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Is Money Essential? An Experimental Approach

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Abstract

Monetary exchange is called *essential* when better outcomes are incentive feasible with money than without it. We study essentiality theoretically, and experimentally, using finite-horizon monetary models that are naturally suited to the lab. Following mechanism design, we also study the effects of strategy recommendations, both when they are incentive compatible and when they are not. Results show output and welfare are significantly enhanced by fiat currency when monetary equilibrium exists. Also, recommendations help if incentive compatible, but not much otherwise. Sometimes money gets used when it should not, and we investigate why, using surveys and measures of social preferences.

Keywords: Money, mechanism design, experimental economics

JEL Classification numbers: D92, E4, E5

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1 Introduction

A central issue in economics is to understand what makes monetary exchange a socially useful institution. Based on Hahn (1973, 1987), money is said to be *essential* if more desirable outcomes are incentive feasible with money than without it. This is particularly relevant for fiat currency, an object that may have value even though it is *intrinsically useless* (Wallace 1980). While it has no such role in traditional general equilibrium theory, there are by now various formalizations, surveyed in Lagos et al. (2017) and Nosal and Rocheteau (2017), where frictions make fiat money essential. It is commonly understood that three ingredients are needed for essentiality: a double coincidence problem; limited commitment; and imperfect information.

To explain this, a double coincidence problem means there are gains from trade that cannot be exhausted by pure barter. In the spirit of Jevons (1875), suppose you are in a world where agents specialize in production and consumption, meet bilaterally at random, and engage in quid pro quo exchange. It may be rare (a coincidence) to meet someone who produces what you like, and very rare (a double coincidence) to meet someone who produces what you like and likes what you produce. A venerable notion is that money is useful because it permits trade in single coincidence meetings. Yet this is not sufficient for essentiality, as ex ante payoffs are typically higher if everyone simply produces whenever asked. If there is commitment, agents would agree to this, and efficient outcomes can be sustained without money.

Without commitment, however, agents may be tempted to renege when asked to produce, rendering the commitment solution inconsistent with dynamic incentives. Yet that is still not enough for essentiality if trading histories are observable, since desirable outcomes can often be supported without money, akin to cooperative equilibria in repeated games: agents who do not produce when asked are punished by having others not produce for them in the future. This can be interpreted as a credit arrangement, with punishments involving the denial of future credit to those who fail to honor obligations, as in the literature following Kehoe and Levine (1993).

As Kocherlakota (1998), Araujo (2004), Aliprantis et al. (2007) and others emphasize, such punishments must be precluded for money to be essential. Conventional wisdom is this: if it is incentive feasible to implement monetary exchange, and trading histories are publicly observed, the credit arrangement described above is also feasible, and it is at least as good if not better in terms of welfare. This suggests that essentiality requires information frictions, and while there are different ways to capture these (e.g., see Gu et al., 2016), a common thread is that it must be hard to monitor, communicate or keep records of what happens in pairwise meetings – sometimes described as imperfect memory.

In this context, Wallace (2001, 2010) refers to the view that essentiality is salient as the *mechanism design approach* to monetary economics, and argues that mechanism design methods are attractive because they provide a clear distinction between the environment and the rules of the game mapping actions into outcomes, so, given a set of feasible mechanisms, it is possible to decide whether money is essential.[1] What may not have been anticipated is that this leads to models of monetary exchange that are in some ways ideally suited to experimental economics, because they are tractable enough that their theoretical properties are well understood and subjects in the lab should be able to comprehend the details, yet the outcomes are not obvious because there are multiple equilibria due to the self-referential nature of liquidity (what you accept in payment depends on what others accept).

There has by now emerged a significant body of experimental monetary economics.[2] However, previous papers do not address our main issue, which is to ask, from a

[1]As Wallace (2010) puts it, "The mechanism-design approach to monetary theory is the search for fruitful settings or environments in which something that resembles monetary trade actually accomplishes something – or, in Hahn's (1973) terminology, settings in which money is essential." For those interested in history of thought, Hahn actually focused on the essentiality of a *sequence economy* where the sequence of trades may not lead to Arrow-Debreu outcomes. If the sequence is inessential, money might be a way of registering transactions, but nothing important is lost by focusing on Arrow-Debreu. To properly study monetary economics, therefore, one should analyze economies where the trading sequence is essential – which is certainly true in what follows.

[2]Brown (1996), Duffy and Ochs (1999, 2002) and Duffy (2001) experiment with Kiyotaki and Wright (1989); Jiang and Zhang (2018) use Matsuyama et al. (1993); Rietz (2019) uses Curtis and Waller (2000); Camera and Casari (2014) use something like Kiyotaki and Wright (1993); Duffy and Puzzello (2014*a*,*b*; 2022) and Ding and Puzzello (2020) use Lagos and Wright (2005) or its extension by Zhang (2014). Marimon and Sunder (1993), Marimon et al. (1993) and Arifovic (1996) use OLG (overlapping generations) models.

mechanism design perspective, if money helps achieve higher welfare in theory and in the lab *for the same reasons*. To this end, we work with finite-horizon monetary models that nicely suit experimental economics, because in the lab games must end at some finite time T (one is simply not allowed to keep subjects for more than a few hours). Then two environments are considered that are identical in all aspects, except that agents may or may not know where they are in the sequence of trading opportunities: in one, monetary exchange is an equilibrium outcome, even with a finite horizon, and is superior to the best outcome without money; in the other, there is no monetary equilibrium. Hence a small change in the specification takes us from a case where money is essential to one where it is not.

Intuitively, when monetary exchange is an equilibrium outcome, subjects give up something of value for cash because they rationally put positive probability on being able to exchange the case later for something they value more. In contrast, in environments where trade ends with probability 1 at $T < \infty$, without uncertainty over where agents are in the trading sequence, accepting fiat money cannot be an equilibrium: assuming they understand the game, no one should sacrifice anything at T to get money, so no one at $T - 1$ should sacrifice anything to get it, and by backward induction fiat currency should never be valued. So in standard models with $T < \infty$, if subjects accept money in the lab, we cannot be sure why, but it cannot be because they rationally expect to spend it later with positive probability.

Experimentalists address this in various ways. Often random termination times are used, where the game ends with some probability after each round. This is meant to generate discounting, as assumed in infinite-horizon models, but does nothing to avoid the backward induction argument if there is still a hard stop at $T < \infty$.[3] Another idea for implementing infinite-horizon monetary theory in the lab is to assign

[3]This issue goes well beyond monetary economics and concerns experiments with dynamic games more generally. Consider Selten et al. (1997): "Infinite supergames cannot be played in the laboratory. Attempts to approximate the strategic situation of an infinite game by the device of a supposedly fixed stopping probability are unsatisfactory since play cannot continue beyond the maximum time available." See Cooper and Kuhn (2014), Fréchette and Yuksel (2017) and Jiang et al. (2021) for more on this. To be clear, our claim is *not* that taking infinite-horizon models to the lab is without value; we simply want to consider an alternative.

value to cash held at T based on what payoffs would be if the game were to continue (Marimon and Sunder 1993; Arifovic 1996; Jiang et al. 2021). This is interesting, but treads close to giving up on the fiat nature of fiat currency. Here, in equilibrium, genuine fiat objects can be used as media of exchange despite $T < \infty$, and agents accept them because they rationally expect to spend them later.

Our work follows up on Davis et al. (2022).[4] However, there is much here that is new. While Davis et al. (2022) also experiment with finite-horizon models, they do not take a mechanism design approach, nor do they consider strategy recommendations or try to explain behavior using exit surveys and measures of social preferences. Moreover, the experimental designs differ in important ways. Details are given below, but the idea is that our design is meant to reduce repeated-game effects – i.e., subjects believing that their current actions will affect the actions of others in future play – that may have plagued Davis et al. (2022). We conjecture that this can explain the results referred to as "puzzling" in that paper, and test this explicitly in Section 5.

One clean aspect of what follows is our focus on a controlled experiment comparing two environments with money, where in one there is a monetary equilibrium and in the other there is not, as opposed to the usual practice of comparing one environment with and without money (although we do that, too). Also, we go beyond previous work by considering strategy recommendations – e.g., "always produce for money" – as a device to deal with coordination problems endemic to monetary economies. The idea, related to Myerson (1986), is that mediation can help coordination, although, importantly, agents may ignore the mediator. The use of such suggestions is not only consistent with mechanism design, but also with a standard interpretation of equilibria going back to Nash (1950): give agents a strategy profile and see if they deviate. While it is rare in experimental economics to consider suggestions, they are appropriate for the issues at hand, and in any event we want to know if they serve

[4]The theory in that paper is related to Kovenock and de Vries (2002), which is itself related to the analysis of bubbles in Allen et al. (1993), Allen and Gorton (1993), Moinas and Pouget (2020) or Awaya et al. (2022). These papers are all ultimately connected to Samuelson's (1987) discussion of how a lack of common knowledge about T ameliorates end-game effects.

mainly as a coordination device, or subjects just follow them blindly.[5]

To preview the model, consider 3 agents, and 2 rounds of bilateral meetings, for simplicity. Now suppose that sometimes agents being offered money do not know if it is the first or second round. Accepting money in the second round is rational if an agent puts high enough probability on it being the first round. So monetary exchange can be an equilibrium even if all players know the horizon is $T = 2$, and it yields higher ex ante payoffs than the best nonmonetary outcome. That means money is essential.

Yet questions arise. Do agents always use money when a monetary equilibrium exists? No, according to theory, since there always coexists a nonmonetary equilibrium. Might agents accept money when there is no monetary equilibrium? No, according to theory, but in past experiments they sometimes do, and we want to understand why – is it due to mistakes, social preferences (agents caring about others) or something else? This is addressed using exit surveys and measures of social preferences extracted from auxiliary experiments that we correlate with subjects' behavior.

To summarize: (i) We compare environments with and without money. (ii) In environments with money we compare specifications where monetary exchange is an equilibrium and where it is not. (iii) We compare cases with and without recommendations, both when following them is incentive compatible and when it is not. (iv) We use theory that allows valued fiat currency with a finite horizon. (v) We make experimental design choices different from related studies. (vi) We use surveys and measures of social preferences to gain insights into anomalous behavior. (vii) We focus squarely on essentiality.[6]

The results are largely consistent with theory. Payoffs are significantly higher when money is introduced if monetary equilibrium exists; otherwise money may be used

[5] Only a few other experimental papers have tried suggestions – e.g., Duffy and Feltovich (2010); Van Huyck et al. (1992); Cason and Sharma (2007) – and these do not study monetary models. Recommending that agents "produce for money" has not been tried.

