Money and Credit with a Finite Horizon: Theory and Experimental Evidence

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Abstract

We consider a finite horizon model where monetary equilibria are sustained using an endgame with multiple equilibria. In the spirit of information design we also study effects of strategy recommendations. Experimental results show that monetary exchange occurs in almost every exchange meeting in our main monetary treatment when recommendations are used, generating desirable allocations. We also study a model where money is replaced by credit. When recommendations are used, allocations are very similar to those supported by monetary exchange, consistent with theory. In all other main treatments, trade is close to autarky. Importantly, these include a treatment where following the recommendation to produce is not incentive compatible, and then it is largely ignored even though subjects follow the incentive-compatible endgame recommendation. Hence, it appears that recommendations act as a coordinating device.

Keywords: Money, credit, experimental economics, coordination

JEL Classification numbers: E4, E5

1 Introduction

It is widely accepted that monetary exchange is an efficiency enhancing institution. By trading goods for money many transactions that cannot be implemented in a barter economy become feasible: in an economy with, say, bilateral meetings, monetary exchange allows for trades in situations where only one side can offer the other side a good of value (a single coincidence meeting), whereas barter requires a double coincidence of wants. In the absence of a centralized marketplace there is therefore a potential role for money. There are many formalizations of this idea (see Lagos, Rocheteau, and Wright 2017 or Rocheteau and Nosal 2017). This literature provides important insights about what other conditions are needed to theoretically justify the use of money. In particular, there must be a reason why credit arrangements cannot fulfill the same role as money, or why agents cannot achieve desirable outcomes in equilibria without money. The literature typically explains this from a combination of limited commitment and information frictions.

Jiang et al. (2024) ask whether use of money is sustainable and useful (essential in the language of the literature) in a laboratory setting for the reasons suggested by theory. Unlike previous experiments, which are based on infinite horizon models such as Kiyotaki and Wright (1989) or Lagos and Wright (2005), this is done using a finite horizon model. An advantage with a finite horizon setup is that infinite horizon models must be truncated in ways that make monetary exchange inconsistent with equilibrium to be able to finish the experiment in finite time with probability one. Typically this issue is addressed by "approximating" an infinite horizon by a combination of a random termination rule and a hard stop that occurs with a small probability. However, with rational agents and preferences assumed by the theory, backward induction implies that there is no monetary equilibrium. Hence, the experiments in Jiang et al. (2024) are arguably cleaner tests of whether money circulates for the reasons suggested by theory.

A problem for building on Jiang et al. (2024) to investigate more applied questions is that the agents in the model do not know their positions in the game. This makes it impossible to study effects of inflation as subjects could infer their position in the game from the price level. Subjects could then backward induct, and make monetary exchange incompatible

with equilibrium play.[1] Additionally, it would be very challenging to compare money and credit (in the sense of Kehoe and Levine 1993), as the punishments required to sustain credit become hard to support with uncertainty about the position.

In this paper we propose a complementary approach to implement finite horizon experiments on monetary exchange that does not require uncertainty of the position in the game, and use this to investigate how money compares to an institution that we interpret as credit. It builds on the seminal work by Benoît and Krishna (1985) who note that finitely repeated games and infinitely repeated games are qualitatively similar if the stage game has multiple non-Pareto ranked equilibria. Kovenock and De Vries (2002) use multiplicity in a similar way to show that fiat money can be valued with a finite horizon, and our model is closely related to theirs.

Concretely, we consider a two-period setup where players in period 1 engage in monetary exchange along the lines of Kiyotaki and Wright (1989); player 1 is equipped with intrinsically useless money and player 2 can produce a consumption good for player 1 at a cost. The consumption good is worth more to player 1 than the cost for player 2 to produce it, so it is efficient to produce the good. In period 2, player 2 can transfer the money to player 3 before playing a battle of the sexes game. This (static) game has an equilibrium that is strictly preferred by player 2 to another equilibrium, allowing for equilibria in the dynamic game where player 2 "buys" a better outcome in period 2 using money, which, for the right parameter values, justifies giving up something valuable for money in period 1 as an equilibrium.[2]

Some things should be noted. Firstly, the same construction works with any $T < \infty$ periods of monetary exchange before the endgame. We focus on a single exchange period ($T = 1$, or two periods in total) because a large number of studies show that the larger is T the more likely experimental subjects fail to backward induct. Secondly, the endgame is obviously not there because of realism, but it gives agents in period 1 continuation incentives analogous to

[1] Another issue with Jiang et al. (2024) is that some subjects used the perceived wait time to try to infer their position. This is not a problem in the setting studied in this paper.

[2] Cooper and Kühn (2014) use a similar construction in the context of repeated games. In the context of monetary economics, Davis et al. (2022) tried it experimentally, but results were negative.

those in an infinite horizon model without backward-induction unraveling.

We also run experiments without money. In some of these, histories are observable and in others they are not. The purpose of the experiments with unobservable histories is to use them as controls. In contrast, the experiments with observable histories are meant to see if it matters whether subjects engage in monetary exchange or (with no money) the trading history is public. The latter have equilibria that can be interpreted as credit. Moreover, in theory the two settings are outcome equivalent in our simple environment.[3]

In each case, player 2 has a choice of producing the consumption good for player 1 or not, followed by players 2 and 3 playing the battle of the sexes game. With unobservable histories, no Nash equilibrium has the good produced in period 1. This is because one cannot make endgame play contingent on behavior in period 1 when period 1 actions are unobservable.

With observable actions, endgame play can be made contingent on period 1 play, and subgame perfect equilibria emerge in which there is trade in period 1. Finally, money substitutes for observable histories as being able to transfer money lets player 2 certify past production, sustaining period 1 trading in a perfect Bayesian equilibrium. With two periods it would actually suffice to show the money, but with a longer horizon money would need to be transferred, so we also include a three-period robustness treatment with four players where money must change hands across two exchange meetings.

Coordination in games can be a challenge in laboratory settings, in particular with multiple equilibria. We therefore also run experiments where we recommend strategies. With money, the recommendation is to produce the consumption good in exchange for money in period 1, play the equilibrium favorable to player 2 in period 2 if money is offered, and play the equilibrium favorable to player 3 otherwise. This is a perfect Bayesian equilibrium. In the case when the trading history is public (credit), the recommendation is for player 2 to produce for player 1 in period 1, play the equilibrium favorable to player 2 in period 2 if the good was produced, and play the equilibrium favorable to player 3 otherwise. This is a

[3]This is not generally true. In many models money is an imperfect substitute for public histories. It is also possible in some models that money outperforms public histories/credit (a simple example is Jiang et al. 2024).

subgame perfect Nash equilibrium and supports the same allocation as the efficient monetary equilibrium. Finally, with no money and unobservable period 1 actions, we recommend that player 2 produce the consumption good for player 1 in period 1, and that the equilibrium favorable to player 2 be played in period 2. This is not even a Nash equilibrium as not producing is a profitable deviation for player 2. The reason why we recommend strategies also in this setting is to disentangle whether recommendations act as a coordination device or whether subjects are just blindly following them.

Our experimental results are very clean. Without money or public histories, exchange in period 1 is low and declines toward autarky with experience, and this holds true regardless of whether recommendations are provided or not. That is, subjects behave in accordance with theory in these treatments. Of particular importance is that the recommendation to produce in period 1, which is not incentive compatible, has no lasting effect beyond the earliest rounds. At the same time, these subjects follow the recommendation in period 2, where it is a Nash equilibrium, confirming that they ignore the period 1 recommendation selectively rather than out of inattention.

With money or with public histories, exchange in period 1 is similarly low and declines toward autarky in the treatments without strategy recommendations. This is also consistent with theory and not so surprising as the equilibria with no period 1 trade are arguably simpler than the ones with trade. It thus seems like subjects are unable to coordinate on an equilibrium that links behavior in the two periods when left to their own devices.

In contrast, when there is money or histories are public and we recommend that the good be produced in period 1, and players 2 and 3 play the equilibrium preferred by player 2 in period 2 if and only if money is transferred or period 1 production is observed, then most subjects follow the recommendation. In fact, although this seems like a more fragile strategy profile than the one used in Jiang et al. (2024), subjects now participate in monetary exchange at an even higher rate. Monetary exchange also carries over to our four-player, three-period robustness treatment.

Finally, we already noted that the model with money and the one with credit are equivalent in terms of outcomes. Again, our experimental results are consistent with theory as subjects

behave very similarly in the two settings.

2 Theory

Consider three versions of a two-period model with three agents, called player 1, player 2, and player 3. In period 1, players 1 and 2 meet pairwise: player 1 is a consumer and player 2 is a producer of an indivisible good. If player 2 produces the good for player 1, player 1 gets utility u and player 2 incurs a production cost c (for a period 1 payoff of $-c$). Given $u > c > 0$, production in period 1 is Pareto efficient.[4] Player 3 does not act in period 1.

In period 2, players 2 and 3 play a battle of the sexes game with payoff matrix

		Player 3	
		X	Y
Player 2	X	a, b	$0, 0$
	Y	$0, 0$	b, a

where $a > b > 0$. Players maximize the sum of their payoffs over the two periods, and payoffs and the structure of the game are common knowledge. Taken as a one-shot game, this endgame has two pure strategy Nash equilibria, (X, X) and (Y, Y), preferred by player 2 and player 3, respectively, and one mixed strategy equilibrium in which player 2 plays X with probability $\frac{a}{a+b}$ and player 3 plays X with probability $\frac{b}{a+b}$, yielding expected payoff $\frac{ab}{a+b} < b$ to each player. Notice why this endgame suits our purposes: player 2 can be rewarded or punished in period 2 by coordinating on (X, X) or (Y, Y), and because the two pure equilibria are not Pareto ranked, neither outcome is focal on efficiency grounds alone.

Where the three versions of the model differ is in whether there is money and whether period 1 actions are publicly observed.

No money and no public history. In the baseline version without money or observable histories, player 1 has no action in period 1, and player 2 simply chooses whether to produce

[4] Equivalently, the environment can be interpreted as an exchange economy in which player 2 is endowed with a good valued at c and can transfer it to player 1, who values it at u. We use the production interpretation throughout to match our experimental instructions.

for player 1. In period 2, players 2 and 3 simultaneously choose between X and Y without player 3 observing player 2's period 1 choice. Although the multiplicity of equilibria in the battle of the sexes generates multiple equilibria in the two-period game, no Nash equilibrium entails production in period 1, for an obvious reason: since player 3 does not observe what happened in period 1, period 2 play cannot be conditioned on whether player 2 produced. Bearing cost $c > 0$ in period 1 has no effect on period 2 payoffs, so player 2 never produces in equilibrium.

Money and no public history. In the version with money, period 1 actions remain unobservable to player 3, but player 1 is endowed with one unit of fiat money, an indivisible object with no intrinsic value. In period 1, player 1 chooses whether to offer money to player 2. After observing whether money is offered, player 2 chooses whether to produce for player 1, acquiring the money if and only if it is offered and player 2 produces (if no money is offered, player 2 can still produce, but gets nothing in return). In period 2, if player 2 holds money, player 2 chooses whether to offer it to player 3. Unlike in period 1, this is a unilateral transfer: player 3 makes no acceptance decision and simply observes whether money is offered before players 2 and 3 play the battle of the sexes game.

Because money is intrinsically useless, there always exist nonmonetary equilibria with the same outcomes as in the baseline version without money. Whenever period 2 strategies ignore whether money is offered (for instance, if players 2 and 3 play (X, X) after every history), player 2 should never bear cost $c > 0$ in period 1 just to acquire money.

Monetary equilibria arise when period 2 play is contingent on whether player 2 offers money. Even though player 3 does not observe period 1 directly, receiving money in period 2 reveals that player 1 offered it and player 2 produced in exchange for it (whereas if no money is offered in period 2, player 3 cannot tell whether player 1 withheld the money, player 2 chose not to produce, or player 2 kept the money). Now consider the strategy profile in which player 1 offers money in period 1; player 2 produces if and only if offered money, and offers money to player 3 in period 2 whenever holding it; and in period 2, players 2 and 3 play (X, X) if money is offered and (Y, Y) otherwise.

To check when this profile is an equilibrium, start in period 2. Because period 2 payoffs do

not depend directly on period 1 actions and both (X, X) and (Y, Y) are Nash equilibria of the battle of the sexes game, period 2 play is sequentially rational after any history, regardless of player 3's beliefs when no money is offered. Given this continuation play, player 2 gets a from offering money and b otherwise, so $a > b$ makes offering money in period 2 optimal whenever player 2 holds it. Moving back to period 1, if offered money, player 2 gets $a - c$ from producing and b from not producing (while if not offered money, not producing yields $b > b - c$). Hence, producing for money is optimal for player 2 if and only if $a - b \geq c$. Finally, since $u > 0$ and money has no intrinsic value, offering money in period 1 is optimal for player 1. Thus, if and only if $a - b \geq c$, this strategy profile is a perfect Bayesian (and subgame perfect) equilibrium with monetary exchange in period 1.[5]

No money and public history. The third version has no money, so as in the baseline version player 1 has no action in period 1 and player 2 simply chooses whether to produce for player 1. The difference is that player 3 publicly observes whether player 2 produced in period 1 before players 2 and 3 play the battle of the sexes game. Following Kehoe and Levine (1993) and Kocherlakota (1998), equilibria with period 1 production here can be interpreted as credit: player 2 produces for player 1 in period 1 in return for a public record of production that is rewarded in period 2.

