions to the monetary literature. First, previous research has documented that monetary systems affect behavior and outcomes in repeated social dilemmas (Camera and Casari, 2014; Camera et al., 2013). However, no study had yet provided an empirical test of the theoretical assertion that the role of money in a society is simply to reveal past conduct (Kocherlakota, 1998). To test the operating principle behind this assertion, we constructed economies in which strangers—who by design cannot engage in relational contracting—can derive significant benefits from cooperating over the long haul. We find that the suggested theoretical parallel between money and memory does not empirically translate into a functional equality. Remarkable differences emerge between the Money and Memory treatments in terms of long-run efficiency, strategies employed, and distribution of earnings. This demonstrates that the behavior observed when a monetary system is in place is not comparable to the one observed when the same conduct can be sustained by relying on a public memory. Our test represents a unique contribution to monetary economics and also to the experimental literature on money. Of course, one could imagine other forms of record keeping, which could be tested to address different research questions. The form we adopted captures the essential features of the "memory" discussed in the theory of money: it must allow a replication of monetary trade without exchanging physical objects, it must allow public observability of deviations form efficient play, while not revealing the past conduct of any future counterpart.

Second, we offer a theoretical rationale for our findings: monetary systems affect the structure of incentives more effectively than memory-based systems. The Baseline treatment shows that groups of strangers struggle to coordinate on collective punishment schemes possibly due to heterogeneity in beliefs or preferences, coordination frictions, cognitive or emotional factors. The addition of a record-keeping system in Memory solves these problems only in part, by providing individual-specific information on past conduct (Result 1). Subjects *can* identify individuals as being free-riders based on their balances—unlike in Baseline—but still do not consistently sanction them (Result 4). The exchange of tokens in Money overcomes this shortcoming (Result 3) because punishment is directly built into the monetary trading system: liquidity constraints make monetary trade self-enforcing. A producer has an incentive to help for a token—to have a means of payment in the future—and has nothing to gain from helping free-riders, who have nothing to offer in exchange. Support for this interpretation comes from comparing Money to Money Unconstrained, where consumers can always "pay" by creating one token if they have none. Here, cooperation would be self-enforcing if no one helped consumers without tokens, but there is a weak incentive to do so because producers who help always "get paid." This lack of punishment generates a negative externality for the entire group, magnifying the incentive to free ride, thus displacing cooperation (Result 7). As a result—although illiquidity is typically a source of inefficiency—relaxing liquidity constraints in the experiment (as we do in Memory and Money Unconstrained) lowered long-run efficiency. In a way, liquidity constraints impose a much-needed discipline on sanctioning free riders, which more effectively aligns incentives with cooperative behavior.

A third contribution is to provide a robustness check for previous experiments about social norms and the endogenous emergence of monetary systems (Camerer et al., 2016). This paper replicates findings about the Baseline and Money treatments from earlier experiments with a similar design, but different conditions in terms of length of interaction, subject pool, payoffs asymmetry, role alternation, and instructions.

These findings sharpen our understanding of how different types of legal, economic, and social institutions affect the structure of incentives for cooperation in societies of strangers. The simple and theoretically appealing record-keeping system we introduced turned out to be empirically unsuitable to sustain efficient play. By contrast, out of some barren and theoretically inessential tokens, a rudimentary monetary system emerged that greatly boosted cooperation and efficiency.

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