[6] Essentiality is discussed by Camera and Casari (2014) and Duffy and Puzzello (2014*a,b*), but there money is not essential; optimal outcomes can be implemented with credit.

initially, but the impact quickly decreases as subjects seem to learn that accepting it lowers their payoffs. Recommendations help if following them is incentive compatible; otherwise subjects tend to ignore them. When theory says no one should accept money, some subjects still do. Our measures of social preferences, perhaps surprisingly, do not correlate with this, but exit surveys suggest social preferences do play a role. While some subjects make mistakes, others are quite sophisticated, trying to infer which round it is based on the time it takes for meetings to occur, which led us to generalize the theory to allow such inferences. Finally, we show our changes in experimental design from Davis et al. (2022) seem to help avoid anomalous outcomes: if we use their design, their "puzzling" outcomes reappear.

2 Theory

There are two environments, Model M and Model N, that are identical except for their information structures. The labels M and N indicate that the former model has a monetary equilibrium while the latter does not. A common feature is that there are 3 agents and 2 sequential, pairwise meetings; and in each meeting one agent is a producer while the other is a consumer of an indivisible good. This can be considered a truncation of a standard random-matching model or an OLG model. When those models include fiat currency they assume the horizon is $T = \infty$. We can do that, too, but need not, since fiat currency can be valued here with $T < \infty$.[7]

Nature determines the roles of players randomly. First there is meeting 1, where one agent is a consumer called Player 1, while the other is a producer called Player 2. Player 1 may or may not be endowed with money (an indivisible, intrinsically useless token). In this meeting, possible actions for the consumer include asking for the good for free, and, if endowed with money, offering it in exchange for the good, or offering it for free. The producer can then accept or reject these requests or offers. Next there is meeting 2, where possible actions are the same, although now whether the

[7] The theory extends to any $T \leq \infty$, with or without random terminations at $t < T$, but we use $T = 2$ because it should minimize the chance subjects irrationally regard big T as "approximately" ∞, and because it helps make the game easy to learn in the lab.

consumer has money depends on what happened in meeting 1. Then the game ends. In each meeting, if a producer gives the good to a consumer the latter gets utility u while the former gets $-c$, a production (or opportunity) cost. Given $u > c > 0$, before nature determines types it is ex ante Pareto efficient for producers to produce in all meetings.

Where the models differ is that in Model M some agents do not know if they are in meeting 1 or 2, while in Model N the timing of meetings is common knowledge. Thus, in Model N, Player 3 in a meeting knows it is the last meeting, and so there only exists a nonmonetary, autarkic equilibrium for the following obvious reason: it is irrational for Player 3 to bear cost c unless Player 2 gives something of value in exchange, and all that can potentially be offered is money, which is worthless since the game ends after the second meeting. Given that money is not valued in the second meeting, it is not valued in the first meeting, so the unique equilibrium entails no trade, the same as the equilibrium without money.

In Model M, when matched in the second meeting, the producer does not know it is the second meeting. Without money the unique equilibrium is autarky; with money that is still an equilibrium, but there is also a monetary equilibrium with trade in both meetings if $u > 2c$. To confirm this, suppose you believe others will produce when offered money. Then the probability of getting to spend the money after receiving it is 1/2, equal to the probability of the meeting being the first rather than the second. Hence the expected payoff to producing for money is $\frac{1}{2}(-c+u) + \frac{1}{2}(-c) > 0$. Thus, monetary exchange is an equilibrium, and money is essential because without it expected payoffs are 0 for all agents. Now, the realized payoff to Player 3 is $-c$ upon getting stuck with money, but this is still desirable because ex ante payoffs are higher, or, amounting to the same thing, average payoffs are higher if the game is played multiple times. Money thus expands the strategy set in both Models M and N, but in Model M it also expands the set of equilibrium outcomes.

There is also a stationary, symmetric, mixed-strategy equilibrium, where everyone produces for money with probability $2c/u$, and a stationary, asymmetric, pure-strategy equilibrium, where a fraction of agents accept money while the rest do not (see Shevchenko and Wright 2004 for a discussion of equilibria with partial acceptability in a related model). One interesting feature of the mixed equilibrium is that monetary exchange is mechanically more likely in the first than in the second meeting, as the latter requires the former. We also mention there are nonstationary equilibria, and sunspot equilibria, where money is accepted in only some dates and states, but we mostly ignored these, for now, in the interest of space (see Marimon et al. 1993 for experiments with sunspots).

Notice that Model M turns into Model N if all actions become publicly observable, which can be considered perfect memory. There is no equilibrium other than autarky with perfect memory. Hence, we provide a counterexample to the generally accepted proposition that money is at best an imperfect substitute for memory (Awaya and Fukai 2017 also have a counterexample, but it is much more complicated). In many environments, as discussed by Kocherlakota (1998), Wallace (2010) and others, that proposition is valid. It impies anything one can do with money one can also do with memory, and often one can do better with memory – but here money strictly dominates memory. Indeed, it is incomplete knowledge of the timing that allows fiat currency to be valued, and that is what allows an improvement on autarky.[8]

While this baseline model serves our purposes nicely in the lab, there is an extension that is interesting for its own sake, and especially relevant in light of the experimental results discussed below. Although in theory Model M has players unable to distinguish between the first and second meetings, if the game proceeds in real time, inferences may be possible based on how long it takes to meet a potential trading partner. Since this sometimes happens in our experiments, we now show monetary equilibria still exist if waiting time is a *noisy* signal.

[8]A referee suggests that money here operates through "obfuscation." In Model M with money, if your trading partner has money, you don't know if you are Player 2 or 3, so you might produce. If they do not have money, you know you are Player 3 and won't (or shouldn't). What is important is that money provides some, but not complete, information. This is related to work on "opacity," e.g., Andolfatto et al. (2014) or Dang et al. (2017). In that context, suppose Players 2 and 3 do not know the timing, but a third party (maybe Player 1 or maybe someone else) does; ex ante Players 2 and 3 prefer that party not reveal the information.

There are different ways to formalize this, but suppose, for the sake of simplicity, that agents can distinguish between $\{t_E, t_M, t_L\}$, indicating early, middle and late in the game (this can be extended to richer sets of signals at a cost in terms of notation). Assume meeting 1 can occur at t_E or t_M and meeting 2 at t_M or t_L, generating a signal-extraction problem: agents cannot tell meeting 1 from 2 when $t = t_M$. The probability distribution over $\{t_E, t_M, t_L\}$ conditional on being in meeting 1 is

$$\Pr\left(t_E|\text{meeting 1}\right) = 1 - q,\ \Pr\left(t_M|\text{meeting 1}\right) = q,\ \Pr\left(t_L|\text{meeting 1}\right) = 0,$$

where q is an objective probability that is part of the environment. Similarly, the distribution conditional on being in meeting 2 is

$$\Pr\left(t_E|\text{meeting 2}\right) = 0,\ \Pr\left(t_M|\text{meeting 2}\right) = r,\ \Pr\left(t_L|\text{meeting 2}\right) = 1 - r.$$

If a meeting occurs early (late) the producer knows it is the first (second). The inference when being offered money at $t = t_M$ is more subtle, and the interpretation of getting a money offer depends on producers' acceptance strategy, because if players do not accept money then a money offer reveals it is meeting 1. If there is an equilibrium in which money is accepted for sure at $t \in \{t_E, t_M\}$, Bayes rule implies that the producer has posterior beliefs

$$\Pr\left(\text{meeting 1}\middle|\text{meeting 1}\middle|_{\text{meeting 1}|_{\text{meeting 1}|t_M} = \frac{q}{q+r}}\right.$$

when offered money at $t = t_M$. If it is meeting 2 the agent that just produced cannot trade money for goods, but in case it is meeting 1 there is a chance that the money can be used to get the good.

However, if the next producer can detect that it is meeting 2 there will be no exchange. Hence, conditional on signal t_M and being in the first meeting trade occurs in the second meeting if the next producer also receives signal t_M, which happens with probability r. The expected payoff from accepting at $t = t_M$ is thus

$$\frac{qr}{q+r}(u-c) + \left(1 - \frac{qr}{q+r}\right)(-c) = \frac{qr}{q+r}u - c.$$

Acceptance at t_E gives $ru - c$, so if players are best responding by accepting money at t_M they will optimally accept offers at t_E. Hence, there is a pure strategy equilibrium where players produce in exchange for money, except when they know it is the last meeting, provided that $qru/(q+r) \geq c$.

So monetary equilibria exist if the signal of waiting time is imprecise. Notice $q = r = 1$ is Model M and $q = r = 0$ is Model N, so the extension spans the two environments. Also, notice production rates will be higher in meeting 1 than 2 here, similar to what happens in the mixed strategy equilibrium, but now this is true even conditional on the consumer having money in meeting 2.

In what follows we sometimes consider recommendations, which may or may not be consistent with equilibrium play. In versions with money, these take the form:

> *A suggestion: Each player in a group may consider making the following choices: 1. Whenever you have the token, transfer it to the next player (if there is one). 2. Produce ONLY if you are offered the token. This is simply a suggestion. Feel free to follow it or not.*

In versions without money, they take the form:

> *A suggestion: If you are not Player 1, you may consider choosing to produce. This is simply a suggestion. Feel free to follow it or not.*

Notice: (i) in Model N, following the suggestion is not incentive compatible or Pareto superior; (ii) in Model M with money it is incentive compatible and Pareto superior; (iii) in Model M without money it is Pareto superior but not incentive compatible. This helps us disentangle if: (i) suggestions coordinate behavior; (ii) subjects do what we suggest even if it is not in their self interest; (iii) subjects act based on a desire to achieve better social payoffs.

Based on the theory, we design experiments below to check three main conjectures:

> **Conjecture 1.** *There is more production in Model M with money than in Model M without money.*
>
> **Conjecture 2**. *There is more production in Model M with money than in Model N with money.*
>
> **Conjecture 3**. *Suggestions have more of an impact in Model M with*

money than in Model M without money or in Model N with money.

In practice Conjectures 1 and 2 are how we treat essentiality – it means that a monetary equilibrium theoretically exists, and payoffs dominate the best outcome available without money. Conjecture 3 is about suggestions mattering more when they are consistent with incentives, as a mechanism design approach would suggest.