As before, any autarkic equilibrium outcome of the baseline version remains an equilibrium if period 2 play ignores period 1 history. Conditioning period 2 play on the public history, however, supports period 1 production under the exact same parameter condition as with money: the profile in which player 2 produces in period 1, and players 2 and 3 play (X, X) if player 2 produced and (Y, Y) otherwise, is a subgame perfect Nash equilibrium if and only if $a - b \geq c$. More generally, unlike in Jiang et al. (2024), where public histories eliminate monetary equilibrium and money outperforms memory, money and public histories are outcome equivalent here: the set of perfect Bayesian equilibrium outcomes with money coincides with the set of subgame perfect equilibrium outcomes with public histories (see Appendix A.1).

[5] If the mixed strategy equilibrium of the battle of the sexes were played when money is not offered in period 2, period 1 trade could be supported under the weaker condition $a - \frac{ab}{a+b} \geq c$. Throughout, we focus on pure strategy play in the endgame.

Finally, as noted in the introduction, the monetary model extends to any finite number $T \geq 1$ of exchange periods before the endgame, with $T + 2$ players. In our experiments we also study $T = 2$, which has four players and three periods: player 1 meets player 2 in period 1, player 2 meets player 3 in a second exchange meeting in period 2 (where player 3 can produce a good worth u to player 2 at cost c), and players 3 and 4 play the battle of the sexes game in period 3 after player 3 chooses whether to offer money. Notice why $T = 2$ is an informative check: with $T = 1$, merely displaying money in the endgame would suffice in theory, whereas with $T \geq 2$ money must actually be transferred from player 2 to player 3 to sustain trade in both exchange meetings. Under the analogous monetary strategy profile, player 3 produces for money in period 2 whenever $a - b \geq c$, and anticipating that, player 2 produces for money in period 1 to buy consumption in period 2, which is optimal since $u > c$ (see Appendix A.2).

2.1 Hypotheses

All three versions of the model admit multiple autarkic equilibria associated with different equilibria of the battle of the sexes game in period 2. However, coordinating on an equilibrium that ties period 2 play to period 1 actions without communication, as necessary to sustain equilibria with period 1 production in the versions with money and with public histories, is not trivial in the lab, even when $a - b \geq c$.

In the spirit of mediation in Myerson (1991), we address this coordination issue by considering nonbinding strategy recommendations. With money, we recommend that player 1 offer money in period 1, that player 2 produce if and only if offered money and offer money to player 3 in period 2 if held, and that players 2 and 3 play (X, X) if money is offered and (Y, Y) otherwise. With public histories, we recommend that player 2 produce in period 1, and that players 2 and 3 play (X, X) if player 2 produced and (Y, Y) otherwise. In the baseline version without money or public histories, we recommend that player 2 produce in period 1 and that players 2 and 3 play (X, X) in period 2.

Notice how incentives differ across environments and periods when $a - b \geq c$: (1) with money or public histories, the entire recommended profile is subgame perfect (and perfect

Bayesian with money) and implements the efficient outcome; (2) in the baseline without money or public histories, the period 1 recommendation for player 2 to produce is not incentive compatible, even though it increases social surplus, because period 2 play does not depend on period 1 choices; and (3) in period 2, the recommended endgame actions form a Nash equilibrium in all three versions of the model, including (X, X) in the baseline. This design lets us disentangle whether recommendations coordinate equilibrium play or whether subjects follow them blindly (say, to please the experimenter, to avoid thinking through the game, because they trust the recommendation more than their own analysis, or to raise group payoffs), both across environments in period 1 and across periods 1 and 2 within the baseline itself.

To summarize the predictions of theory: because autarky is always an equilibrium, making past actions observable through money or public histories is necessary for equilibrium trade, while recommendations should help subjects coordinate on a productive equilibrium as in Jiang et al. (2024). Recommendations should also increase adherence when following them is incentive compatible (in period 1 with money or public histories, and in period 2 across all three environments) much more than when it is not (in period 1 of the baseline). Finally, because money and public histories are outcome equivalent, they should generate comparable period 1 trade and total group surplus. On the basis of the theory, we design our experiments to test three main hypotheses.

Hypothesis 1 *In period 1, production with money or public histories is at least as high as with neither, and strictly higher when strategy recommendations are provided.*

Hypothesis 2 *Strategy recommendations increase adherence when following the recommended actions is incentive compatible, and have a stronger effect than when following them is not.*

Hypothesis 3 *Period 1 production rates and realized group surplus are comparable in the versions with money and with public histories.*

3 Experimental Design

We now describe the key aspects of our design. Treatments cover each of the three versions of the model in Section 2, labeled B for the baseline with no money and no public history, M for money and no public history, and H for no money and public history, crossed with 0 (no strategy recommendations) and 1 (strategy recommendations) in our main three-player, two-period setup. In addition, we run a four-player, three-period treatment with money and recommendations, labeled M-1*, where money must circulate across two exchange meetings before the endgame. Table 1 summarizes all seven treatments.

Table 1: Overview of Experimental Treatments

Treatment	**Money**	**History**	**Recommendation**	**Number of Players**
B-0	No	No	No	3
B-1	No	No	Yes	3
M-0	Yes	No	No	3
M-1	Yes	No	Yes	3
H-0	No	Yes	No	3
H-1	No	Yes	Yes	3
M-1*	Yes	No	Yes	4

It is standard to have subjects play multiple rounds (15 in each session) so they can gain experience. To provide experience while minimizing repeated game effects, we randomly rematch subjects into new groups of three (or four in M-1*) in every round. At the start of a session, each subject is randomly assigned a role as player 1, 2, or 3 (or 4 in M-1*), which remains fixed for all 15 rounds.[6]

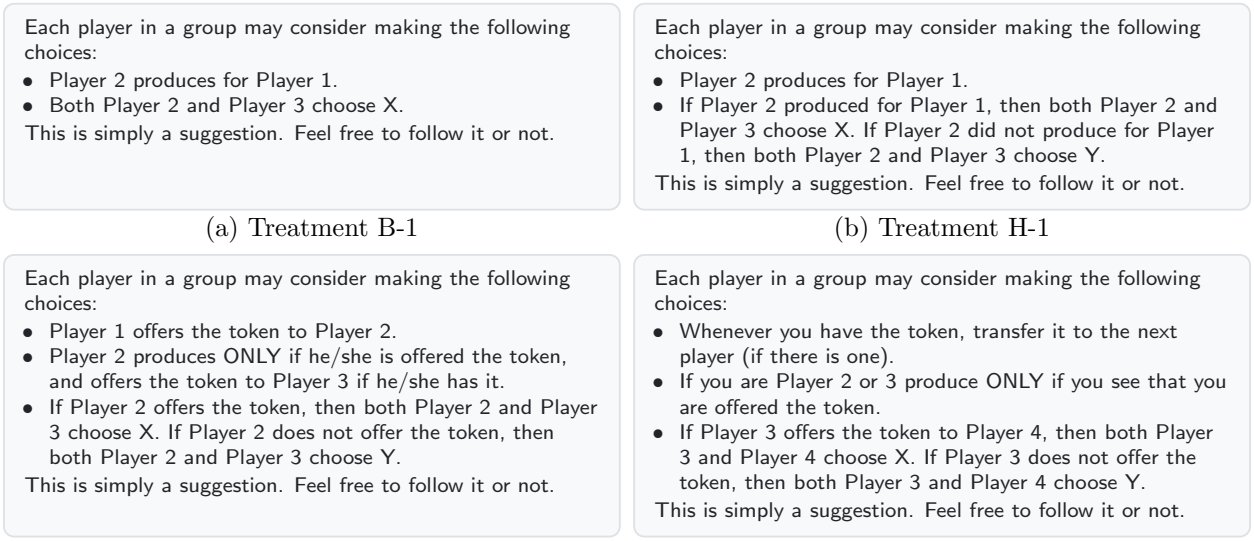
In the treatments with money (M-0, M-1, and M-1*), fiat money is called a "token" in the instructions so that monetary terminology does not prime subjects to accept it. Within each round, the sequence of moves follows Section 2: player 1 begins each round endowed with a token, each producer sees whether a token is offered before deciding whether to produce, and any player holding a token before the next meeting chooses whether to offer it to the next player.[7] The instructions and comprehension quiz state explicitly that tokens cannot

[6] Fixed roles further diminish repeated game incentives: if roles switched across rounds, a subject acting as player 2 might produce in hopes that others would reciprocate when acting as player 1 in a later round.

[7] To keep choice sets parallel across meetings and treatments, any player holding a token chooses whether to offer it to the next player, even in the three-player setup where merely showing it would suffice in theory.

be carried across rounds and have zero redemption value at the end of the session. In the treatments with public histories (H-0 and H-1), there are no tokens, and player 3 sees whether player 2 produced for player 1 in period 1 before players 2 and 3 choose between X and Y in period 2.

In the treatments with strategy recommendations (B-1, M-1, H-1, and M-1*), recommendations are framed neutrally in the instructions and on subjects' decision screens as "suggestions" that they are free to follow or ignore. Figure 1 shows the exact wording displayed on screen in each of these four treatments.



(a) Treatment B-1 (b) Treatment H-1

(c) Treatment M-1 (d) Treatment M-1*

Figure 1: Strategy Recommendations Across Treatments

In all treatments, payoffs in each exchange meeting are set to $u = 3$ points for the consumer (player 1 in period 1, and player 2 in period 2 of M-1*) and $c = 1$ point for the producer (giving -1 point in that meeting). In the battle of the sexes endgame, payoffs are $a = 3$ and $b = 1$ points, with mismatched choices giving 0.[8] Notice that $u = 3 > 1 = c$ and $a - b = 2 > 1 = c$, so efficiency and the equilibrium incentive condition in Section 2 hold strictly.

[8]In a related experiment, Davis et al. (2022) use a Hawk-Dove endgame for a similar purpose. We use a battle of the sexes game instead because coordinated outcomes are equilibria and mismatched choices yield zero rather than negative payoffs; in their design, playing Hawk yields a negative payoff if the other player also plays Hawk, which may discourage subjects from playing Hawk for reasons unrelated to equilibrium incentives.

From April 2024 to March 2026, we ran four in-person sessions per treatment at the Indiana University IELab in Bloomington, with 9 or 12 subjects per session in the three-player treatments and 12 subjects per session in M-1*, for a total of 315 subjects across 28 sessions. Each session has several parts. First, instructions are read aloud to establish common knowledge, followed by a comprehension quiz whose answers are reviewed before play begins. Next, subjects play 15 rounds of the main game. Subjects then complete an exit survey asking how they made their decisions, a Social Value Orientation (SVO) task consisting of generalized dictator games (Murphy, Ackermann, and Handgraaf, 2011) (the idea being that agents in the theory care only about their own payoffs, whereas subjects in the lab might also care about others), a demographic survey covering gender, age, English fluency, field of study, and qualitative risk tolerance, and a Cognitive Reflection Test (CRT).[9]

For the main game, subjects start with a 3-point endowment and three of the 15 rounds are randomly selected for payment, ensuring nonnegative point earnings (since the largest loss in any round is 1 point). Points convert to cash at \$2 per point. Including earnings from the SVO and CRT tasks and a \$5 show-up fee, subjects earned an average of \$24 for roughly one hour in the lab. Full instructions, quizzes, and post-game surveys are available in the Online Appendix.

4 Results

An overall finding is that period 1 production is sustained at high rates when subjects have both strategy recommendations and a way to condition on past play, whether money (M-1) or public histories (H-1), and is close to autarky otherwise. Table 2 and Figure 2 show period 1 production rates across the 15 rounds for the six main treatments. Without recommendations (B-0, M-0, and H-0), output starts low and declines toward autarky in all three settings. With recommendations, output holds steady across rounds with money (M-1, 0.940 overall) and with public histories (H-1, 0.758 overall), while in the baseline with

[9]We use a qualitative risk tolerance question because it is simple to administer and correlates well with behavior in incentivized risk tasks (see, e.g., Dohmen et al., 2011). As reported in Section 4, SVO, CRT, risk tolerance, and demographic controls have virtually no effect on our estimated treatment differences.

neither (B-1) it falls from 0.413 in the first five rounds to 0.200 in the last five, as subjects figure out that producing reduces their payoffs.

Table 2: Production Rate in Period 1

Treatment	Rounds			
	All	1–5	6–15	11–15
B-0	0.171	0.233	0.140	0.133
B-1	0.300	0.413	0.244	0.200
M-0	0.263	0.363	0.213	0.188
M-1	0.940	0.925	0.948	0.938
H-0	0.235	0.258	0.223	0.208
H-1	0.758	0.750	0.763	0.775

Notes: Entries report unweighted averages across the four sessions per treatment.

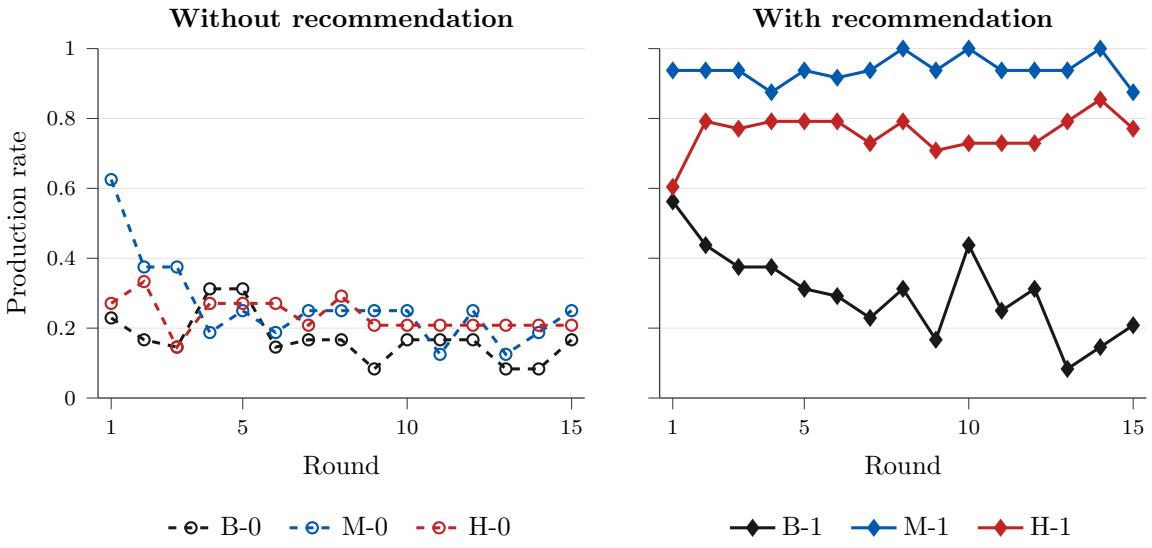


Figure 2: Production Rate in Period 1, by Strategy Recommendation

Below, Sections 4.1–4.3 test Hypotheses 1–3 in turn: Section 4.1 compares money and public histories against the baseline, Section 4.2 evaluates the effect of strategy recommendations on period 1 production across environments, and Section 4.3 compares production and group surplus between money and public histories. Section 4.4 then examines period 2 play to test Hypothesis 2 in the endgame and check whether realized payoffs make the productive strategy profitable. Finally, Section 4.5 studies the four-player treatment M-1*.

As in Jiang et al. (2024), our main nonparametric tests treat average behavior in each

session as one independent observation ($N = 4$ per treatment) and report exact two-sided Wilcoxon–Mann–Whitney (WMW) p-values for all rounds and for rounds 1–5, 6–15, and 11–15 to account for experience.[10] Appendices B–D complement these tests with session-clustered regressions and session-level breakdowns (covering Hypotheses 1–3 in Appendix B, endgame behavior in Appendix C, and M-1* in Appendix D). The parametric results yield the same conclusions while providing direct interaction tests for Hypothesis 2 and greater power when a single outlier session makes four-session rank tests conservative. As for the individual characteristics elicited after the game (Section 3), adding them to the regressions leaves treatment effects virtually unchanged: SVO, CRT, risk tolerance, English fluency, and field of study are all insignificant, confirming in particular that period 1 production is not driven by social preferences, while male subjects produce more on average and age has a small, marginally negative effect in later rounds. Appendices B.4 and C.2 analyze round-by-round learning dynamics and reinforcement, and Appendix C.3 reports supplementary unconditional endgame outcome and payoff tables.

4.1 Effects of Money and Public Histories on Period 1 Production

Consider first Hypothesis 1: that period 1 production with money or public histories is at least as high as in the baseline without either, and strictly higher when strategy recommendations are provided. Table 3 summarizes the results, holding the recommendation regime fixed.

Without strategy recommendations, introducing money or public histories does little to foster period 1 trade. Averaged over all rounds, production is 0.171 in B-0, 0.263 in M-0, and 0.235 in H-0, falling by rounds 11–15 to 0.133, 0.188, and 0.208. Notice that low trade in M-0 is not caused by player 1 withholding the token: offering the token is weakly dominant for

[10] In Sections 4.4 and 4.5, we use exact two-sided Wilcoxon signed-rank tests on within-session payoff differences to compare realized payoffs when subjects follow versus deviate from the productive equilibrium strategy. Pooling M and H under each recommendation regime in Section 4.4 gives up to eight independent session observations per role. Because M-1* in Section 4.5 has only four sessions (where the smallest possible two-sided exact signed-rank p-value is 0.125), we pool session-role averages across the two producer roles (players 2 and 3, up to eight session-role cells across the four sessions), while noting within-session dependence across roles and verifying that the mean payoff advantage is positive in all four sessions and confirmed at the subject level.

Table 3: Effect of Money and Public Histories on Production

	Average production rate						WMW p-value			
Rounds	B-0	M-0	H-0	B-1	M-1	H-1	B-0 vs. M-0	B-0 vs. H-0	B-1 vs. M-1	B-1 vs. H-1
All	0.171	0.263	0.235	0.300	0.940	0.758	0.486	0.686	0.029	0.114
1–5	0.233	0.363	0.258	0.413	0.925	0.750	0.314	0.629	0.257	0.257
6–15	0.140	0.213	0.223	0.244	0.948	0.763	0.886	0.886	0.029	0.114
11–15	0.133	0.188	0.208	0.200	0.938	0.775	0.771	0.686	0.029	0.086

Notes: Entries report unweighted session averages of period 1 production rates and exact two-sided WMW p-values ($N = 4$ sessions per treatment).

player 1, who offers it in 0.888 of meetings, yet player 2 produces in only 0.280 of meetings when offered a token (0.210 in rounds 11–15). From the p-values in Table 3, we cannot reject the null that output in M-0 or H-0 equals output in B-0 in any round segment. Consistent with the weak inequality in Hypothesis 1, subjects do not manage on their own to coordinate on strategies linking period 2 play to period 1 trade.

In contrast, with strategy recommendations, both money and public histories raise period 1 production substantially above the baseline. In B-1, production averages 0.300 over all rounds, starting at 0.413 in rounds 1–5 before dropping to 0.200 in rounds 11–15 as subjects in the role of player 2 learn that producing goes unrewarded. When past play is revealed through money or public histories, output stays high throughout the session: production holds steady at 0.940 over all rounds in M-1 (0.938 in rounds 11–15; player 1 offers the token in 0.992 of meetings overall and 1.000 after round 5, so production conditional on a token offer is 0.948) and at 0.758 in H-1 (0.775 in rounds 11–15). Because of initial production in B-1, differences are not yet significant in rounds 1–5 ($p = 0.257$). Once baseline production unravels, we reject equal output between M-1 and B-1 ($p = 0.029$ over all rounds and after round 5), and H-1 reaches nearly four times the B-1 rate by rounds 11–15 ($p = 0.086$, and $p < 0.010$ in the regressions of Appendix B.1). This provides clear support for Hypothesis 1.

Result 1 *Consistent with Hypothesis 1, period 1 production with money or public histories is close to the baseline without strategy recommendations, and substantially higher than in the baseline when strategy recommendations are provided.*

4.2 Effects of Strategy Recommendations on Period 1 Production

Now consider the period 1 prediction of Hypothesis 2: that recommendations have more impact when following them is incentive compatible (M and H) than when it is not (B). Table 4 compares period 1 production with and without recommendations within each institution.

Table 4: Effect of Strategy Recommendations on Production

	Average production rate						WMW p-value		
Rounds	B-0	B-1	M-0	M-1	H-0	H-1	B-0 vs. B-1	M-0 vs. M-1	H-0 vs. H-1
All	0.171	0.300	0.263	0.940	0.235	0.758	0.486	0.029	0.029
1–5	0.233	0.413	0.363	0.925	0.258	0.750	0.600	0.029	0.029
6–15	0.140	0.244	0.213	0.948	0.223	0.763	0.486	0.029	0.029
11–15	0.133	0.200	0.188	0.938	0.208	0.775	0.971	0.029	0.029

Notes: Entries report unweighted session averages of period 1 production rates and exact two-sided WMW p-values ($N = 4$ sessions per treatment).

In the baseline without money or public histories, producing in period 1 is not consistent with equilibrium. Recommendations raise early production somewhat in rounds 1–5 (0.233 in B-0 versus 0.413 in B-1), but the gap vanishes by rounds 11–15 (0.133 versus 0.200, $p = 0.971$). From the p-values in Table 4, we cannot reject the null that recommendations have no effect on period 1 production in the baseline ($p = 0.486$ over all rounds). As shown below in Section 4.4, these same subjects in B-1 follow the period 2 recommendation, where it is a Nash equilibrium, confirming that they ignore the period 1 recommendation selectively rather than out of inattention.

In contrast, when money or public histories make period 1 trade incentive compatible, recommendations generate large and persistent gains. Over all rounds, recommendations raise average production from 0.263 to 0.940 with money and from 0.235 to 0.758 with public histories, with both effects growing by rounds 11–15 (0.188 to 0.938 in M and 0.208 to 0.775 in H). From the p-values in Table 4, we reject the null that recommendations have no effect in every round interval for both M and H ($p = 0.029$; Appendix B.2 verifies that both gains significantly exceed the baseline effect). Notice also that while unmediated finite-horizon continuation games struggle to sustain monetary exchange, as in Davis et al. (2022) and our M-0 treatment, adding recommendations resolves the coordination problem and yields

even higher trade than in Jiang et al. (2024). We conclude that period 1 outcomes are improved by recommendations when they are consistent with equilibrium and not otherwise, even though following the baseline recommendation would raise group payoffs.

Result 2 *Consistent with Hypothesis 2, strategy recommendations strongly increase period 1 production with money or public histories, but have no lasting effect in the baseline with neither, indicating that recommendations serve as an equilibrium coordination device rather than inducing blind compliance.*

4.3 Money versus Public Histories

Now consider Hypothesis 3: that money and public histories (credit) yield comparable period 1 production and total group surplus, as implied by their theoretical outcome equivalence. Table 5 compares period 1 production across the two institutions. Without recommendations, output is similarly low in M-0 and H-0 (0.263 versus 0.235 over all rounds, and 0.188 versus 0.208 in rounds 11–15). With recommendations, both M-1 and H-1 sustain high exchange (0.940 versus 0.758 over all rounds, and 0.938 versus 0.775 in rounds 11–15). Although the point average is higher in M-1 than in H-1 (reflecting greater cross-session dispersion under public histories; see Appendix B.3), from the p-values in Table 5 we cannot reject equal production in any segment ($p = 0.886$ for M-0 versus H-0 and $p = 0.743$ for M-1 versus H-1 over all rounds).

Table 5: Production with Money versus Public Histories

	Average production rate				WMW p-value	
Rounds	M-0	H-0	M-1	H-1	M-0 vs. H-0	M-1 vs. H-1
All	0.263	0.235	0.940	0.758	0.886	0.743
1–5	0.363	0.258	0.925	0.750	0.200	0.400
6–15	0.213	0.223	0.948	0.763	1.000	1.000
11–15	0.188	0.208	0.938	0.775	0.971	0.829

Notes: Entries report unweighted session averages of period 1 production rates and exact two-sided WMW p-values ($N = 4$ sessions per treatment).

As for total surplus across both periods, recall that each three-player group generates $u - c = 2$ points from period 1 production and $a + b = 4$ points from coordinating on (X, X) or (Y, Y)

in period 2, so the efficient maximum is 6 points (4 points in coordinated autarky). Table 6 reports average group surplus per round. Without recommendations, frequent endgame miscoordination leaves surplus well short of 4 points in both M-0 and H-0 (2.408 versus 2.908 over all rounds, $p = 0.343$). With recommendations, slightly tighter endgame coordination in H-1 offsets its lower period 1 production, resulting in virtually identical welfare: 4.781 points in M-1 (0.797 of the 6-point maximum) versus 4.767 in H-1 (0.794 of the maximum; $p = 0.943$) over all rounds, and 4.825 versus 5.183 in rounds 11–15 ($p = 0.486$). Again, we cannot reject equality between money and credit in any round interval.

Table 6: Group Surplus with Money versus Public Histories

	Average group surplus				WMW p-value	
Rounds	M-0	H-0	M-1	H-1	M-0 vs. H-0	M-1 vs. H-1
All	2.408	2.908	4.781	4.767	0.343	0.943
1–5	2.175	2.617	5.083	4.217	0.343	0.114
6–15	2.525	3.054	4.629	5.042	0.343	0.686
11–15	2.475	3.200	4.825	5.183	0.171	0.486

Notes: Entries report unweighted session averages of three-player group surplus per round (out of an efficient maximum of 6 points) and exact two-sided WMW p-values ($N = 4$ sessions per treatment).

Result 3 *Consistent with Hypothesis 3, period 1 production rates and realized group surplus do not differ significantly between money and public histories either with or without strategy recommendations.*

4.4 The Endgame and Incentive Compatibility

We now turn to period 2 play in the battle of the sexes endgame. Unlike period 1 production, the recommended period 2 actions form a Nash equilibrium in all three environments: (X, X) unconditionally in B-1, and (X, X) after a token transfer or production and (Y, Y) otherwise in M-1 and H-1. Under Hypothesis 2, recommendations should therefore raise adherence in period 2 across B, M, and H alike.

Figure 3 plots the fraction of X choices over all rounds by player 2 (Panel A) and player 3 (Panel B), unconditionally in B-0 and B-1 and conditional on period 1 history in M and H. With recommendations, period 2 behavior lines up closely with the recommended

equilibrium: players 2 and 3 choose X at high rates in B-1 (0.988 and 0.874), in M-1 after a token offer (1.000 and 0.728), and in H-1 after production (0.978 and 0.853), while switching sharply toward Y when no token is offered in M-1 (X falls to 0.405 and 0.036 overall, and to 0.333 and 0.000 in rounds 11–15) and when player 2 does not produce in H-1 (0.603 and 0.022 overall, and 0.183 and 0.000 in rounds 11–15).