3 Experimental Design

We now describe the key aspects of our design.[9] Treatments include cases with and without money, cases with money in Models M and N, and cases with and without suggestions. Table 1 summarizes the treatments, labeled with M or N depending on the underlying model, with the first 1 or 0 indicating if there is money, and the second 1 or 0 indicating if suggestions are used. Previous work focuses on comparing treatments with and without money. We do that, and we compare Model M and N with money, since in both cases strategies contingent on monetary offers are feasible, but in theory accepting money is only consistent with equilibrium in Model M. There is one more treatment labeled N-1-0*, where * indicates that we use the design in Davis et al. (2022) to see how that affects the results.

TABLE 1: Treatment and Session Characteristics

Treatment	Money	Suggestions	# of Sessions	# of Subjects per Treatment (Session)
M-0-0	No	No	4	45 (9,9,12,15)
M-1-0	Yes	No	4	51 (12,12,15,12)
M-1-1	Yes	Yes	4	48 (9,12,15,12)
M-0-1	No	Yes	4	51 (12,15,12,12)
N-1-0	Yes	No	4	48 (15,12,9,12)
N-1-0*	Yes	Yes	2	21 (12,9)
N-1-1	Yes	Yes	4	48 (12,12,12,12)

NOTE.—M or N stand for choice of model; the first digit is 1 or 0 for money or no money; the second digit is 1 or 0 for suggestions or no suggestions; * is for a special treatment discussed in the text.

It is standard to have subjects play multiple rounds to gain experience. Unfortu-

[9]Go to www.sultanum.com/papers/Money_Essential_Instruction_and_additional_results.pdf for all appendices, including Appendix H, which has the full instructions given to subjects.

nately, this may make them regard the experiment as a repeated game (more on this below). To provide experience while trying to minimize repeated-game effects, we randomly group players in each round. While some subjects interact more than once, they are anonymous, and the number of participants is large enough that reputation building seems difficult. In Model N, a subject is Player $i \in \{1, 2, 3\}$ in every round, and in Model M a subject is either Player 1, or randomly assigned Player 2 and 3 each round. This diminishes incentives to try to achieve cooperative outcomes. One can imagine, e.g., Player 3 producing, in hopes that it would make others more likely to do so later in the game, but this should be less of an issue given the way subjects are assigned to roles in treatments other than N-1-0*.

Each session of the experiment has multiple parts. First, instructions are read aloud, followed by a quiz to see if subjects understand the game. We then go over the answers as a way to further explain the rules. Then there are 15 rounds of play in either Model M or N. Next, subjects complete an exit survey and a demographic survey.[10] Finally, subjects play a series of generalized dictator games designed to elicit information about social preferences, the idea being that in the theory agents only care about their own payoffs, but they might care about others in the lab, and this is a way to measure that.

At the beginning of a treatment with Model N, each participant is randomly assigned a role as Player $i \in \{1, 2, 3\}$, which they keep for all 15 rounds (with the exception of N-1-0*). In each round, groups of three are formed by randomly drawing one of each type. Player 1 is endowed with a token. To simplify the choice set in the lab, we change the model in Section 2 slightly by letting a consumer either offer money for the good, or not, then letting a producer either produce or not. Consumers can get the good for free if there is production when no money is offered, but what is

[10]The exit survey is discussed in Section 5. As for the demographic survey, Appendix H provides details but, in brief, it asks about gender, age, English proficiency and field of study, and was included because past work finds such characteristics can matter: Croson and Buchan (1989) find women return more wealth than men in trust games, while Eckel and Grossman (1998) find women donate twice as much to anonymous partners in dictator games; Marwell and Ames (1981), Carter and Irons (1991) and Frank et al. (1993) find economics students behave differently in a variety of experiments. Hence, it is somewhat common to check if demographics matter in experiments. It turns out they do *not* matter in our results, but we would not know that without checking.

eliminated from Section 2 is the dominated strategy of offering money for free. After this happens twice, in the first and the second meeting, the round is complete, and players are randomly reassigned to new groups, except when the session ends.

Model M treatments are similar, except that only Player 1 subjects stay in that role for all 15 rounds while the others are either Player 2 or 3 with equal probability in each round, and are uninformed about their role when they decide to produce. In monetary treatments with Model M, Player 1 is endowed with a token and can offer it in exchange in meeting 1, but, different from Model N, the recipient accepts or rejects not knowing if it is meeting 1 or 2. Then, if there is another meeting and Player 2 has a token, it can be offered to Player 3. Player 3 accepts or rejects while similarly uninformed. Then payoffs are tallied and subjects are randomly assigned to new groups, except in round 15 when the session ends.

Subjects start with 3 points, then earn $u = 3$ points from consumption and lose $c = 1$ points from production, so payoffs are nonnegative. Three out of the 15 games are randomly selected for actual dollar payments (while evidence is mixed, some studies find that paying subjects for a subset of games is about as effective as paying for all games, e.g., Charness et al. 2020). Each point is worth 2 dollars, while tokens are worth 0, as explicitly described in the instructions: "The token does not yield points directly and cannot be transferred from one game to another."

In the second part of a session subjects play generalized dictator games, and from the results we compute a SVO (social value orientation) score as in Murphy et al. (2011). Details are in Appendix A and F, but the rationale is to see whether social preferences help explain departures from predictions of theory. Each subject plays 15 of these games and payoffs are determined from one randomly selected round where the subject is a proposer and one where the subject is a receiver.

From 2020 to 2023 we ran four sessions online for each treatment, except N-1-0*, where we ran two.[11] These were programmed using oTree (Chen et al. 2016). The

[11] We ran only two for N-1-0* because it is a robustness treatment, not part of the main analysis, and because in this case each group (as opposite to each session) is an independent observation. Also, we originally ran four in-person sessions in the IELAB lab at Indiana University, before

subject pool was Indiana University students recruited via the Online Recruitment System for Economic Experiments (Greiner 2015). Each subject participated in only one session. The number of subjects per session ranged from 9 to 15, depending on how many showed up from the recruitment procedure. In total there were 312 subjects, who earned on average $19 for 45 to 60 minutes of their time.

4 Main Results

An overall finding is that there is more production in Model M with money – shown in the left panel of Fig. 1 – than in Model M without money or Model N with money – shown in the right panel. Here production is aggregated over both meetings, and the darker lines are averages across treatments. In addition to output being higher in the left panel, it is stable, while in the right it declines over the rounds, as (presumably) subjects figure out that producing for money in Model N, or for nothing in Model M, reduces their payoffs.

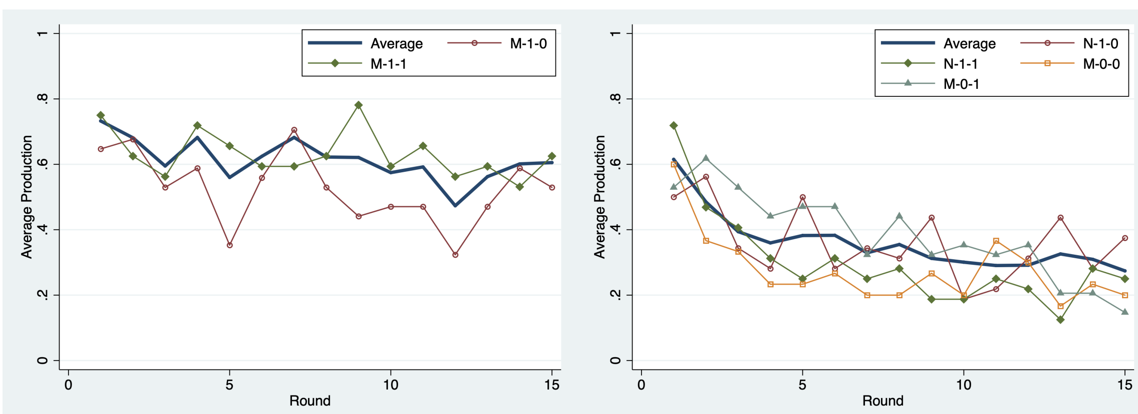

Fig 1. Average production by treatment. The left (right) panel shows the treatments with (without) a monetary equilibrium.

Conjecture 1 is that there is more production in Model M with money than without

moving online due to the pandemic. The in-person results are not used in the main analysis, but are discussed in Appendix B. The main difference is that the in-person results are somewhat closer to theory, so not using them seems conservative. We do not propose a definitive explanation for the difference between online and in-person sessions, and there is no consensus on this in the literature, although Hergueux and Jaquemet (2015) find online subjects make more other-regarding decisions. Another possibility is that online subjects are more distracted, and hence make more noisy decisions. Some evidence for this is that quiz scores were higher for in person sessions (e.g., 95% vs 82% for N-1-0). In fact, when we control for quiz scores in regression analysis in Appendix B, the in-person and online results are not significantly different.

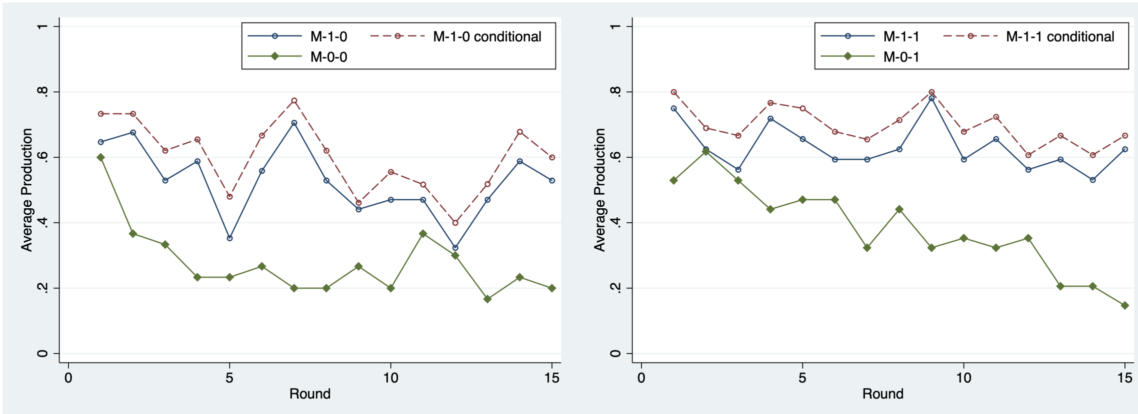

Fig 2. Average production in Model M. The left (right) panel shows treatments without (with) suggestions. Shown is average production unconditional and conditional on a buyer having money.

it. Fig. 2 shows this is true, for cases with and without suggestions, in the right and left panels, respectively. This is similar to findings by Camera and Casari (2014), Duffy and Puzzello (2014a,b) and Davis et al. (2002). However in the first two, in theory money is not essential (recall fn. 6), and we will go into much detail later about Davis et al. (2022). As for *how much* money increases output, Table 2 shows this in terms of the percentage of meetings that have production. Averaged over meetings and all rounds, with money output holds steady at around 52% without suggestions, and around 62% with suggestions. In contrast, without money, after the first few rounds output decreases to around 25%, with or without suggestions.