Without recommendations, play in B-0 (0.708 and 0.439) is close to the static mixed strategy equilibrium (0.750 and 0.250), while in M-0 and H-0 conditional coordination breaks down because of player 3. Since there is no reason to incur the period 1 production cost only to play Y, player 2 chooses X after transferring a token or producing almost universally across all four M and H treatments (0.882 to 1.000). In M-0 and H-0, player 2 also chooses X without a transfer or production (0.811 and 0.852) as a best response to player 3. Player 3, however, fails to provide dynamic incentives when there are no recommendations: (1) in M-0, player 3 fails to reward token offers, playing X at only 0.394 when offered a token (versus 0.728 in M-1 and 0.761 in H-0, both $p = 0.029$) while playing X at 0.548 without one ($p = 0.029$ versus M-1); (2) in H-0, player 3 rewards observed production (0.761 X overall and 0.867 in rounds 11–15; $p = 0.343$ overall versus H-1), but fails to punish non-production, still playing X at 0.547 overall and 0.663 in rounds 11–15 ($p = 0.057$ overall versus H-1).

Why does player 3 fail to reward token offers in M-0 even though X is player 3's empirical best reply to player 2's 0.882 X play? Three factors may contribute: (1) because player 2 chooses X almost as often without transferring a token (0.811), the observed link between token offers and X is weak; (2) player 3 may misread a token offer as a gesture of goodwill signaling that player 2 will concede to Y; or (3) player 3 may simply fail to condition period 2 play on period 1 actions, consistent with experimental evidence on failures of contingent reasoning (Niederle and Vespa, 2023; Park, 2025).[11]

Table 7 tests Hypothesis 2 in period 2 by comparing adherence to the recommended endgame

[11]Notice also why period 1 production stays low in H-0 even though player 2 is rewarded with (X, X) after producing more frequently under public histories than under money (both without and with recommendations): what governs the incentive to produce is the payoff differential between producing and not producing. Strategy recommendations operate on both margins, raising the payoff from trading (more (X, X) after a token offer or production) while lowering the payoff from not trading (shifting non-trade outcomes from (X, X) toward (Y, Y)).

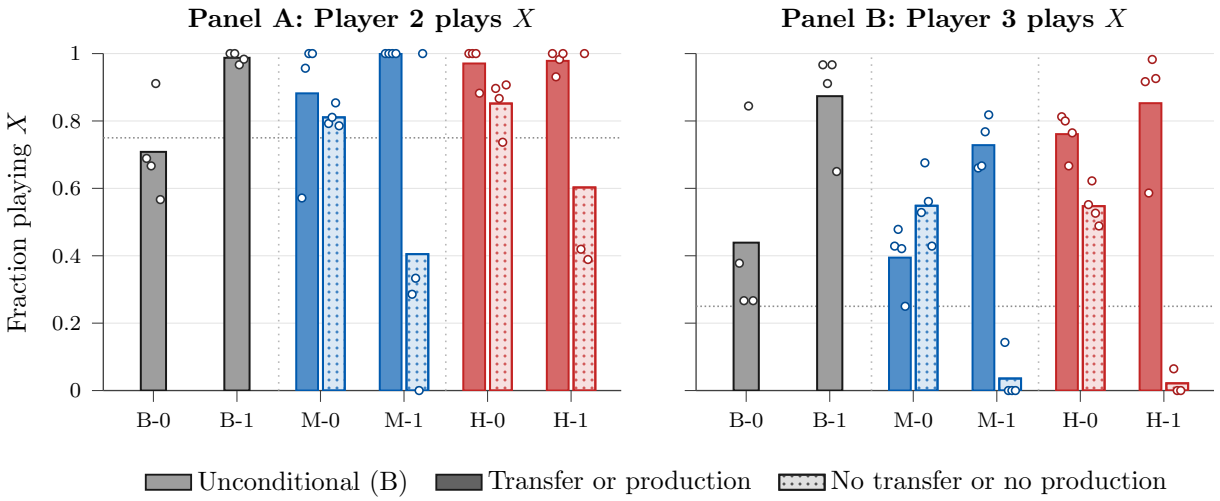


Figure 3: Period 2 X Choices by Role Across All Six Main Treatments

Notes: Bars report unweighted session averages over all 15 rounds: unconditional in B-0 and B-1 (gray) and conditional on a token transfer or period 1 production (dark) versus no transfer or no production (light, dotted) in M and H. Circles show individual session averages ($N = 4$ sessions per cell, except $N = 3$ for no production in H-1 because one session had 1.000 production in all rounds, making $p = 0.057$ the exact two-sided minimum for four versus three sessions). Dotted horizontal lines mark static mixed-equilibrium probabilities (0.750 for player 2 and 0.250 for player 3).

strategy with and without recommendations. In the baseline, X play rises from 0.708 in B-0 to 0.988 in B-1 for player 2 ($p = 0.029$, reaching 1.000 in rounds 11–15, and 0.985 overall right after not producing in period 1) and from 0.439 to 0.874 for player 3 ($p = 0.057$, reaching 0.933 in rounds 11–15). Thus, within B-1 itself, the same subjects in the role of player 2 who reject the non-equilibrium period 1 recommendation follow the equilibrium period 2 recommendation almost universally. With money and public histories, conditional adherence (X after a transfer or production and Y otherwise) rises over all rounds from 0.354 to 0.982 for player 2 and from 0.458 to 0.743 for player 3 in M, and from 0.347 to 0.882 and from 0.532 to 0.906 in H. From the p-values in Table 7, we reject the null of no recommendation effect at $p = 0.029$ in every round interval for both roles in M and H (after round 5, player 3's adherence is also higher in H-1 than in M-1, $p = 0.029$, explaining why group surplus is equal in Section 4.3).

Finally, we check whether realized endgame behavior makes playing the productive equilibrium strategy profitable. In B-0 and B-1, producing in period 1 is never profitable because period 2 play cannot depend on it. For M and H, which are outcome equivalent in theory and

Table 7: Adherence to the Recommended Period 2 Strategy by Role

		Baseline			Money			Public histories		
Role	Rounds	B-0	B-1	WMW p	M-0	M-1	WMW p	H-0	H-1	WMW p
Player 2	All	0.708	0.988	0.029	0.354	0.982	0.029	0.347	0.882	0.029
	1–5	0.775	0.975	0.057	0.425	0.988	0.029	0.417	0.838	0.029
	6–15	0.675	0.994	0.029	0.319	0.979	0.029	0.313	0.904	0.029
	11–15	0.688	1.000	0.029	0.275	0.975	0.029	0.233	0.946	0.029
Player 3	All	0.439	0.874	0.057	0.458	0.743	0.029	0.532	0.906	0.029
	1–5	0.433	0.804	0.057	0.513	0.821	0.029	0.575	0.817	0.029
	6–15	0.442	0.908	0.057	0.431	0.704	0.029	0.510	0.950	0.029
	11–15	0.433	0.933	0.057	0.350	0.763	0.029	0.454	0.963	0.029

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) of the fraction of period 2 meetings matching the recommended endgame strategy: playing X in B, and playing X after a token transfer (M) or period 1 production (H) and Y otherwise. In M-1, player 2 transfers an acquired token in 210 of 211 cases (0.995). WMW p reports exact two-sided p-values.

empirically (Result 3), Table 8 pools M-0 with H-0 and M-1 with H-1 and compares realized payoffs when subjects follow versus deviate from the productive equilibrium strategy: player 2's full two-period strategy (net of the 1-point production cost) and player 3's conditional period 2 strategy. Without recommendations, missing rewards in M-0 and missing punishments in H-0 mean that following the productive strategy does not pay: over all rounds, player 2 earns strictly less by following than by deviating (0.660 versus 1.378, signed-rank $p = 0.023$), and player 3 earns no more (0.654 versus 0.704, $p = 0.688$, and strictly less in rounds 11–15, 0.410 versus 0.808, $p = 0.008$). In contrast, with recommendations, following pays substantially more than deviating in every session where deviations occur: over all rounds, player 2 earns 1.388 versus 0.400 ($p = 0.016$) and player 3 earns 1.140 versus 0.063 ($p = 0.008$), with large advantages persisting in rounds 11–15 (1.500 versus 0.356, $p = 0.031$, and 1.222 versus 0.000, two-sided $p = 0.063$, one-sided $p = 0.031$; Table 24 in Appendix C.2 confirms $p < 0.001$ in M-1 and H-1 separately).

Table 8: Realized Payoffs from Following versus Not Following the Recommended Strategy

		Without recommendations (M-0 & H-0)			With recommendations (M-1 & H-1)		
Role	Rounds	Followed	Not followed	Signed-rank p	Followed	Not followed	Signed-rank p
Player 2	All	0.660	1.378	0.023	1.388	0.400	0.016
	1–5	0.510	0.965	0.086	1.365	0.427	0.063
	6–15	0.870	1.574	0.063	1.396	0.320	0.016
	11–15	0.714	1.820	0.063	1.500	0.356	0.031
Player 3	All	0.654	0.704	0.688	1.140	0.063	0.008
	1–5	0.637	0.562	0.609	1.076	0.063	0.008
	6–15	0.665	0.763	0.461	1.168	0.100	0.063
	11–15	0.410	0.808	0.008	1.222	0.000	0.063

Notes: Entries report unweighted session averages (across sessions with at least one observation in that cell) pooled across M-0 and H-0 and across M-1 and H-1, where "Followed" denotes choices matching the productive equilibrium strategy recommended in M-1 and H-1 (full two-period strategy and net round payoffs for player 2; conditional period 2 strategy and period 2 payoffs for player 3). Signed-rank p reports exact two-sided Wilcoxon signed-rank p-values on within-session payoff differences ($N = 8, 8, 6, 6$ sessions with both cells for player 2 and $N = 8, 8, 8, 8$ for player 3 in M-0 & H-0; $N = 7, 5, 7, 6$ for player 2 and $N = 8, 8, 5, 5$ for player 3 in M-1 & H-1, where 0.063 is the exact two-sided minimum for $N = 5$ and one-sided $p = 0.031$).

Result 4 *Consistent with Hypothesis 2, subjects in period 2 follow the recommended Nash equilibrium actions across B-1, M-1, and H-1, making the recommended strategy strictly more profitable than deviating for both player 2 and player 3 under money and public histories.*

4.5 Treatment with Four Players

We now turn to treatment M-1*, which has four players and two exchange meetings before the period 3 battle of the sexes endgame ($T = 2$). Because player 2 never enters the endgame, player 2 accepts a token in period 1 only to spend it in period 2, providing a direct check on monetary circulation across exchange meetings. Figure 4 and Table 9 compare period 1 and period 2 production in M-1* with M-1 and B-1.

Monetary exchange extends cleanly to two exchange periods. In period 1 of M-1*, production averages 0.939 over all rounds (0.967 in rounds 11–15), matching M-1 (0.940, $p = 1.000$) and far exceeding B-1 (0.300, $p = 0.029$), while tokens are offered nearly whenever held (1.000, 0.953, and 0.980 by players 1, 2, and 3). In period 2 of M-1*, production averages 0.656 unconditionally and 0.723 conditional on a token offer, rising in rounds 11–15 to 0.700

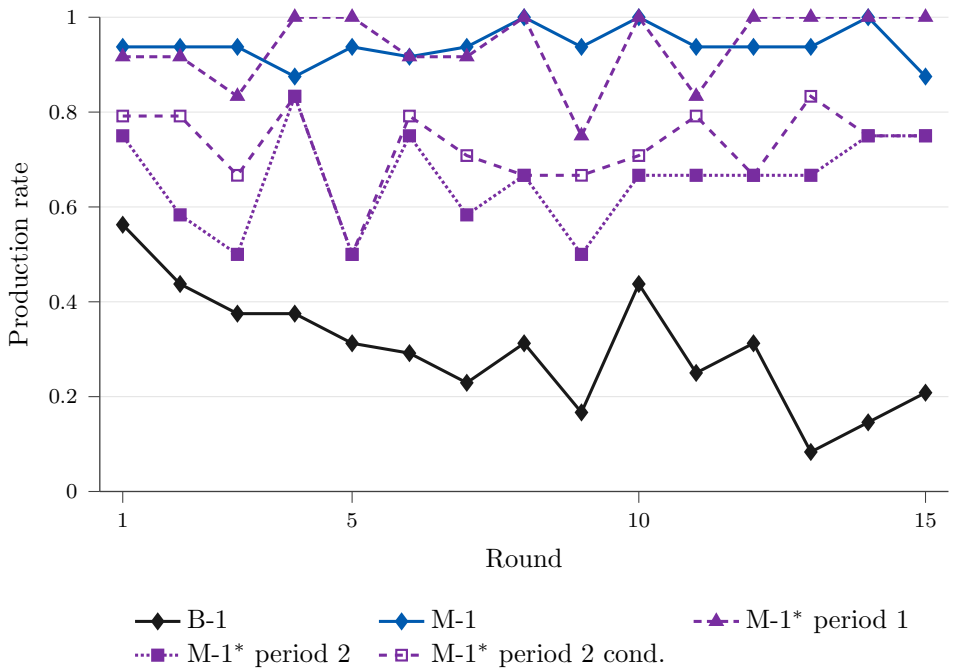


Figure 4: Production Rates in M-1* versus B-1 and M-1

Table 9: Production Rates in M-1* versus B-1 and M-1

	Average production rate					WMW p-value				
Rounds	B-1	M-1	M-1* period 1	M-1* period 2	M-1* period 2 cond.	M-1 vs. M-1* period 1	M-1 vs. M-1* period 2	M-1 vs. M-1* period 2 cond.	B-1 vs. M-1* period 1	B-1 vs. M-1* period 2
All	0.300	0.940	0.939	0.656	0.723	1.000	0.029	0.029	0.029	0.171
1–5	0.413	0.925	0.933	0.633	0.714	0.829	0.057	0.057	0.200	0.257
6–15	0.244	0.948	0.942	0.667	0.727	1.000	0.029	0.057	0.029	0.171
11–15	0.200	0.938	0.967	0.700	0.745	0.400	0.257	0.257	0.029	0.086

Notes: Entries report unweighted session averages of production rates ($N = 4$ sessions per treatment) and exact two-sided WMW p-values. "M-1* period 2 cond." conditions on player 2 offering the token to player 3 (and "M-1 vs. M-1* period 2 cond." conditions M-1 on player 1 offering the token to player 2).