Table 2 reports p-values from WMW (Wilcoxon-Mann-Whitney) tests where the unit of observation is average production at the session level, for different segments of the experiment – all rounds, rounds 1-5, etc. – to condition on experience. The p-values provide formal nonparametric tests, which are complemented by regression analysis in the Appendices, that yield this conclusion: production is significantly higher with money than without it, especially in later rounds, once subjects settle into the game. With the exception of rounds 1-5, we can reject at reasonable significance levels the null hypothesis that output in Model M is the same with and without money, in favor of the alternative that output is different with and without money. This provides clear support for Conjecture 1.[12]

[12]Here, as in other tables, the p-values generally indicate significance when they should, with a few exceptions that always obtain for rounds 1-5. Also note that we focus on two-sided tests, to

TABLE 2: Production in Model M

	Average				WMW p-values	
	M-1-0	M-0-0	M-1-1	M-0-1	M-1-0 v M-0-0	M-1-1 v M-0-1
All Rounds	0.52	0.28	0.62	0.39	0.029	0.114
Rounds 1-5	0.55	0.37	0.64	0.52	0.114	0.343
Rounds 6-15	0.51	0.24	0.61	0.32	0.029	0.057
Rounds 11-15	0.48	0.25	0.59	0.25	0.057	0.057

NOTE.—The p-values from the WMW test are exact and two-sided. There are 4 observations per treatment.

The results in Table 2 are perhaps not the best test because they are not conditional on the consumer having money, and obviously if money is not accepted in the first meeting then it cannot be offered in the second meeting. Fig. 2 also shows production conditional on the consumer having money, which is around 60% without suggestions and 69% with them. Table 3 provides statistics. From these p-values, we reject at more stringent levels the null that output in Model M is the same with and without money. The results summarized in Table 3 provide even stronger support for Conjecture 1.

TABLE 3: Production in Model M Conditional on Money in Meeting

	Average		WMW p-values	
	M-1-0	M-1-1	M-1-0 v M-0-0	M-1-1 v M-0-1
All Rounds	0.60	0.69	0.029	0.029
Rounds 1-5	0.64	0.72	0.057	0.114
Rounds 6-15	0.58	0.68	0.029	0.029
Rounds 11-15	0.55	0.65	0.029	0.029

NOTE.—Same as Table 2.

Even when money is accepted in most meetings in Model M, it is not accepted in all meetings. Why do some subjects reject it while others seem to coordinate on monetary exchange? We are not too surprised by a few deviations from theory, or deviations by a few subjects, but note that money can be essential if some, not necessarily all, agents accept it. However, there is another interpretation. Recall that

be more conservative, but to get one-sided p-values simply divide by two. In any case, Appendix D discusses production by session, while Appendix E contains parametric analysis. Table E.1 gives results from linear probability and probit models with controls for meetings and rounds. These also show output in Model M is significantly higher with money than without it.

there is a mixed-strategy equilibrium where everyone accepts money with probability $2c/u$, as well as an asymmetric pure-strategy equilibrium where $2c/u$ always and the rest never accept it. In the experiments $2c/u = 2/3$. We obviously do not know that subjects are playing such an equilibrium, but $2/3$ is remarkably close to the numbers in Table 3, where it is between 0.60 and 0.69 over all rounds.

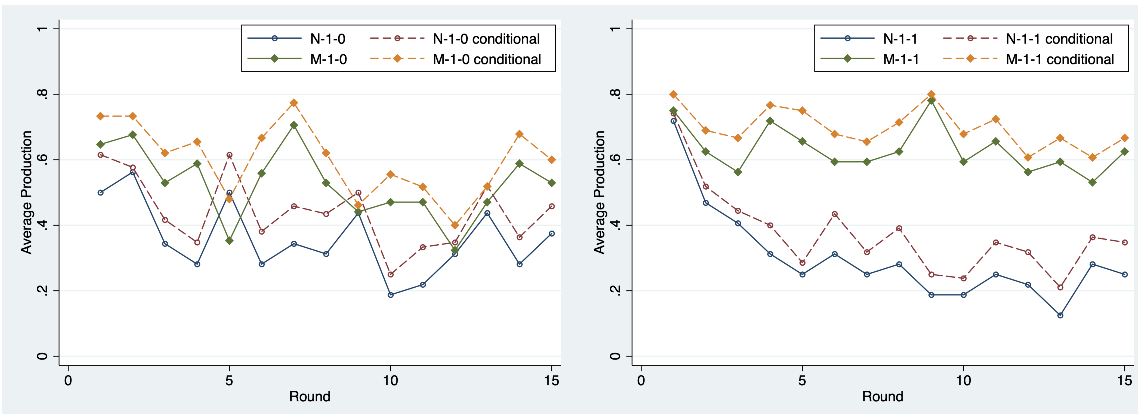

Fig 3. Average production in Models M and N. The left (right) panel shows monetary treatments without (with) suggestions. Shown is average production unconditional and conditional on a buyer having money.

Now consider Conjecture 2, that there is more production in Model M with money than Model N with money. From Fig. 3, this also seems to be true. In Model N without suggestions production averages 35% over all rounds, falling from 43% in the first five rounds to 32% in the last five, and with suggestions it averages 30% over all rounds, falling from 43% in the first five rounds to 22% in the last five (of course, in theory money should never be accepted in Model N, but, again, we are not surprised by a few deviations from theory). In Model M without suggestions, production is 52% over all rounds, only declining from 55% to 48%, and with suggestions it is 62% across all rounds, declining from 64% to 59%. From p-values in Table 4, we can reject the null at reasonable significance levels that output is the same in Model M with money and Model N with money. This provides clear support for Conjecture 2.

Importantly, our result of low monetary exchange in Model N differs from Davis et al. (2002), who find significant monetary exchange in Model N, where money should not even be accepted in theory. In fact, they find money increases output more in

Model N than in Model M. We return to this in more detail in Section 5.[13]

TABLE 4: Production in Treatments with Money

	Average				WMW p-values	
	N-1-0	M-1-0	N-1-1	M-1-1	N-1-0 v M-1-0	N-1-1 v M-1-1
All Rounds	0.35	0.52	0.30	0.62	0.029	0.029
Rounds 1-5	0.43	0.55	0.43	0.64	0.057	0.086
Rounds 6-15	0.31	0.51	0.23	0.61	0.029	0.029
Rounds 11-15	0.32	0.48	0.22	0.59	0.029	0.029

NOTE.—Same as Table 2.

Now consider Conjecture 3, that suggestions have more of an impact in Model M with money than in Model M without money or in Model N with money. The left panel of Fig. 4 and Table 5 summarize the results. In Model M with money, suggestions increase production from 0.52 to 0.62 over all rounds, and the effects are reasonably significant except in rounds 1-5. In the other treatments suggestions have smaller or even negative effects, but they are not significant. From the p-values, we cannot reject the null that suggestions have no effect in Model M without money or Model N with money, and we can reject the null at reasonable significance levels that they have no effect in Model M with money, providing support for Conjecture 3.[14]

We conclude that outcomes can be improved by suggestions if they are consistent with equilibrium, but not otherwise, even if following the suggestions may generate a Pareto superior outcome. So it seems the main impact of suggestions in Model M with money is attributable to coordination, as opposed to a desire by subjects to please the experimenter, or to achieve higher social payoffs.

To summarize, the experimental evidence is broadly consistent with theory: Money is essential in the sense that payoffs are higher in Model M with money than without it. Money is less likely to be used in Model N than Model M. And suggestions are

[13]Again the appendices complement the non-parametric analysis with parametric analysis, and the findings are similar. Table E.2 reports results from linear probability and probit models, with controls for meeting and round, and shows production in Model M with money is significantly higher than in Model N with money.

[14]Once again the appendices complement this analysis with linear probability and probit models with controls for meeting and round, and once again the findings are similar.

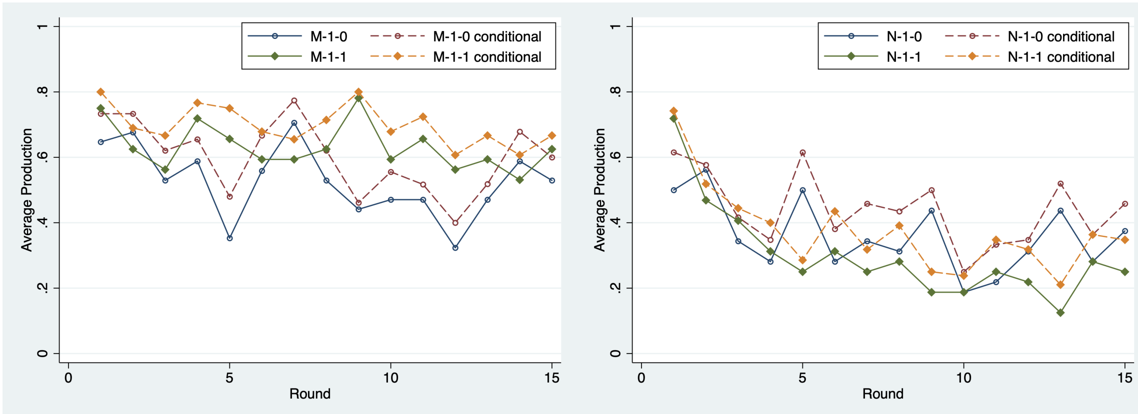

Fig 4. Average production in monetary treatments. The left (right) panel shows Models M-1-0 and M-1-1 (N-1-0 and N-1-1). Shown is average production unconditional and conditional on a buyer having money.

TABLE 5: Effect of Suggestions

Average						
	N-1-0	N-1-1	M-1-0	M-1-1	M-0-0	M-0-1
All Rounds	0.35	0.30	0.52	0.62	0.28	0.39
Rounds 1-5	0.43	0.43	0.55	0.64	0.37	0.52
Rounds 6-15	0.31	0.23	0.51	0.61	0.24	0.32
Rounds 11-15	0.32	0.22	0.48	0.59	0.25	0.25

WMW p-value			
	N-1-0 v N-1-1	M-1-0 v M-1-1	M-0-0 v M-0-1
All Rounds	0.629	0.086	0.400
Rounds 1-5	0.971	0.400	0.114
Rounds 6-15	0.343	0.057	0.571
Rounds 11-15	0.171	0.057	1.000

NOTE.—Same as Table 2.

helpful mainly if they are consistent with equilibrium.

5 Additional Results

We now explore how subjects' behavior correlates with social preferences as captured by SVO scores, demographic characteristics and major field of study, to see how factors not captured by standard theory matter. We also discuss responses from exit surveys to shed further light on subjects' motivation. Then we study how output varies across meetings 1 and 2. Finally, we compare our results to Davis et al. (2022).

We regressed production on agents' SVO scores, demographic characteristics and major field of study separately for each model. As for demographic characteristics and field of study, they are included because past work shows they can matter (recall fn. 10), but they do not have significant effects in our regressions. As for SVO scores, we expected they would be positively correlated with individuals producing whether or not that is consistent with equilibrium. However, the general finding is that coefficients on SVO tend to be insignificant or have the wrong sign, suggesting that either social preferences do not explain why agents produce when theory says they should not, or that our SVO scores are not a good measure of social preferences for our purposes.[15]

We also employed exit surveys, which turned out to provide more insight than SVO regressions. In surveys from the treatments with money, we asked Players 2 and 3 why they produced in exchange for the token, and Tables 6 and 7 give the number choosing each answer; for the nonmonetary treatments, we asked why they produced, and Table 8 gives those numbers. Note that the columns need not add to the number of subjects because they can choose more than one answer.

Table 6: Reasons for Monetary Exchange in Model N

		Player 3		Player 2	
		N-1-0	N-1-1	N-1-0	N-1-1
a	Not applicable: I was never in this situation	5	6	1	0
b	To increase the chance of trading it for the good with another player	1	1	13	14
c	I made a mistake	3	2	0	1
d	To help the other player	6	5	7	7
e	I wanted the token for the sake of it	6	4	1	0
f	To follow the suggestions	-	4	-	7
g	Other reason. Please explain:	1	1	1	1

NOTE.—This table shows the responses to: "If you were offered the token and you produced in exchange for the token, why did you do it? Check all that apply." Option (f) applies only to N-1-1. The total number of subjects is 16 for each treatment

[15] Appendix F regresses production on individual characteristics separately for Model M and N with money, as well as model M without money. The coefficient on SVO is significant at the 5% level only in rounds 6-15 in Model N and then it is negative; it is positive but insignificant in Model M with money; and it is positive and significant only in early rounds in Model M without money.

Starting in Model N with money, but without a monetary equilibrium, and without suggestions, 16 subjects acted as Player 3. Of these, 5 reported that they never produced, consistent with theory. The rest reported that they produced for money. Among those, 6 reported that they wanted to help the other player, which can be interpreted as social preferences. Also, 6 reported that they wanted the token for its own sake, inconsistent with rationality, given the fiat nature of the token. Then 3 selected the option "I made a mistake." Just 1 reported they wanted to increase the chance of trading with another player, even though Player 3 does not meet another player. In the treatment with suggestions, more subjects produced for money, and of those that did, 4 reported they were following the suggestion. For subjects that acted as Player 2, many indicated they produced for money to increase the chance of trading with another player, which can be rationalized if sometimes Player 3 accepts money even though that is not equilibrium play (see below).

In Model M with money, the survey does not distinguish between Players 2 and 3 since roles are uncertain when actions are taken. From Table 7, strategic considerations play a dominant role: most subjects produced for money and said they did so to increase the chance of trading in the next meeting, consistent with monetary equilibrium. Finally, for Model M without money, Table 8 shows some subjects produce when in theory they should not, and many said they did so to increase the chance of others producing for them in this game and to increase the chance of others producing for them in the next game.

Moving to how output varies across meetings 1 and 2, the results are shown in Fig. 6: production in Model M with money is higher in the first than in the second meeting. The difference is statistically significant and big – around 15% (see Appendix G for details). This is production conditional on the consumer having money, so the explanation is not simply that subjects are playing a mixed-strategy equilibrium; instead the finding suggests that subjects can to some extent distinguish between the two meetings, as in the extension of the baseline with noisy signals. Sophisticated subjects may make inferences based on how long they wait for a meeting, and not produce if they infer a high probability of meeting 2. However, it is not likely that

Table 7: Reasons for Monetary Exchange in Model M

		M-1-0	M-1-1
a	Not applicable: I was never in this situation.	1	1
b	To increase the chance of trading it for the good with player 3 in case I turn out to be player 2	31	29
c	I made a mistake	0	1
d	To help the other player	7	8
e	I wanted the token for the sake of it.	1	2
f	To follow the suggestion.	-	5
g	Other reason. Please explain:	1	6

NOTE.—This table shows the responses to: "If you were offered the token and you produced in exchange for the token, why did you do it? Check all that apply." Option (f) applies only to M-1-1. The total number of subjects is 34 for treatment M-1-0, and 32 for treatment M-1-1.

TABLE 8: Reasons for Production in Model M without Money

		M-0-0	M-0-1
a	Not applicable: I never produced	6	5
b	To increase the chance of others producing for me in this game	15	15
c	To increase the chance of others producing for me in a following game	16	24
d	I made a mistake	1	1
e	To help the other player	10	18
f	To follow the suggestion	-	4
g	Other reason. Please explain:	1	3

NOTE.—This table shows the responses to the question, "If you produced in a game, why did you do it? Check all that apply." Option (f) applies only to M-0-1. The total number of subjects is 30 for treatment M-0-0, and 34 for treatment M-0-1.

they can predict perfectly whether it is meeting 1 or 2.

At the end of the sessions, Players 2 or 3 were asked whether they could tell what their positions were, and some of them said that they tried to guess based on the time they had to wait to have access to the decision screen. However, some also said their guesses were often wrong, suggesting that inference is noisy. During the experiment, subjects proceed to meeting 2 after their groups finish meeting 1, so a longer waiting time can also be due to slow group members, making inference noisy in practice. The fact that the difference between production in meetings 1 and 2 is bigger in Model M than Model N also lines up with theory.

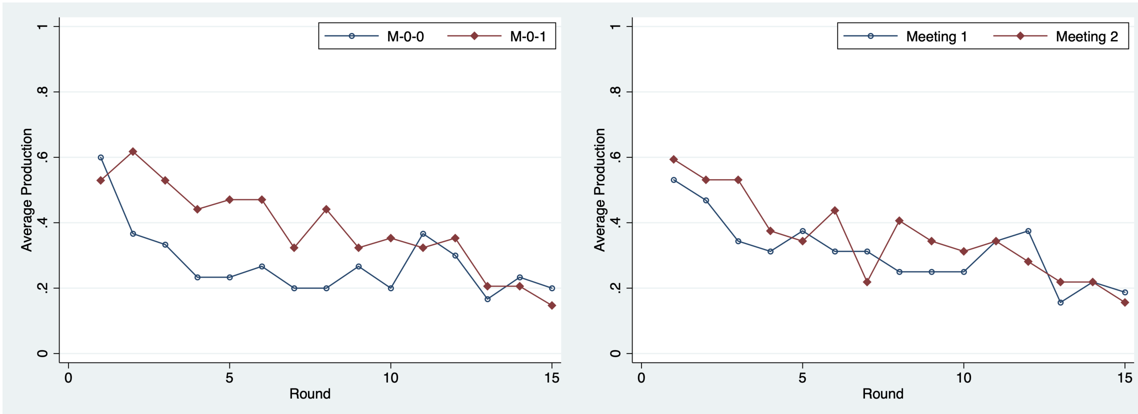

Fig 5. The left (right) panel shows average production in Models M-0-0 and M-0-1 (production by meeting using pooled data from Models M-0-0 and M-0-1).

In Model N subjects know which meeting they are in. Hence, in theory no one should produce for money in either meeting, but in practice the two meetings are not quite the same, and this shows up in Fig. 6, where the right panel displays production in the first and second meeting for treatments with Model N. This can be explained by noticing that if you accept money in the first meeting there is at least a chance you can spend it in the next meeting – not in equilibrium, but in the experiment – while if you accept it in the second meeting there is no chance. Hence, even if someone is rational, there is a rationale for accepting money if it is believed that other players may accept it due to irrationality, social preferences, or limited ability to use backward induction. In any case, in Model M without money there is no systematic difference between the two meetings, again consistent with theory, as can be seen in the right panel of Fig. 5.[16]

Next, we compare our results, which are largely consistent with theory, to those in Davis et al. (2022), where in Model N agents are more likely to use money even though that is not an equilibrium. A candidate explanation for the disparate results is that the experimental designs are different. For one difference, note that there are two common ways to experiment with dynamic games: the "strategy method" where ex ante subjects make conditional decisions for each possible information set,

[16] Appendix G reports regression results verifying that production is significantly lower in the second meeting in Model M and in model N with money. It also shows that production does not decline across meeting in Model M without money.

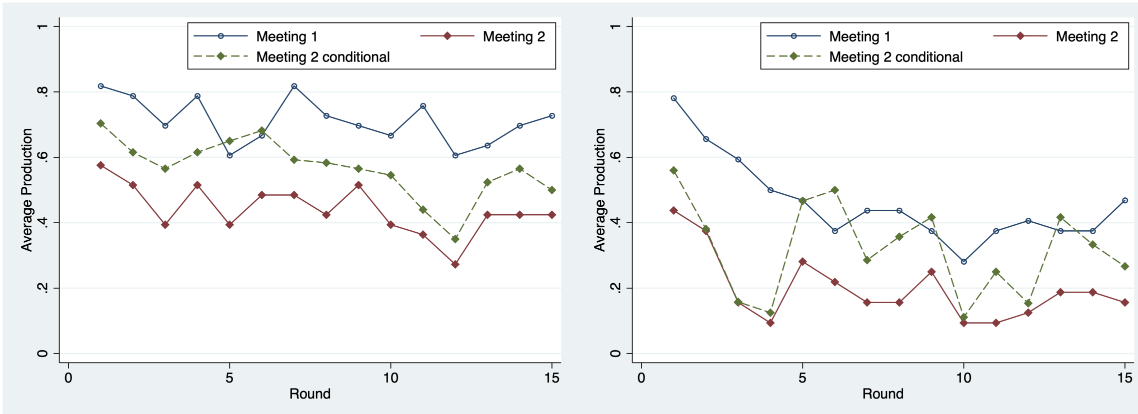

Fig 6. Average production by meeting. The left (right) panel pools data from treatments M-1-0 and M-1-1 (N-1-0 and N-1-1). Shown is average production unconditional and conditional on a buyer having money.

and the "direct-response method" where they observe previous play before deciding. We use the latter, as it better captures the dynamic nature of the theory, and use it consistently in all treatments, which is not the case in Davis et al. (2022).