(0.745 conditional), where it exceeds B-1 (0.200, $p = 0.086$; $p = 0.024$ over all rounds in Appendix D) and is not significantly different from M-1 ($p = 0.257$). Notice that the lower overall period 2 rate relative to M-1 ($p = 0.029$) is driven by just three of the twelve subjects in the role of player 3 who opted out of monetary exchange, whereas the other nine average 0.852 unconditional and 0.937 conditional production.[12] Across all three periods, four-player

[12]In exit surveys, these three subjects in the role of player 3 reported ignoring the recommendations from the start (session 2), preferring a safe 1-point payoff from playing Y over risking miscoordination with player 4 (session 3), or stopping production after round 2 following early miscoordination with player 4 (session 4).

groups realize 6.722 out of 8 points of surplus (0.840 of the efficient maximum, rising to 7.000 in rounds 11–15), beating the 4-point coordinated-autarky benchmark in every session.

Table 10 checks realized payoff incentives for players 2, 3, and 4 in M-1*. Over all rounds, following the recommended strategy rather than deviating pays 1.168 versus −0.254 for player 2, 1.689 versus 0.852 for player 3, and 1.437 versus 0.271 for player 4 (who deviates in only one session after round 5), with substantial payoff gains in every round interval. Pooling session-role observations across the two producer roles (players 2 and 3) in the rightmost column, we reject equal payoffs from following and deviating in every segment ($p = 0.016$ over all rounds and $p = 0.031$ in rounds 11–15; within each session, the mean payoff gap across players 2 and 3 is also positive in all four sessions in every interval).

Table 10: Realized Payoffs from Following versus Not Following the Recommended Strategy in Treatment M-1*

	Player 2		Player 3		Player 4		Pooled P2 & P3
Rounds	Followed	Not followed	Followed	Not followed	Followed	Not followed	Signed-rank p
All	1.168	−0.254	1.689	0.852	1.437	0.271	0.016
1–5	1.143	−0.500	1.266	0.729	1.514	0.222	0.039
6–15	1.180	−0.067	1.893	0.937	1.404	0.900	0.008
11–15	1.236	−0.333	1.927	0.492	1.333	0.667	0.031

Notes: Entries report unweighted session averages across sessions (out of four in M-1*) with at least one observation in that cell: net round payoffs for player 2 and player 3, and period 3 endgame payoffs for player 4. "Not followed" averages across four sessions in every cell except rounds 11–15 for player 2 ($N = 2$) and all rounds, rounds 1–5 ($N = 3$), and rounds 6–15 and 11–15 ($N = 1$) for player 4, where remaining sessions had zero deviations. The rightmost column reports exact two-sided Wilcoxon signed-rank p-values on within-session-role payoff differences pooling players 2 and 3 ($N = 8, 8, 8, 6$ session-role cells; one-sided $p = 0.008, 0.020, 0.004, 0.016$; subject-level two-sided signed-rank $p = 0.007, 0.089, 0.004, 0.016$ across $N = 18, 13, 12, 9$ subjects with both cells).

Result 5 *With four players and two exchange periods before the endgame, monetary exchange remains near-universal in period 1 and substantial in period 2, supported by higher realized payoffs from following the recommended strategy across all active roles.*

5 Conclusion

This paper studied, theoretically and experimentally, models of exchange in which an endgame with multiple equilibria sustains valued fiat money and credit (public histories) over a finite horizon without uncertainty about players' positions in the game. With strategy recommendations, both money and public histories generated high and stable period 1 production as well as comparable group surplus, consistent with their theoretical outcome equivalence. Without recommendations, or in the baseline without money or public histories, period 1 production was low and declined toward autarky with experience, also consistent with theory. These results provide evidence that subjects produce in period 1 for strategic reasons: player 2 incurs a production cost today when acquiring a token or establishing a public record of production is expected to be rewarded in the endgame. Controlling for SVO scores and demographic characteristics leaves these treatment effects unchanged, and SVO has no significant effect on production, consistent with strategic incentives rather than social preferences.

Another finding is that strategy recommendations improved outcomes when following them was incentive compatible and had no lasting effect on period 1 production in the baseline, while those same baseline subjects followed the Nash equilibrium recommendation in period 2. This implies that the impact of recommendations does not come from blind compliance but from coordinating on equilibrium play. Yet another result is that without recommendations, dynamic incentives failed in the battle of the sexes endgame because player 3 did not reward token offers under money or punish non-production under public histories, whereas with recommendations following the productive strategy strictly paid. We also found that monetary exchange carried over cleanly to our four-player, three-period treatment, M-1*, where tokens had to change hands across two exchange meetings before the endgame. Non-production in period 2 of M-1* was concentrated among a few player 3 subjects who, on the basis of exit surveys, ignored the recommendations, preferred a safe endgame payoff, or stopped producing after early miscoordination.

In terms of extensions, one idea is to study inflation and monetary policy, which is feasible here because players know their positions in the game. Unlike in Jiang et al. (2024),

changing prices over time no longer unravel monetary equilibrium by revealing the period, especially once money or goods are made divisible so that the terms of trade are determined endogenously. Another is to consider environments with imperfect record-keeping, larger populations, or longer horizons, where money and credit are no longer outcome equivalent and imperfect monitoring can make fiat money outperform credit. Finally, since subjects rarely coordinated dynamic incentives on their own, it would be interesting to study alternative ways to coordinate play beyond exogenous strategy recommendations, such as preplay communication or endogenously emerging norms.

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A Equilibrium Characterization and Proofs

Section 2 focused on the pure strategy equilibria implemented by our strategy recommendations. This appendix provides complete equilibrium characterizations and formal proofs for all theoretical claims in Section 2. Appendix A.1 characterizes the full set of Nash, subgame perfect, and perfect Bayesian equilibrium outcomes (allowing both pure and mixed play) across the three versions of the two-period model, and proves outcome equivalence between money and public histories even though player 3 observes only whether money is offered in period 2. Appendix A.2 formalizes the monetary model with any finite number $T \geq 1$ of exchange periods before the endgame, proves when the monetary strategy profile is an equilibrium, and shows why any horizon $T \geq 2$ unravels if money can only be displayed rather than transferred.

A.1 Two-Period Models and Outcome Equivalence

Recall from Section 2 that $u > c > 0$ and $a > b > 0$. Let Σ_{BS} denote the set of static Nash equilibria of the period 2 battle of the sexes game: the two pure strategy equilibria (X, X) and (Y, Y), and the mixed strategy equilibrium σ^M in which player 2 plays X with probability $\frac{a}{a+b}$ and player 3 plays X with probability $\frac{b}{a+b}$. Each $\sigma \in \Sigma_{\text{BS}}$ induces a distribution over period 2 action profiles $(s_2, s_3) \in \{X, Y\}^2$ and gives player 2 expected period 2 payoff $V_2(\sigma) \in \left\{a,\, b,\, \frac{ab}{a+b}\right\}$, where $a > b > \frac{ab}{a+b} > 0$.

To fix notation across the three versions of the two-period model, let $q_1 \in \{0, 1\}$ denote whether player 2 produces for player 1 in period 1. Without money, player 1 has no action. With money, player 1 begins with one unit of fiat money and chooses whether to offer it to player 2, $m_1 \in \{0, 1\}$; after observing m_1, player 2 chooses $q_1 \in \{0, 1\}$ and enters period 2 with money holding $h_2 = m_1 q_1 \in \{0, 1\}$. At the start of period 2 in the version with money, player 2 chooses whether to offer money to player 3, $m_2 \in \{0, 1\}$, whenever $h_2 = 1$ (with $m_2 = 0$ if $h_2 = 0$). Players 2 and 3 then simultaneously choose $(s_2, s_3) \in \{X, Y\}^2$. Before choosing s_3, player 3 observes nothing in the baseline, observes q_1 under public histories, and observes only m_2 under money (not player 2's private history (m_1, q_1, m_2)). Because fiat money is intrinsically useless, payoffs in all three versions depend only on (q_1, s_2, s_3): player

1 gets uq_1, player 2 gets $-cq_1$ plus the battle of the sexes payoff from (s_2, s_3), and player 3 gets the battle of the sexes payoff from (s_2, s_3). Accordingly, an *outcome* of the game is a joint probability distribution over $(q_1, s_2, s_3) \in \{0, 1\} \times \{X, Y\}^2$.

Proposition 1 *In the two-period model with $u > c > 0$ and $a > b > 0$:*

1. *No money and no public history: Every Nash equilibrium has $q_1 = 0$ with probability 1, and the set of Nash, subgame perfect equilibrium (SPE), and perfect Bayesian equilibrium (PBE) outcomes consists of $q_1 = 0$ paired with any $\sigma \in \Sigma_{BS}$.*

2. *No money and public history: The set of SPE outcomes consists of all distributions parameterized by a period 1 production probability $p = \Pr(q_1 = 1) \in [0, 1]$ and period 2 equilibria $\sigma(1), \sigma(0) \in \Sigma_{BS}$ such that:*

   (a) $p = 0$ and $(s_2, s_3) \sim \sigma(0)$ for any $\sigma(0) \in \Sigma_{BS}$;

   (b) $p = 1$ and $(s_2, s_3) \sim \sigma(1)$ whenever $V_2(\sigma(1)) - V_2(\sigma(0)) \geq c$ for some $\sigma(0) \in \Sigma_{BS}$ (with pure period 2 strategies, $(q_1 = 1, X, X)$ if and only if $a - b \geq c$; allowing mixed period 2 play, $(q_1 = 1, X, X)$ if and only if $\frac{a^2}{a+b} \geq c$ and $(q_1 = 1, Y, Y)$ if and only if $\frac{b^2}{a+b} \geq c$);

   (c) $p \in (0, 1)$, with $(s_2, s_3) \mid (q_1 = 1) \sim \sigma(1)$ and $(s_2, s_3) \mid (q_1 = 0) \sim \sigma(0)$, whenever $V_2(\sigma(1)) - V_2(\sigma(0)) = c$.

3. *Money and no public history (Outcome Equivalence): The set of PBE outcomes with money coincides with the set of SPE outcomes with public histories, both across all behavioral strategies and when period 2 play is restricted to pure strategies.*

Proof. Part 1. Without money or observable histories, player 3 has a single period 2 information set, so player 3's probability of playing X, $\sigma_3 \in [0, 1]$, cannot depend on q_1. Against any σ_3, player 2's maximum period 2 payoff is $\bar{v}_2(\sigma_3) = \max\{a\sigma_3, b(1 - \sigma_3)\}$ regardless of q_1. Any strategy for player 2 with $p = \Pr(q_1 = 1) > 0$ yields at most $\bar{v}_2(\sigma_3) - pc < \bar{v}_2(\sigma_3)$, whereas choosing $q_1 = 0$ and playing a period 2 best response yields $\bar{v}_2(\sigma_3)$. Thus every Nash equilibrium has $p = 0$. Because $q_1 = 0$ occurs with probability 1, player 2's period 2 choice at $q_1 = 0$ and player 3's choice σ_3 must be mutual best responses in the battle of the

sexes game, so $(s_2, s_3) \sim \sigma$ for some $\sigma \in \Sigma_{\text{BS}}$. Conversely, for any $\sigma \in \Sigma_{\text{BS}}$, setting $q_1 = 0$ and playing σ after both $q_1 = 0$ and $q_1 = 1$ forms a Nash equilibrium, SPE, and PBE with outcome $(0, \sigma)$.

Part 2. With public histories, each $q_1 \in \{0, 1\}$ initiates a proper period 2 subgame isomorphic to the static battle of the sexes game, so subgame perfection requires $\sigma(q_1) \in \Sigma_{\text{BS}}$ for each $q_1 \in \{0, 1\}$. Anticipating this continuation play, player 2 gets $-c + V_2(\sigma(1))$ from $q_1 = 1$ and $V_2(\sigma(0))$ from $q_1 = 0$. Hence $p = 0$ is optimal if and only if $V_2(\sigma(1)) - V_2(\sigma(0)) \leq c$, which holds for any $\sigma(0) \in \Sigma_{\text{BS}}$ by setting $\sigma(1) = \sigma(0)$ since $c > 0$; $p = 1$ is optimal if and only if $V_2(\sigma(1)) - V_2(\sigma(0)) \geq c$; and $p \in (0, 1)$ is optimal if and only if $V_2(\sigma(1)) - V_2(\sigma(0)) = c$. Substituting $V_2(\sigma) \in \left\{a,\, b,\, \frac{ab}{a+b}\right\}$ gives the parameter thresholds in (b): with pure period 2 play, only $\sigma(1) = (X, X)$ and $\sigma(0) = (Y, Y)$ satisfy $V_2(\sigma(1)) > V_2(\sigma(0))$, yielding $a - b \geq c$; allowing $\sigma(0) = \sigma^M$ yields $a - \frac{ab}{a+b} = \frac{a^2}{a+b} \geq c$ for $\sigma(1) = (X, X)$ and $b - \frac{ab}{a+b} = \frac{b^2}{a+b} \geq c$ for $\sigma(1) = (Y, Y)$, while $\sigma(1) = \sigma^M$ cannot sustain trade because $\frac{ab}{a+b}$ is the minimum payoff in Σ_{BS}.