Probably a more important difference is the way we try to reduce repeated-game effects. In our experiments subjects have fixed roles within a treatment and are randomly matched in each round. In contrast, in Davis et al. (2022) each group interacts repeatedly, randomly switching roles. One possibility is that subjects in the older design might be more prone to repeated-game reasoning, leading to cooperative behavior: subjects in one role may think their actions influence others' actions later when their roles are reversed. In the newer design, their roles are not reversed within the session, and across rounds they are not very likely (due to random regrouping) to meet again the same agents.[17]

This is crucial because these kinds of repeated-game effects can make it more likely to observe monetary exchange in model N, where monetary equilibrium does not exist (you take money from someone today to increase the likelihood they take it from you later, even though it is not a best response in either case). To test this,

[17]To be clear, backward induction applies in both cases, so in theory the results should not change; but given subjects may fail at backward induction, which is not uncommon in experiments, whether or not they play repeatedly with the same subjects in different roles might matter. Cooperation in the newer design seems difficult: if you are Player 3, e.g., you should not consider producing for Player 2 in one round to get them to maybe produce when you meet again and the roles are reversed, since you remain Player 3 during the entire session in Model N.

we ran two sessions of the treatment labeled N-1-0*, for Model N with money and no suggestions, replicating the older design, i.e., we keep a small group of subjects interacting repeatedly and randomly switching roles.

Details are in Appendix C but the results can summarized as follows: (i) N-1-0* has similar overall production to M-1-0 (0.50 vs 0.52); (ii) N-1-0* has significantly higher production than N-1-0 (0.50 vs 0.35); (iii) in the exit survey for N-1-0*, when subjects were type 2 and produced to get the token, 81% said they did it to increase the chance that their group members produce for them in the future, when roles may be reversed; and when subjects were type 3 and produced to get the token, 57% said they did so for that reason. The bottom line is that when we use the design in Davis et al. (2022) we replicate the results referred to as "puzzling" in that paper, and when we use the new design we get results that are more consistent with theory.

6 Conclusion

This paper studied, theoretically and experimentally, models of exchange that can have valued fiat currency even with a finite horizon, focusing on essentiality and a mechanism design approach. The introduction of money was found to have large and significant effects on production in Model M, consistent with theory. Monetary exchange and production were low and declined quickly with experience in Model N, also consistent with theory. These results provide evidence that money is used for strategic reasons: agents trade to get it because they rationally expect they may later trade it for something else. When money should not be accepted, sometimes it is, as in past experiments. Based on exit surveys, if not SVO measures, this may be due to social preferences, although some subjects admitted to making mistakes.

Another finding is that even in Model M, when most agents accept money, some do not. There are alternative ways to interpret this, including the possibility that they are playing mixed strategies. Yet another result is that suggestions improved outcomes when they were incentive compatible, but not much otherwise, implying that their impact does not come from subjects feeling obliged to follow them, but from coordinating on monetary equilibrium. We also found some subjects used waiting time as an indicator of position in the trading sequence, which led us to extend the theory to allow inferences. This extension implies that monetary exchange is more likely in the first meeting than in the second meeting, which is consistent with the findings.

In terms of extensions, one idea is to add more agents or meetings to see how that affects the results. Another is to study alternative ways to coordinate play: in addition to suggestions, one could consider different specifications for private or public histories, or pre-play communication. Also, there are other ways to get monetary equilibria in finite environments – e.g., after the final period of the exchange game, add a one-shot game with multiple equilibria, where selection depends on whether money was accepted in the past. There are many applications and extensions of monetary economics that can be studied in the lab, including models of commodity instead of fiat money, models with multiple monies, etc., and one can revisit all those using finite-horizon theory. Finally, it would be interesting to make goods or money divisible, something neglected here to avoid determining the terms of trade, allowing us to focus on the pattern of trade.

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Appendices for Online Publication

A Social Value Orientation

We use a measure of SVO (Social Value Orientation) introduced by Murphy et al. (2011) to capture social or altruistic preferences. This is constructed by having participants play six generalized dictator games that differ in the costs for the sender to give money to the receiver. The SVO index is computed as an increasing function of the ratio of the amount given to the amount kept, so higher SVO scores correspond to more altruistic preferences (see Murphy et al. 2011). Experiments were conducted using the computerized module for zTree and oTree developed by Crosetto et al. (2019) with the ring matching protocol, where each subject acts as both a sender and a receiver (see Crosetto et al. 2019 for details). In addition to the six games used to compute SVO scores, we added nine secondary games from Murphy et al. (2011) and Crosetto al. (2019) that may help disentangle motives associated with maximization of social surplus from equality concerns, but since not even the basic SVO score has any explanatory power, we did not pursue this. One game where the subject was a proposer and another where the subject was a receiver were randomly chosen to determine subjects' payments.

B In-person vs Online Sessions of Treatment N-1-0

Before the pandemic, we ran four in-person sessions for Model N-1-0 with subjects from the same pool as the online sessions used in the paper. The in-person sessions were programmed using zTree (Fischbacher 2007).

Table 9 provides a non-parametric comparison of production rates between online and in-person sessions overall and conditional on money offers, and Table 10 provides a parametric comparison where we add controls. Online production rates are higher than in-person, even when controlling for age, gender, field of study, and SVO scores (controls A). However, when adding controls for the quiz score (controls B), the difference is no longer significant. The average quiz score is 95% for in-person and 82% for online sessions, suggesting that subjects may be more inattentive online.[18]

Survey results displayed in Table 11 also suggest that subjects may be more confused ("I made a mistake" or "I wanted the token for the sake of it") online than in-person, which may explain the higher production. These results are consistent with

[18]For the in-person sessions, the quiz was done on paper. We retrieved the quiz score of 44 subjects out of 51. Of those, we managed to link the quiz score to choices for 35 participants. The average quiz score of 82% and the regression with controls in specification B are based on these 35 participants.

TABLE 9: AVERAGE PRODUCTION IN MODEL N-1-0: ONLINE VS. IN-PERSON

	Average				WMW p-values	
	Online	In-person	Online (Cond.)	In-person (Cond.)	Online vs. In-person	Online v In-person (Cond.)
All Rounds	0.35	0.17	0.44	0.25	0.029	0.029
Rounds 1-5	0.43	0.20	0.51	0.28	0.086	0.114
Rounds 6-15	0.31	0.16	0.40	0.23	0.057	0.057
Rounds 11-15	0.32	0.14	0.40	0.22	0.029	0.029

NOTE.—The p-values from the WMW test are exact and two-sided, and there are 4 observations per treatment.

TABLE 10: PRODUCTION IN MODEL N-1-0: ONLINE VS. IN-PERSON

Rounds	In-Person	Online	Difference (t-test)	Difference (controls A)	Difference (controls B)	# of Obs.
All	0.1706***	0.3583***	0.1877***	0.1382**	0.0489	990
	(0.0243)	(0.0445)	(0.0470)	(0.0561)	(0.0739)	
1–5	0.2000***	0.4375***	0.2375***	0.1928**	0.1028	330
	(0.0254)	(0.0442)	(0.0473)	(0.0549)	(0.0773)	
6–15	0.1559**	0.3188***	0.1629**	0.1108	0.0220	660
	(0.0436)	(0.0553)	(0.0652)	(0.0733)	(0.1005)	
11–15	0.1412**	0.3250***	0.1838**	0.1294*	0.0294	330
	(0.0388)	(0.0457)	(0.0556)	(0.0637)	(0.0847)	

NOTE.— Standard errors in parentheses are clustered by session.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Controls A include age, gender, field of study and their SVO scores. Controls B includes A and adds subjects' quiz score.

Hergueux and Jaquemet (2015), who find that subjects tend to make more other-regarding decisions in online settings. Indeed, the exit surveys indicate that player 3 more frequently were willing to produce for the other player "To help the other player" online than in-person.

There is no consensus on the difference between online and in-person experiments. Our results line up with Hergueux and Jaquemet (2015), but others, such as Buso et al. (2021), find no differences. Further investigations into differences between online and laboratory behavior are beyond the scope of our paper.

TABLE 11: REASONS FOR MONETARY EXCHANGE IN N-1-0: ONLINE VS. IN-PERSON

		Player 3		Player 2	
		Online	In-person	Online	In-person
a	Not applicable: I was never in this situation	5	13	1	3
b	To increase the chance of trading it for the good with another player	1	1	13	12
c	I made a mistake	3	1	0	1
d	To help the other player	6	1	7	5
e	I wanted the token for the sake of it	6	2	1	1
f	Other reason. Please explain:	1	0	1	2

NOTE.—The number of responses to the question: "If you were offered the token and you produced in exchange for the token, why did you do it? Check all that apply." The total number of subjects of each type is 16 for the four online sessions, and 17 for the four in-person sessions.

C Alternative Implementation of Model N-1-0

Our results differ starkly from Davis et al. (2022), where production rates are similar regardless of whether money is essential or not. We believe that this can be attributed to aspects of their design that generated repeated game effects. This is important because when desirable allocations can be supported using repeated game strategies money is not essential in theory.

To explore this, we conducted two additional sessions of Model N-1-0 adopting an alternative design, similar to Davis et al. (2022). In this treatment, which we label N-1-0*, subjects played in fixed groups of three participants each for all 15 rounds, and their role was randomly determined at the beginning of each round. These two sessions generated seven independent observations (one session had three independent groups and the other had four). Our results suggest that these design choices indeed affect production rates (see Table 12, Table 13 and Figure C.1) as conjectured. There is more production in treatment N-1-0* than in N-1-0 (averaged across all 15 rounds, the average production rate is 0.35 in N-1-0 versus 0.50 for N-1-0*). Further, production rates in treatment N-1-0* are comparable with treatment M-1-0, exactly like in Davis et al. (2022). Table 14 reports results from the exit survey, which also provides suggestive evidence that many subjects approached the experiment as a repeated game: the most common explanation for producing in exchange of money is "To increase the chance that my group members produce for me in future games when I could turn out to be player 1 or 2".

TABLE 12: Average Production in Model N-1-0, N-1-0* and M-1-0

	Average			WMW p-values	
	N-1-0	N-1-0*	M-1-0	N-1-0 vs. N-1-0*	N-1-0 vs. M-1-0
All Rounds	0.35	0.50	0.52	0.067	0.873
Rounds 1-5	0.43	0.58	0.55	0.248	0.800
Rounds 6-15	0.31	0.45	0.51	0.067	0.248
Rounds 11-15	0.32	0.41	0.48	0.053	0.630

NOTE.—The p-values from the WMW test are exact and two-sided, and there are 4 observations in treatments N-1-0 and M-1-0, and 7 observations in treatment N-1-0*.

Table 13: Production in Models N-1-0* vs. N-1-0 v. M-1-0

Rounds	N-1-0*	N-1-0	Difference (t-test)	# of Obs.
All	0.5000*** (0.0346)	0.3583*** (0.0219)	0.1417** (0.0477)	690
1–5	0.6000*** (0.0590)	0.4375*** (0.0393)	0.1625 (0.0982)	230
6–15	0.4500*** (0.0422)	0.3188** (0.0261)	0.1313* (0.0537)	460
11–15	0.4000*** (0.0590)	0.3250*** (0.0371)	0.0750 (0.0795)	230
Rounds	**N-1-0***	**M-1-0**	**Difference (t-test)**	**# of Obs.**
All	0.5000*** (0.0346)	0.5255*** (0.0221)	−0.0255 (0.0264)	720
1–5	0.6000*** (0.0590)	0.5588*** (0.0382)	0.0412 (0.1026)	240
6–15	0.4500*** (0.0422)	0.5088*** (0.0272)	−0.0588* (0.0262)	480
11–15	0.4000*** (0.0590)	0.4765*** (0.0384)	−0.0765 (0.0690)	240

NOTE.– Standard errors in parentheses are clustered by session.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

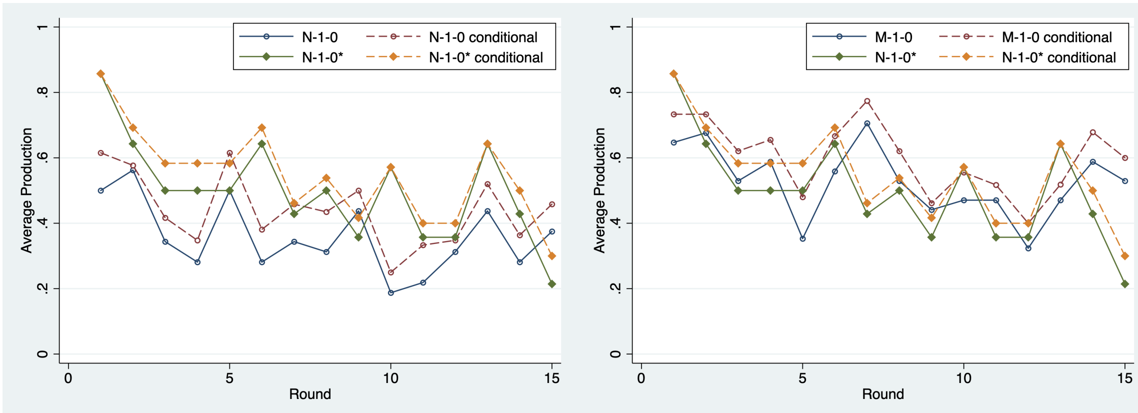

Fig C.1. Average production in Models M-1-0, N-1-0 and N-1-0⋆. Shown are average production unconditional and conditional on a buyer having money.

TABLE 14: Reasons for Monetary Exchange in N-1-0⋆

		Player 3	Player 2
a	Not applicable: I was never in this situation	3	1
b	To increase the chance of trading it for the good with another player in that particular game	9	16
c	To increase the chance that my group members produce for me in future games where I could turn out to be player 1 or 2	12	17
d	I made a mistake	2	1
e	To help the other player	5	3
f	I wanted the token for the sake of it	4	1
g	Other reason. Please explain:	2	1

NOTE.—The number of responses to the question, "In games where you were player 2 (3), when player 1 (2) offered you the token and you produced in exchange for the token, why did you do it?" The total number of subjects is 21.

D Production by Session and Treatment

Table D.1 reports production by session and treatment for rounds 1-5, 6-15, 11-15 rounds, and all rounds. Table D.2 provides the same information conditional on money in the meeting.

Table 15: Production by Session: All Meetings

Treatment	Session	Rounds				Treatment	Session	Rounds			
		1–5	6–15	11–15	All			1–5	6–15	11–15	All
M-1-0	1	0.64	0.52	0.48	0.56	N-1-0	1	0.42	0.19	0.20	0.27
	2	0.50	0.49	0.42	0.49		2	0.30	0.27	0.33	0.28
	3	0.42	0.57	0.52	0.52		3	0.55	0.36	0.35	0.42
	4	0.65	0.45	0.48	0.52		4	0.44	0.42	0.40	0.43
	Mean	0.55	0.51	0.48	0.52		Mean	0.43	0.31	0.32	0.35
M-1-1	1	0.75	0.67	0.65	0.7		5†	0.20	0.16	0.14	0.17
	2	0.57	0.55	0.50	0.58		6†	0.15	0.14	0.12	0.14
	3	0.50	0.58	0.53	0.56		7†	0.27	0.05	0.05	0.12
	4	0.76	0.64	0.66	0.68		8†	0.17	0.27	0.25	0.24
	Mean	0.65	0.61	0.59	0.62		Mean	0.20	0.16	0.14	0.17
M-0-0	1	0.24	0.26	0.3	0.25	N-1-1	1	0.27	0.09	0.1	0.15
	2	0.37	0.22	0.2	0.27		2	0.50	0.34	0.27	0.39
	3	0.33	0.1	0.07	0.18		3	0.52	0.27	0.22	0.36
	4	0.53	0.37	0.43	0.42		4	0.42	0.24	0.3	0.3
	Mean	0.37	0.24	0.25	0.28		Mean	0.43	0.23	0.22	0.3
M-0-1	1	0.62	0.31	0.22	0.42	N-1-0*	1	0.70	0.44	0.32	0.52
	2	0.42	0.26	0.22	0.32		2	0.47	0.47	0.50	0.47
	3	0.38	0.16	0.06	0.23		Mean	0.58	0.45	0.41	0.50
	4	0.67	0.56	0.52	0.6						
	Mean	0.52	0.32	0.25	0.39						

NOTE.— (†) Sessions were conducted in person. All the other sessions were conducted online. Treatment N-1-0⋆ was conducted with subjects in fixed groups and random roles.

Table 16: Production by Session: Conditional on Money in Meeting

Treatment	Session	Rounds				Treatment	Session	Rounds			
		1–5	6–15	11–15	All			1–5	6–15	11–15	All
M-1-0	1	0.74	0.60	0.55	0.64	N-1-0	1	0.50	0.28	0.30	0.36
	2	0.59	0.59	0.50	0.59		2	0.38	0.35	0.41	0.36
	3	0.53	0.65	0.64	0.61		3	0.62	0.46	0.45	0.52
	4	0.70	0.48	0.50	0.57		4	0.53	0.51	0.46	0.51
	Mean	0.64	0.58	0.55	0.60		Mean	0.51	0.40	0.40	0.44
M-1-1	1	0.79	0.75	0.74	0.76		5†	0.30	0.25	0.23	0.27
	2	0.68	0.61	0.56	0.63		6†	0.20	0.22	0.20	0.21
	3	0.60	0.66	0.62	0.64		7†	0.37	0.07	0.09	0.20
	4	0.81	0.69	0.67	0.73		8†	0.17	0.37	0.33	0.33
	Mean	0.72	0.68	0.65	0.69		Mean	0.28	0.23	0.22	0.25
N-1-0*	1	0.74	0.50	0.40	0.58	N-1-1	1	0.37	0.15	0.17	0.23
	2	0.56	0.51	0.54	0.53		2	0.56	0.44	0.39	0.48
	Mean	0.65	0.50	0.47	0.56		3	0.53	0.33	0.31	0.41
							4	0.52	0.34	0.39	0.40
							Mean	0.49	0.32	0.31	0.38

NOTE.— Sample only includes meetings where the consumer entered the meeting with money. (†) Sessions were conducted in person. All the other sessions were conducted online. Treatment N-1-0⋆ was conducted with subjects in fixed groups and random roles.

E Regression Analysis

In the main text we report p-values from Wilcoxon-Mann-Whitney non-parametric tests to support our findings, and partition our sample into rounds 1–5, 6–15 and 11–15 because we expect play in early rounds to reflect more experimentation and mistakes. Here we summarize OLS (ordinary least square) estimations of the linear probability model and MLE (maximum likelihood estimations) of the probit model. We also provide a robustness check of data partitioning by tabulating results from very early (1-3) and late rounds (13-15).

E.1 Money and Suggestions in Model M

Here we regress production on dummies for money, the interaction with suggestions and controls for round and meeting. The results in Table 17pool data from treatments M-1-0, M-0-0, M-1-1 and M-0-1. We also ran regressions using controls considered in Appendix F, but do not report them here as results are similar.

Results from linear probability and probit estimations are qualitatively and quantitatively very similar, and consistent with the non-parametric results, except that the positive effects of money have higher significance levels. Money increases production between 18% to 33% depending on the round. Aggregating over all rounds, it appears that the effect of suggestions is of similar magnitude when it should not have an effect according to theory (without money) and when it could have a coordinating effect (with money). However, the effect of suggestions without money is concentrated in early rounds, and is slightly negative in late rounds. In contrast, the effect of suggestions in the monetary version of Model M is stable and significant except in the earliest rounds. This suggests that subjects learn not to follow suggestions in treatment M-0-1, but not in M-1-1.

E.2 Model M vs Model N

Next we pool the data from all online treatments with money and regress production on a dummy for Model M, interactions between Models M and N, suggestions and controls for meeting and round. Again we consider OLS of a linear probability and MLE of a probit specification. As the main effect of interest is on the use of money to increase production, we only consider production conditional on the consumer having money.

Table 18 summarizes the results for the linear probability model and the marginal effects from the probit regression. Again the linear probability and probit specifications are similar. Production in Model M-1-0 is more than 15% higher than in Model N-1-0. Suggestions do not have a significant effect in Model N-1-0. By contrast, in Model M, the suggestion has significant effects in all but the earliest rounds.