Part 3. In the version with money, write behavioral strategies as $\mu_1 = \Pr(m_1 = 1)$ for player 1; $\pi_2(m_1) = \Pr(q_1 = 1 \mid m_1)$ in period 1 and $\tau_2 = \Pr(m_2 = 1 \mid h_2 = 1)$ at the start of period 2 for player 2; $\sigma_2(\omega_2) = \Pr(s_2 = X \mid \omega_2)$ for player 2 at each private history $\omega_2 = (m_1, q_1, m_2)$; and $\sigma_3(m_2) = \Pr(s_3 = X \mid m_2)$ for player 3 after observing $m_2 \in \{0, 1\}$.

First, take any SPE $(p, \sigma(1), \sigma(0))$ under public histories. If $p = 0$, set $\mu_1 = 0$, $\pi_2(1) = \pi_2(0) = 0$, and $\tau_2 = 0$, and let players 2 and 3 play $\sigma(0) \in \Sigma_{\text{BS}}$ after every history (for both $m_2 = 1$ and $m_2 = 0$, with player 3's belief placing probability 1 on $(0, 0, 0)$ at $m_2 = 0$ and on $(1, 1, 1)$ at $m_2 = 1$). Because period 2 continuation payoffs equal $V_2(\sigma(0))$ after every history, $\tau_2 = 0$, $\pi_2(1) = \pi_2(0) = 0$, and $\mu_1 = 0$ are sequentially rational, giving a PBE with outcome $(0, \sigma(0))$. If $p > 0$, Part 2 implies $V_2(\sigma(1)) - V_2(\sigma(0)) \geq c > 0$, with equality if $p \in (0, 1)$. Set $\mu_1 = 1$, $\pi_2(1) = p$, $\pi_2(0) = 0$, and $\tau_2 = 1$, and let players 2 and 3 play $\sigma(1)$ whenever $m_2 = 1$ and $\sigma(0)$ whenever $m_2 = 0$, with player 3's belief placing probability 1 on $(1, 1, 1)$ at $m_2 = 1$ and on $(1, 0, 0)$ at $m_2 = 0$ (which follows Bayes' rule when $p < 1$ and specifies a valid off-path belief when $p = 1$). To check sequential rationality, start in period 2: playing $\sigma(1)$ at $m_2 = 1$ and $\sigma(0)$ at $m_2 = 0$ is sequentially rational because both belong to Σ_{BS}, and when $h_2 = 1$,

offering money is strictly optimal for player 2 because $V_2(\sigma(1)) > V_2(\sigma(0))$. Moving back to period 1, $\pi_2(0) = 0$ is strictly optimal ($V_2(\sigma(0)) > -c + V_2(\sigma(0))$), $\pi_2(1) = p$ is optimal since $-c + V_2(\sigma(1)) \geq V_2(\sigma(0))$ (with equality if $p \in (0, 1)$), and $\mu_1 = 1$ is strictly optimal for player 1 since $pu > 0$. On the equilibrium path, $m_1 = 1$ and $m_2 = q_1$ with probability 1, so this PBE induces the same outcome over (q_1, s_2, s_3) as the SPE under public histories (and uses pure period 2 strategies whenever $\sigma(1)$ and $\sigma(0)$ are pure).

Conversely, take any PBE of the version with money. Because player 3 observes only $m_2 \in \{0, 1\}$ rather than $\omega_2 = (m_1, q_1, m_2)$, period 2 does not form a proper subgame; we first show that player 2's period 2 continuation payoff still depends only on m_2 and lies in $\left\{a, b, \frac{ab}{a+b}\right\}$. Fix $m_2 \in \{0, 1\}$ and let $\bar{\sigma}_2(m_2)$ denote the probability that player 2 plays X under player 3's belief at m_2. At any history $\omega_2 = (m_1, q_1, m_2)$, player 2 gets $a\sigma_3(m_2)$ from X and $b(1 - \sigma_3(m_2))$ from Y, regardless of (m_1, q_1), while player 3 gets $b\bar{\sigma}_2(m_2)$ from X and $a(1 - \bar{\sigma}_2(m_2))$ from Y. Consider the three cases for $\sigma_3(m_2)$: (1) if $\sigma_3(m_2) > \frac{b}{a+b}$, then X is strictly optimal for player 2 at every ω_2 with that m_2 ($\sigma_2(\omega_2) = 1$), so $\bar{\sigma}_2(m_2) = 1$ for any belief of player 3, forcing $\sigma_3(m_2) = 1$ and giving player 2 continuation payoff $w(m_2) = a$; (2) if $\sigma_3(m_2) < \frac{b}{a+b}$, then Y is strictly optimal at every ω_2 with that m_2 ($\sigma_2(\omega_2) = 0$), so $\bar{\sigma}_2(m_2) = 0$, forcing $\sigma_3(m_2) = 0$ and $w(m_2) = b$; and (3) if $\sigma_3(m_2) = \frac{b}{a+b}$, sequential rationality for player 3 requires $\bar{\sigma}_2(m_2) = \frac{a}{a+b}$, and both X and Y yield continuation payoff $w(m_2) = \frac{ab}{a+b}$ to player 2 at every ω_2 with that m_2. Thus $w(m_2) \in \left\{a, b, \frac{ab}{a+b}\right\}$ depends only on m_2 (and $w(m_2) \in \{a, b\}$ when period 2 play is restricted to pure strategies, so the corresponding $\sigma \in \Sigma_{\text{BS}}$ lies in $\{(X, X), (Y, Y)\}$). Whenever $\Pr(m_2) > 0$ on the equilibrium path, Bayes' rule implies $\Pr(s_2 = X \mid m_2) = \bar{\sigma}_2(m_2)$; since player 3 observes only m_2, s_3 is conditionally independent of ω_2 given m_2, so the conditional distribution of (s_2, s_3) given m_2 is an equilibrium $\sigma(m_2) \in \Sigma_{\text{BS}}$ with $V_2(\sigma(m_2)) = w(m_2)$.

Now move back to period 1. When $m_1 = 0$, player 2 enters period 2 with $h_2 = 0$ and $m_2 = 0$ regardless of q_1, so producing yields $-c + w(0) < w(0)$ and sequential rationality requires $\pi_2(0) = 0$. The unconditional probability of period 1 production is therefore $p = \Pr(q_1 = 1) = \mu_1\pi_2(1)$. If $p = 0$, then $q_1 = 0$ and $m_2 = 0$ with probability 1, so (s_2, s_3) follows $\sigma(0) \in \Sigma_{\text{BS}}$, matching Part 2(a). Suppose instead that $p > 0$, which implies $\pi_2(1) > 0$.

Offering money in period 1 gives player 1 payoff $u\pi_2(1) > 0 = u\pi_2(0)$, so $\mu_1 = 1$ and $p = \pi_2(1)$. At $m_1 = 1$, player 2 gets $w(0)$ from $q_1 = 0$ and $-c + \max\{w(1), w(0)\}$ from $q_1 = 1$ (after optimally choosing whether to offer money in period 2). Optimality of $\pi_2(1) = p > 0$ requires $-c + \max\{w(1), w(0)\} \geq w(0)$, and hence $w(1) - w(0) \geq c > 0$, with equality if $p \in (0, 1)$. Since $w(1) > w(0)$, player 2 strictly prefers to offer money when holding it ($\tau_2 = 1$). With $\mu_1 = 1$ and $\tau_2 = 1$, we have $m_2 = q_1$ with probability 1 on the equilibrium path. If $p = 1$, then $q_1 = m_2 = 1$ with probability 1, $(s_2, s_3) \sim \sigma(1) \in \Sigma_{\text{BS}}$ with $V_2(\sigma(1)) = w(1)$, and choosing $\sigma(0) \in \Sigma_{\text{BS}}$ with $V_2(\sigma(0)) = w(0)$ gives $V_2(\sigma(1)) - V_2(\sigma(0)) \geq c$, matching Part 2(b). If $p \in (0, 1)$, then both $m_2 = 1$ and $m_2 = 0$ occur with positive probability and coincide on path with $q_1 = 1$ and $q_1 = 0$, so $(s_2, s_3) \mid (q_1 = 1) \sim \sigma(1) \in \Sigma_{\text{BS}}$ and $(s_2, s_3) \mid (q_1 = 0) \sim \sigma(0) \in \Sigma_{\text{BS}}$ with $V_2(\sigma(1)) - V_2(\sigma(0)) = c$, matching Part 2(c). ■

A.2 Finite-Horizon Extension with $T \geq 1$ Exchange Periods

We now extend the monetary model to any finite number $T \geq 1$ of pairwise exchange periods before the endgame, with $T+1$ total periods and $T+2$ players indexed by $i \in \{1, \ldots, T+2\}$. Setting $T = 1$ recovers the three-player, two-period model of Appendix A.1, and setting $T = 2$ gives the four-player, three-period model of treatment M-1*.

Player 1 begins period 1 with one unit of indivisible, intrinsically useless fiat money ($h_1 = 1$, and $h_i = 0$ for $i \geq 2$). In each exchange period $t \in \{1, \ldots, T\}$, player t (consumer) and player $t + 1$ (producer) meet privately: if $h_t = 1$, player t chooses whether to offer money, $m_t \in \{0, 1\}$ (with $m_t = 0$ if $h_t = 0$); after observing m_t, player $t + 1$ chooses whether to produce, $q_t \in \{0, 1\}$, generating period t payoffs uq_t for player t and $-cq_t$ for player $t + 1$ and money holding $h_{t+1} = m_t q_t$ for player $t + 1$ entering period $t + 1$. Notice each player's life cycle: player 1 acts only in period 1; each intermediate player $i \in \{2, \ldots, T\}$ produces in period $i - 1$ and consumes in period i; and player $T + 1$ produces in period T before entering the period $T + 1$ endgame with player $T + 2$. In period $T + 1$, player $T + 1$ chooses whether to offer money to player $T + 2$, $m_{T+1} \in \{0, 1\}$, whenever $h_{T+1} = 1$ (with $m_{T+1} = 0$ if $h_{T+1} = 0$), and player $T + 2$ observes only m_{T+1} before players $T + 1$ and $T + 2$ play the battle of the sexes game.

Notice also why Section 2 distinguishes $T = 1$ from $T \geq 2$ regarding whether money must be transferred. When $T = 1$, once player 2 acquires money in period 1, merely displaying $h_2 = 1$ in the period 2 endgame would work identically to transferring it, because neither player acts after period 2. When $T \geq 2$, however, money must actually change hands in exchange meetings so that the last producer, player $T + 1$, can carry it into the endgame. Proposition 2 formalizes both the monetary equilibrium for general $T \geq 1$ and this unraveling result.

Proposition 2 *Suppose $u > c > 0$ and $a > b > 0$.*

1. *For any finite $T \geq 1$, the monetary strategy profile in which every money holder offers money ($m_t = 1$ whenever $h_t = 1$ for $t = 1, \ldots, T + 1$), every producer produces if and only if offered money ($q_t = m_t$ for $t = 1, \ldots, T$), and players $T + 1$ and $T + 2$ play (X, X) if $m_{T+1} = 1$ and (Y, Y) if $m_{T+1} = 0$ is a PBE if and only if $a - b \geq c$.*

2. *Suppose $T \geq 2$. If money can be verifiably displayed in meetings (and even transferred in period 1) but cannot be transferred in exchange periods $t \geq 2$, then no PBE sustains positive production in any period $t \in \{1, \ldots, T\}$.*

Proof. Part 1. Under the candidate profile, every information set with $h_t = 1$ or $m_t = 1$ is reached with probability 1, and as verified below, sequential rationality at off-path information sets where $m_t = 0$ holds for any off-path beliefs. To check sequential rationality, start in period $T+1$. Playing (X, X) after $m_{T+1} = 1$ and (Y, Y) after $m_{T+1} = 0$ forms a static Nash equilibrium of the battle of the sexes game in each case and is therefore sequentially rational for any beliefs of player $T + 2$. When $h_{T+1} = 1$, offering money gives player $T + 1$ continuation payoff a rather than b, so $m_{T+1} = 1$ is strictly optimal since $a > b$.

Now consider period T. If $m_T = 0$, player $T + 1$ enters period $T + 1$ with $h_{T+1} = 0$ and continuation payoff b regardless of q_T, so $q_T = 0$ is strictly optimal ($b > -c + b$). If $m_T = 1$, producing gives $h_{T+1} = 1$ and payoff $-c + a$, whereas not producing gives $h_{T+1} = 0$ and payoff b. Hence $q_T = m_T$ is sequentially rational for player $T + 1$ if and only if $a - b \geq c$ (if $a - b < c$, deviating to $q_T = 0$ and playing Y in period $T + 1$ is strictly profitable). Given $q_T = m_T$, offering money when $h_T = 1$ gives player T consumption payoff $u > 0$, whereas

keeping it gives 0 because player T does not act after period T, so $m_T = 1$ is strictly optimal.

If $T \geq 2$, proceed by backward induction over $t = T-1, \ldots, 1$. Under the profile, in period $t+1$ player $t+2$ plays $q_{t+1} = m_{t+1}$ and player $t+1$ offers money whenever $h_{t+1} = 1$. Because player $t+1$ does not act after period $t+1$, entering period $t+1$ with $h_{t+1} = 1$ yields continuation payoff u (for any beliefs about periods before t), whereas entering with $h_{t+1} = 0$ yields 0. In period t, if $m_t = 0$, then $h_{t+1} = 0$ regardless of q_t, making $q_t = 0$ strictly optimal ($0 > -c$). If $m_t = 1$, producing gives $h_{t+1} = 1$ and net payoff $-c + u$, whereas not producing gives $h_{t+1} = 0$ and payoff 0; since $u > c$, $q_t = 1$ is strictly optimal. Given $q_t = m_t$, offering money when $h_t = 1$ gives player t consumption $u > 0$ rather than 0, so $m_t = 1$ is strictly optimal. Thus the profile is a PBE if and only if $a - b \geq c$.

Part 2. Suppose $T \geq 2$ and money cannot be transferred in exchange periods $t \geq 2$. Because players 1 and 2 do not enter the period $T+1$ endgame (since $T+1 \geq 3$) and no player $i \geq 3$ can ever acquire money, $h_{T+1} = 0$ after every history. Player $T+1$ can therefore neither transfer nor display money in period $T+1$, so player $T+2$ has a single period $T+1$ information set and plays X with a single probability $\sigma_{T+2} \in [0,1]$. By sequential rationality in period $T+1$, player $T+1$'s continuation payoff is $\bar{v}_{T+1}(\sigma_{T+2}) = \max\{a\sigma_{T+2},\, b(1-\sigma_{T+2})\}$ after every period T history, regardless of q_T and regardless of whether player T displayed money in period T. In period T, producing yields $-c + \bar{v}_{T+1}(\sigma_{T+2}) < \bar{v}_{T+1}(\sigma_{T+2})$ at every period T information set of player $T+1$ (both on and off the equilibrium path), so sequential rationality requires $q_T = 0$ with probability 1 after every period T history.

Now induct backward over $t = T-1, \ldots, 1$. Suppose that in period $t+1$, player $t+2$ chooses $q_{t+1} = 0$ with probability 1 after every period $t+1$ history. Because player $t+1$ does not act after period $t+1$ and money has no intrinsic value, player $t+1$ gets continuation payoff 0 in period $t+1$ after every period t history, whether or not player $t+1$ holds or displays money. Hence at every period t information set of player $t+1$, producing yields $-c < 0$ whereas not producing yields 0, requiring $q_t = 0$ with probability 1 after every period t history. By induction, no PBE sustains positive production in any period $t \in \{1, \ldots, T\}$. ∎

B Period 1 Production, Group Surplus, and Learning Dynamics

This appendix complements the session-level nonparametric tests in Sections 4.1–4.3 with session-clustered regressions and session- and subject-level breakdowns. Treating each session average as one observation in a four-session WMW test ($N = 4$ per treatment), as in Section 4, is fully nonparametric, while a regression framework adds three things: (1) it uses cardinal session magnitudes rather than ranks alone, sharpening power when an outlier session makes rank tests conservative (as in the B-1 versus H-1 comparison of Section 4.1); (2) it estimates interaction terms directly to test Hypothesis 2; and (3) it incorporates the post-experiment individual measures from Section 3 to check whether period 1 production reflects social preferences or demographic characteristics.

Allowing for arbitrary correlation across subjects and rounds within each session, we estimate an ordinary least squares (OLS) linear probability model of player 2's period 1 production decision across the six main treatments (89 subjects in the role of player 2 over 15 rounds, or 1,335 observations across 24 sessions):

$$\begin{aligned} y_{it} = \alpha &+ \beta_M \,\text{Money}_i + \beta_H \,\text{History}_i + \beta_D \,\text{Recommendation}_i \\ &+ \beta_{MD}\,(\text{Money}_i \times \text{Recommendation}_i) \\ &+ \beta_{HD}\,(\text{History}_i \times \text{Recommendation}_i) + \gamma' X_i + \lambda_t + \varepsilon_{it}. \end{aligned} \tag{1}$$

Here $y_{it} \in \{0, 1\}$ equals 1 if subject i produces in period 1 of round t. Treatment B-0 is the omitted baseline, and Money_i, History_i, and Recommendation_i are indicators for the treatments with money (M-0 and M-1), public histories (H-0 and H-1), and strategy recommendations (B-1, M-1, and H-1). The vector X_i contains post-experiment individual controls (SVO angle, CRT score, self-reported risk tolerance, gender, age, English fluency, and an indicator for Economics, Finance, Accounting, Business, or Management majors), and λ_t denotes round fixed effects. Standard errors are clustered by session (24 clusters, df = 23).

Table 11 reports estimates over all 15 rounds without individual controls in Column (1) and

with X_i across all rounds and three round intervals in Columns (2)–(5). Two patterns stand out. First, the standalone coefficients on money (β_M), public histories (β_H), and strategy recommendations (β_D) are all small and insignificant, whereas the interactions β_{MD} and β_{HD} are large, positive, and grow with experience: period 1 trade emerges when observable past play is paired with recommendations, and not from either feature alone. Second, comparing Columns (1) and (2) shows that adding X_i barely moves the treatment and interaction estimates. Consistent with self-interested play in Section 2, the coefficient on SVO angle is essentially zero in every interval (-0.001 over all rounds), ruling out social preferences as an explanation for period 1 production; CRT score, risk tolerance, English fluency, and major are also insignificant, while male subjects produce more on average and age has a small, marginally negative effect after round 5. Below, Appendices B.1–B.3 test Hypotheses 1–3 and examine session-level dispersion, and Appendix B.4 analyzes round-by-round production trends and subject-level distributions.

B.1 Tests of Hypothesis 1

Consider first Hypothesis 1. Table 12 reports the four linear combinations from equation (1) comparing money and public histories against the baseline under each recommendation regime (Section 4.1). Without recommendations, we cannot reject the null that period 1 production with money ($\beta_M = 0.098$ without controls and 0.073 with controls) or public histories ($\beta_H = 0.063$ and 0.030) equals production in B-0 in any round interval. In contrast, with recommendations, money raises production above B-1 ($\beta_M + \beta_{MD}$) by 0.671 without controls and 0.689 with controls over all rounds, rising to 0.786 in rounds 11–15 ($p = 0.001$ in rounds 1–5 and $p < 0.001$ in all other columns). Public histories ($\beta_H + \beta_{HD}$) similarly raise production above B-1 by 0.502 without controls ($p = 0.009$) and 0.562 with controls ($p < 0.001$) over all rounds, reaching 0.687 in rounds 11–15 ($p < 0.001$).

Notice why the four-session WMW test between B-1 and H-1 in Section 4.1 ($p = 0.114$ over all rounds and $p = 0.086$ in rounds 11–15; Table 3) is conservative relative to Table 12. In Table 16, three of the four B-1 sessions averaged between 0.050 and 0.200, below every H-1 session (0.483 to 1.000), whereas B-1 session 2 averaged 0.800 overall. That single session

Table 11: Period 1 Production Regressions Across the Main Treatments

	(1) All rounds	(2) All rounds	(3) Rounds 1–5	(4) Rounds 6–15	(5) Rounds 11–15
Money (β_M)	0.098	0.073	0.080	0.069	0.064
	[0.108]	[0.121]	[0.092]	[0.141]	[0.117]
History (β_H)	0.063	0.030	-0.008	0.049	0.048
	[0.093]	[0.117]	[0.087]	[0.134]	[0.127]
Recommendation (β_D)	0.103	0.047	0.101	0.019	-0.013
	[0.149]	[0.134]	[0.148]	[0.133]	[0.130]
Money × Recommendation (β_{MD})	0.573***	0.617***	0.486***	0.683***	0.722***
	[0.170]	[0.158]	[0.158]	[0.169]	[0.154]
History × Recommendation (β_{HD})	0.440**	0.532***	0.439**	0.578***	0.639***
	[0.198]	[0.185]	[0.186]	[0.191]	[0.183]
SVO angle		-0.001	-0.001	-0.001	-0.002
		[0.002]	[0.002]	[0.002]	[0.002]
CRT score		-0.021	0.041	-0.053	-0.039
		[0.061]	[0.063]	[0.065]	[0.065]
Risk tolerance (0–10)		-0.018	-0.011	-0.021	-0.018
		[0.013]	[0.016]	[0.014]	[0.013]
Male		0.201***	0.141*	0.231***	0.218***
		[0.062]	[0.079]	[0.061]	[0.062]
Age		-0.015	-0.014	-0.016*	-0.017*
		[0.009]	[0.011]	[0.008]	[0.009]
Native/Fluent English		-0.098	-0.178	-0.058	-0.013
		[0.124]	[0.149]	[0.117]	[0.118]
Econ/Fin/Business major		0.035	0.051	0.027	0.014
		[0.065]	[0.070]	[0.068]	[0.069]
Constant	0.261***	0.704**	0.682**	0.614**	0.547**
	[0.084]	[0.268]	[0.320]	[0.247]	[0.258]
Individual controls	No	Yes	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes	Yes	Yes
Individual-round observations	1,335	1,335	445	890	445
Individuals	89	89	89	89	89
Sessions (clusters)	24	24	24	24	24
R^2	0.370	0.417	0.310	0.482	0.507

Notes: OLS linear probability models of player 2's period 1 production decision across B-0, B-1, M-0, M-1, H-0, and H-1, with B-0 as the omitted category. All columns include round fixed effects. Column (1) omits individual controls; Columns (2)–(5) control for SVO angle, CRT score ($0.50 per correct answer out of three), risk tolerance (0–10 self-reported willingness to take risks), male (1 = male, 0 otherwise), age (years), native/fluent English (1 = native or fluent, 0 = proficient), and Economics/Finance/Business major. Session-clustered standard errors (24 clusters, df = 23) are in brackets; $^{*}p < 0.100$, $^{**}p < 0.050$, $^{***}p < 0.010$ (two-sided t-tests).

keeps the four-session rank-sum p-value above 0.050 over all rounds; once session magnitudes are taken into account, the gain from public histories under recommendations is large and significant at the 1% level.

Because money and public histories operate through the same theoretical channel in Section 2, allowing period 2 play to condition on period 1 trade, pooling the eight sessions with observable past play (M and H, $N = 8$) against the four baseline sessions ($N = 4$) sharpens nonparametric power as well (Table 13). Without recommendations, pooled production in M-0 and H-0 (0.249 over all rounds) does not differ significantly from B-0 (0.171, $p = 0.461$). With recommendations, pooled production in M-1 and H-1 (0.849 over all rounds) is nearly three times the B-1 average (0.300). Although early baseline production mutes the rank-test difference in rounds 1–5 ($p = 0.145$), from the p-values in Table 13 we reject equal output over all rounds and in rounds 6–15 ($p = 0.016$) as well as in rounds 11–15 ($p = 0.012$).

Table 12: Parametric Contrasts Testing Hypothesis 1

	(1) All rounds	(2) All rounds	(3) Rounds 1–5	(4) Rounds 6–15	(5) Rounds 11–15
M-0 − B-0 (β_M)	0.098	0.073	0.080	0.069	0.064
	[0.108]	[0.121]	[0.092]	[0.141]	[0.117]
H-0 − B-0 (β_H)	0.063	0.030	-0.008	0.049	0.048
	[0.093]	[0.117]	[0.087]	[0.134]	[0.127]
M-1 − B-1 ($\beta_M + \beta_{MD}$)	0.671***	0.689***	0.566***	0.751***	0.786***
	[0.131]	[0.121]	[0.154]	[0.110]	[0.109]
H-1 − B-1 ($\beta_H + \beta_{HD}$)	0.502***	0.562***	0.431**	0.627***	0.687***
	[0.175]	[0.147]	[0.171]	[0.138]	[0.132]
Individual controls	No	Yes	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes	Yes	Yes
Individual-round observations	1,335	1,335	445	890	445
Sessions (clusters)	24	24	24	24	24

Notes: Entries report linear combinations of the OLS estimates in Table 11. Session-clustered standard errors (24 clusters, df = 23) computed from the cluster-robust covariance matrix are in brackets; $^{*}p < 0.100$, $^{**}p < 0.050$, $^{***}p < 0.010$ (two-sided t-tests).

Table 13: Joint Nonparametric Test of Money and Public Histories on Production

Rounds	Without recommendations			With recommendations		
	B-0 ($N = 4$)	M-0 & H-0 ($N = 8$)	WMW p-value	B-1 ($N = 4$)	M-1 & H-1 ($N = 8$)	WMW p-value
All	0.171	0.249	0.461	0.300	0.849	0.016
1–5	0.233	0.310	0.360	0.413	0.838	0.145
6–15	0.140	0.218	0.776	0.244	0.855	0.016
11–15	0.133	0.198	0.606	0.200	0.856	0.012

Notes: Entries report unweighted session averages of period 1 production rates and exact two-sided WMW p-values comparing the eight pooled sessions with money or public histories against the four baseline sessions under each recommendation regime. Exact two-sided p-values use doubled one-sided probabilities ($2\min\{\Pr(W \leq w), \Pr(W \geq w)\}$); under symmetric deviations from the mean ($\Pr(|W - E[W]| \geq |w - E[W]|)$), the three unequal-sample cells with ties change by at most 0.002 (e.g., $p = 0.014$ instead of 0.016 over all rounds with recommendations).

B.2 Period 1 Tests of Hypothesis 2

Now consider Hypothesis 2 in period 1. Alongside the within-institution recommendation effects tested nonparametrically in Section 4.2 ($p = 0.029$ in M and H versus $p = 0.486$ over all rounds in B; Result 2), Hypothesis 2 predicts a difference-in-differences comparison: recommendations should raise production strictly more when trade is incentive compatible in M and H than when it is not in B. Table 14 reports both sets of period 1 tests from equation (1); Appendix C evaluates period 2 adherence, where the recommended actions are incentive compatible across B, M, and H, together with selective compliance across periods 1 and 2 in the baseline.