Table 17: PRODUCTION IN MODEL M

	LINEAR PROBABILITY MODEL			PROBIT MARGINAL EFFECTS			
Rounds	Money	Suggestion × Money= 0	Suggestion × Money= 1	Money	Suggestion × Money= 0	Suggestion × Money= 1	# of Obs.
All	0.2477*** (0.0397)	0.1046 (0.0811)	0.1057** (0.0363)	0.2645*** (0.0464)	0.1168 (0.0894)	0.1111*** (0.0394)	1,950
1–5	0.2055** (0.0732)	0.1643* (0.0861)	0.1037 (0.0735)	0.2139*** (0.0782)	0.1707* (0.0902)	0.1108 (0.0792)	650
6–15	0.2688*** (0.0457)	0.0747 (0.0872)	0.1068*** (0.0336)	0.2876*** (0.0541)	0.0884 (0.1008)	0.1092*** (0.0343)	1,430
11–15	0.2231*** (0.0606)	−0.0063 (0.1062)	0.1173** (0.0412)	0.2365*** (0.0694)	−0.0047 (0.1297)	0.1173*** (0.0424)	650
1–3	0.1843*** (0.0547)	0.1255 (0.0816)	0.0282 (0.0654)	0.1868*** (0.0553)	0.1249 (0.0809)	0.0311 (0.0693)	390
13–15	0.3294*** (0.0840)	−0.0137 (0.1051)	0.0539 (0.0907)	0.3484*** (0.0968)	−0.0137 (0.1444)	0.0523 (0.0877)	390

NOTE.– Regression of production on money, suggestion interacted with money, and controls. Money is a dummy that equals 1 in models M-1-0 and M-1-1, and suggestion is a dummy that equals 1 in models M-0-1 and M-1-1. Controls are meeting and round. Standard errors in parentheses are clustered at the session level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 18: PRODUCTION IN MODEL M VS. N WITH MONEY

	LINEAR PROBABILITY MODEL			PROBIT MARGINAL EFFECTS			
Rounds	Model M	Suggestion × Model N	Suggestion × Model M	Model M	Suggestion × Model N	Suggestion × Model M	# of Obs.
All	0.1700*** (0.0448)	−0.0634 (0.0635)	0.0989** (0.0340)	0.1749*** (0.0472)	−0.0672 (0.0665)	0.1100*** (0.0388)	1,549
1–5	0.1360** (0.0610)	−0.0256 (0.0572)	0.0956 (0.0651)	0.1398** (0.0619)	−0.0258 (0.0582)	0.1082 (0.0736)	543
6–15	0.1869*** (0.0603)	−0.0884 (0.0745)	0.1020** (0.0401)	0.1915*** (0.0639)	−0.0959 (0.0804)	0.1110** (0.0431)	1,006
11–15	0.1530*** (0.0450)	−0.0936 (0.0601)	0.1153** (0.0446)	0.1582*** (0.0474)	−0.1024 (0.0666)	0.1232** (0.0485)	502
1–3	0.1715** (0.0672)	0.0496 (0.0756)	0.0252 (0.0564)	0.1783** (0.0694)	0.0533 (0.0770)	0.0317 (0.0632)	336
13–15	0.1582 (0.0919)	−0.1493 (0.0862)	0.0489 (0.0917)	0.1621* (0.0952)	−0.1588* (0.0936)	0.0520 (0.0970)	302

NOTE.– Regression of production on Model M, suggestion interacted with Model M and Model N, and controls. Model M is a dummy that equals 1 in models M-1-0 and M-1-1, Model N is a dummy that equals 1 in models N-1-0 and N-1-1, and suggestion is a dummy that equals 1 in models M-0-1 and M-1-1. Controls are meeting and round. Standard errors in parentheses are clustered at the session level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

F Social Preferences and Demographics

Table 19 reports OLS regression results for SVO, demographic variables and field of study controlling for meeting and round.

Besides SVO scores, we expected that a dummy for majoring in economics or finance (econfin in Table 19) could be important. We ran separate regressions for monetary treatments in Model N (N-1-0 and N-1-1), monetary treatments in Model M (M-1-

Table 19: Production and Individual Characteristics

Models N-1-0 and N-1-1							
Rounds	SVO	Male	EconFin	Suggestion	Age	Native	# of Obs.
All	−0.0033 (0.0022)	0.0082 (0.0899)	0.0067 (0.1117)	−0.0432 (0.0825)	−0.0006 (0.0047)	−0.0502 (0.0792)	701
1–5	−0.0012 (0.0022)	−0.0312 (0.0913)	0.0359 (0.1523)	−0.0214 (0.0684)	−0.0040 (0.0054)	−0.0708 (0.0838)	256
6–15	−0.0045 (0.0027)	0.0394 (0.1014)	−0.0125 (0.1022)	−0.0551 (0.0959)	0.0014 (0.0047)	−0.0435 (0.0931)	445
11–15	−0.0027 (0.0032)	0.0352 (0.1028)	−0.0860 (0.1231)	−0.0572 (0.0812)	−0.0006 (0.0060)	−0.0515 (0.1316)	224
Models M-1-0 and M-1-1							
Rounds	SVO	Male	EconFin	Suggestion	Age	Native	# of Obs.
All	0.0010 (0.0020)	−0.0067 (0.0349)	0.1124 (0.0618)	0.1068** (0.0430)	0.0162 (0.0178)	−0.0176 (0.0598)	848
1–5	0.0011 (0.0016)	0.0564 (0.0576)	0.0571 (0.0489)	0.0881 (0.0660)	0.0190* (0.0095)	0.0374 (0.0803)	287
6–15	0.0011 (0.0026)	−0.0378 (0.0536)	0.1403 (0.0844)	0.1156* (0.0520)	0.0150 (0.0271)	−0.0412 (0.0875)	561
11–15	0.0016 (0.0026)	−0.0960 (0.0678)	0.2338* (0.1019)	0.1430** (0.0595)	0.0242 (0.0254)	−0.0758 (0.0823)	278
Models M-0-0 and M-0-1							
Rounds	SVO	Male	EconFin	Suggestion	Age	Native	# of Obs.
All	0.0054* (0.0024)	−0.0457 (0.0659)	−0.0453 (0.0696)	0.1011 (0.0768)	0.0084 (0.0081)	0.0042 (0.0317)	960
1–5	0.0062** (0.0018)	−0.0198 (0.0682)	−0.0521 (0.0648)	0.1573* (0.0772)	0.0080 (0.0073)	0.0453 (0.0329)	320
6–15	0.0049 (0.0028)	−0.0569 (0.0730)	−0.0434 (0.0894)	0.0733 (0.0844)	0.0086 (0.0091)	−0.0166 (0.0401)	640
11–15	0.0048 (0.0033)	−0.0177 (0.0557)	−0.0672 (0.1089)	−0.0014 (0.1065)	0.0116 (0.0089)	0.0540 (0.0532)	320

NOTE.– Regression of Production on SVO, male, econFin, suggestion, age, native, and controls. The variable SVO is explained in Appendix A, male equals 1 if male, econFin is a dummy that equals 1 for subjects majoring in economics or finance, suggestion is a dummy that equals 1 in models N-1-1, M-1-1, M-0-1, age is age in years, and native is a dummy that equals 1 for producers who are native English speakers. Controls are meeting and round. Standard errors in parentheses are clustered at the session level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

0 and M-1-1), and nonmonetary treatments in Model M (M-0-0 and M-0-1). For monetary treatments, we only consider meetings with money, but this does not affect the conclusions. As Table 19 shows, SVO scores and individual characteristics have small effects that are either insignificant or have unexpected signs. SVO tends to have a negative impact on production in Model N, and a positive effect in Model

M with money, but the magnitude is small and insignificant in late rounds. SVO is significant in the nonmonetary treatments in early rounds but not after the first five. Males tend to produce more in Model N and less in Model M, but this is also insignificant. Economic training seems to help some subjects find equilibria: economics and finance students produces more for money in Model M and less in Model N, but this is not significant at the 10% level except for late rounds for Model M.

G Meeting 1 vs Meeting 2

Table 21 shows results of the regression of production on a dummy for meeting 2 in a linear probability model. We also include a dummy for Model M or N and interact Model M or N with the meeting and suggestions. In addition, we considered the interaction of meeting and suggestions but the effects are small, insignificant, and not robust to specification.

Table 20: PRODUCTION IN MEETING 1 vs MEETING 2

Rounds	Model M	Meeting 2 ×		Suggestion ×		#
		Model N	Model M	Model N	Model M	of Obs.
All	0.2417*** (0.0768)	−0.2274*** (0.0381)	−0.1501*** (0.0260)	0.1097* (0.0615)	0.0990** (0.0340)	1,680
1–5	0.0368 (0.1157)	−0.3379*** (0.0451)	−0.1180** (0.0543)	0.1814** (0.0701)	0.0933 (0.0642)	588
6–15	0.3431*** (0.1075)	−0.1723*** (0.0463)	−0.1680*** (0.0339)	0.0690 (0.0715)	0.1035** (0.0402)	1,092
11–15	0.3284** (0.1220)	−0.2031*** (0.0610)	−0.2115*** (0.0555)	0.0669 (0.0657)	0.1167** (0.0447)	544

NOTE.– Regression of production on Model M, meeting 2 interacted with Model M and Model N, suggestion interacted with Model M and Model N, and round. Model M is a dummy that equals 1 in models M-1-0 and M-1-1, meeting 2 is a dummy that equals 1 in the second meeting, Model N is a dummy that equals 1 in models N-1-0 and N-1-1, and suggestion is a dummy that equals 1 in models M-1-1 and N-1-1. Standard errors in parentheses are clustered at the session level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Results are similar whether we include these variables so we do not report them. The regression includes observations from the four monetary treatments and we only consider meetings where the consumer has the token. There are 1,680 such meetings, where 1,092 are meeting 1 and 588 are meeting 2. In both Models M and N, subjects produce significantly less (by 15% in Model M, and by 23% in Model N) in meeting 2. In Model M, this is consistent with subjects trying to infer which meeting they are in (see Section 5 of the paper). Production in later rounds is still more than 30% higher in Model M than Model N, where they know which meeting they are in.

In Table 21 we display the results for testing the difference in production between meetings in Model M without money (treatments M-0-0 and M-0-1). We run a regression similar to the one above of production on Meeting 2, suggestion, and round as control. The overall difference is 4%, but it is not significant with a p-value of 30%. In the last 5 rounds the difference is even smaller at 1% and a p-value of 81%.

Table 21: MEETING 1 vs 2 IN MODEL M WITHOUT MONEY

Rounds	Meeting 2	Suggestion	# of Obs.
All	0.0417 (0.0368)	0.1046 (0.0840)	960
1–5	0.0687 (0.0433)	0.1643 (0.0892)	320
6–15	0.0281 (0.0419)	0.0747 (0.0904)	640
11–15	−0.0125 (0.0507)	−0.0063 (0.1100)	320

NOTE.— Regression of production on meeting 2, suggestion, and round as control. Meeting 2 is a dummy that equals 1 in the second meeting, and suggestion is a dummy that equals 1 in treatment M-0-1. Standard errors in parentheses are clustered at the session level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.