In Panel A of Table 14, we cannot reject zero recommendation effect in the baseline in any round interval ($\beta_D = 0.103$ over all rounds without controls and 0.047 with controls, $p = 0.731$, falling to -0.013 in rounds 11–15, $p = 0.921$). In contrast, recommendations raise average period 1 production with controls by 0.664 under money ($\beta_D + \beta_{MD}$, $p < 0.001$) and by 0.579 under public histories ($\beta_D + \beta_{HD}$, $p < 0.001$). In Panel B, we reject equal recommendation effects between the baseline and either institution in every column: over all rounds, recommendations raise period 1 production relative to the baseline by an additional 0.573 with money (β_{MD}, $p = 0.003$) and 0.440 with public histories (β_{HD}, $p = 0.037$) without controls, and by 0.617 ($p < 0.001$) and 0.532 ($p = 0.008$) with controls. As early baseline production fades, these interactions widen to 0.722 and 0.639 in rounds 11–15 ($p < 0.001$

and $p = 0.002$), providing clear support for Hypothesis 2 in period 1.

Table 14: Parametric Period 1 Contrasts and Interactions Testing Hypothesis 2

	(1) All rounds	(2) All rounds	(3) Rounds 1–5	(4) Rounds 6–15	(5) Rounds 11–15
Panel A. Within-institution recommendation contrasts					
B-1 − B-0 (β_D)	0.103	0.047	0.101	0.019	-0.013
	[0.149]	[0.134]	[0.148]	[0.133]	[0.130]
M-1 − M-0 ($\beta_D + \beta_{MD}$)	0.675***	0.664***	0.587***	0.702***	0.710***
	[0.081]	[0.087]	[0.059]	[0.110]	[0.087]
H-1 − H-0 ($\beta_D + \beta_{HD}$)	0.542***	0.579***	0.541***	0.598***	0.626***
	[0.131]	[0.125]	[0.109]	[0.135]	[0.127]
Panel B. Difference-in-differences interaction effects					
(M-1 − M-0) − (B-1 − B-0) (β_{MD})	0.573***	0.617***	0.486***	0.683***	0.722***
	[0.170]	[0.158]	[0.158]	[0.169]	[0.154]
(H-1 − H-0) − (B-1 − B-0) (β_{HD})	0.440**	0.532***	0.439**	0.578***	0.639***
	[0.198]	[0.185]	[0.186]	[0.191]	[0.183]
Individual controls	No	Yes	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes	Yes	Yes
Individual-round observations	1,335	1,335	445	890	445
Sessions (clusters)	24	24	24	24	24

Notes: Panel A reports within-institution period 1 recommendation contrasts and Panel B reports difference-in-differences interaction coefficients from Table 11. Session-clustered standard errors (24 clusters, df = 23) are in brackets; $^{*}p < 0.100$, $^{**}p < 0.050$, $^{***}p < 0.010$ (two-sided t-tests).

B.3 Tests of Hypothesis 3 and Session-Level Production

Turning to Hypothesis 3, Table 15 provides the parametric counterpart to the WMW comparisons of money and public histories in Section 4.3 (Tables 5–6 and Result 3). Panel A reports period 1 production contrasts from equation (1), and Panel B reports corresponding OLS contrasts across all 1,335 group-rounds for realized three-player group surplus ($2 \times \text{Produce} + 4 \times \mathbb{1}\{\text{endgame outcome} \in \{(X,X),(Y,Y)\}\}$). Across both panels, we cannot reject outcome equivalence between the two institutions in any column. In Panel A, the production difference between M-0 and H-0 is near zero (0.036 without controls, $p = 0.720$; 0.042 with controls, $p = 0.719$), while the difference between M-1 and H-1 is 0.169 without controls ($p = 0.167$), narrows to 0.127 with controls ($p = 0.172$), and falls to 0.099 in rounds 11–15 ($p = 0.291$). As for realized group surplus in Panel B, the gap between money and public histories is small and insignificant both without recommendations (−0.481 points without controls, $p = 0.165$; −0.298 with controls, $p = 0.367$) and with recommendations

(-0.036 without controls, $p = 0.946$; -0.208 with controls, $p = 0.617$).

Table 16 and Figure 5 report period 1 production rates by session across all six main treatments and clarify why M-1 has a higher point average (0.940) than H-1 (0.758) in Section 4.3. Across individual subjects in the role of player 2, median production is identical in M-1 and H-1 (0.933 over all 15 rounds and 1.000 in rounds 11–15). Where the two treatments differ is in cross-session dispersion, echoing Bigoni, Camera, and Casari (2020) in an infinite-horizon setting: all four M-1 sessions cluster tightly between 0.900 and 0.978 (SD = 0.033), whereas two H-1 sessions average 1.000 and 0.950 and the other two average 0.483 and 0.600 (SD = 0.255; nonparametric Brown–Forsythe WMW $p = 0.029$).

Both of the lower H-1 sessions (0.483 in session 1 and 0.600 in session 3) reflect idiosyncratic choices by specific subjects rather than a general failure to coordinate. In H-1 session 3, (X, X) followed production at a rate of 0.926, and two subjects in the role of player 2 produced in 15 and 12 rounds while the third never produced. In H-1 session 1, one subject in the role of player 2 produced in 14 rounds, but a single subject in the role of player 3 chose Y in every round to push player 2 toward (Y, Y), reducing (X, X) after production to 0.586 and discouraging the other player 2 subjects. In the exit survey, that subject wrote: "I realized that player 2 would lose a point if they produced for player one, so by choosing y when they chose x, they would get punished. By only choosing Y I would reinforce the idea that player 2 would keep on getting punished unless they conformed to me." Although two player 3 subjects in M-1 session 2 (and one each in sessions 3 and 4) reported trying a similar strategy after token offers, subjects in the role of player 2 in those sessions continued producing for tokens (0.933 to 0.978).

Table 15: Parametric Production and Surplus Contrasts Testing Hypothesis 3

	(1) All rounds	(2) All rounds	(3) Rounds 1–5	(4) Rounds 6–15	(5) Rounds 11–15
Panel A. Period 1 production contrasts					
M-0 − H-0 ($\beta_M - \beta_H$)	0.036	0.042	0.088	0.020	0.015
	[0.099]	[0.116]	[0.078]	[0.142]	[0.123]
M-1 − H-1 ($\beta_M + \beta_{MD} - \beta_H - \beta_{HD}$)	0.169	0.127	0.134	0.124	0.099
	[0.118]	[0.090]	[0.090]	[0.096]	[0.091]
Panel B. Three-player group surplus contrasts (points per round)					
M-0 − H-0	-0.481	-0.298	-0.277	-0.309	-0.610
	[0.335]	[0.324]	[0.296]	[0.359]	[0.513]
M-1 − H-1	-0.036	-0.208	0.668	-0.646	-0.519
	[0.521]	[0.411]	[0.486]	[0.431]	[0.428]
Individual controls	No	Yes	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes	Yes	Yes
Group-round observations	1,335	1,335	445	890	445
Sessions (clusters)	24	24	24	24	24

Notes: Panel A reports period 1 production contrasts between money and public histories from Table 11. Panel B reports corresponding contrasts from OLS regressions of three-player group surplus per round ($2 \times \text{Produce} + 4 \times \mathbb{1}\{\text{endgame outcome} \in \{(X,X),(Y,Y)\}\}$) across all 1,335 group-rounds, with round fixed effects in all columns and player 2's individual controls from Table 11 in Columns (2)–(5). Session-clustered standard errors (24 clusters, df = 23) are in brackets; $^{*}p < 0.100$, $^{**}p < 0.050$, $^{***}p < 0.010$ (two-sided t-tests).

Table 16: Period 1 Production Rate by Session in the Main Treatments

	Rounds					Rounds			
	1–5	6–15	11–15	All		1–5	6–15	11–15	All
B-0	**0.233**	**0.140**	**0.133**	**0.171**	**M-1**	**0.925**	**0.948**	**0.938**	**0.940**
Session 1	0.467	0.400	0.333	0.422	Session 1	0.800	0.950	0.950	0.900
Session 2	0.200	0.100	0.133	0.133	Session 2	0.950	0.950	0.900	0.950
Session 3	0.067	0.033	0.067	0.044	Session 3	0.950	0.925	0.900	0.933
Session 4	0.200	0.025	0.000	0.083	Session 4	1.000	0.967	1.000	0.978
B-1	**0.413**	**0.244**	**0.200**	**0.300**	**H-0**	**0.258**	**0.223**	**0.208**	**0.235**
Session 1	0.300	0.075	0.050	0.150	Session 1	0.333	0.367	0.333	0.356
Session 2	1.000	0.700	0.600	0.800	Session 2	0.300	0.275	0.250	0.283
Session 3	0.300	0.150	0.150	0.200	Session 3	0.150	0.000	0.000	0.050
Session 4	0.050	0.050	0.000	0.050	Session 4	0.250	0.250	0.250	0.250
M-0	**0.363**	**0.213**	**0.188**	**0.263**	**H-1**	**0.750**	**0.763**	**0.775**	**0.758**
Session 1	0.350	0.075	0.100	0.167	Session 1	0.550	0.450	0.500	0.483
Session 2	0.400	0.500	0.400	0.467	Session 2	1.000	1.000	1.000	1.000
Session 3	0.500	0.275	0.250	0.350	Session 3	0.600	0.600	0.600	0.600
Session 4	0.200	0.000	0.000	0.067	Session 4	0.850	1.000	1.000	0.950

Notes: Bold rows report unweighted averages across the four sessions in each treatment.

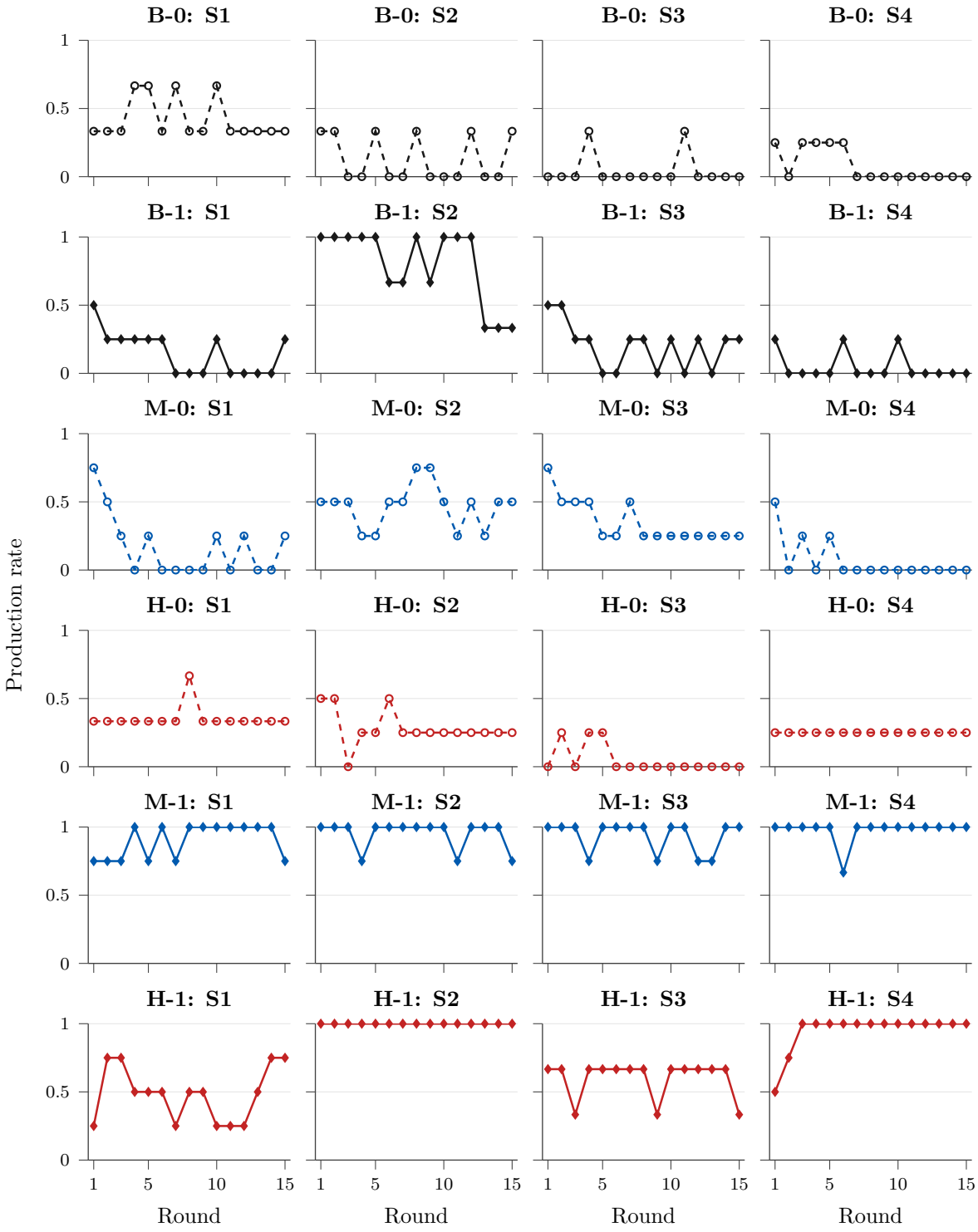


Figure 5: Period 1 Production Rate by Session in the Main Treatments

B.4 Round-by-Round Production Trends and Individual Distributions

Finally, we examine how the treatment differences in Section 4 (Figure 2 and Sections 4.1–4.2) evolve across rounds and individual subjects. Round 1 play, before subjects receive any feedback, shows that the separation between equilibrium and non-equilibrium recommendations emerges through experience rather than initial compliance: in round 1, recommendations raise period 1 production by roughly one-third (0.313 to 0.333) across all three institutions, from 0.229 in B-0 to 0.563 in B-1, from 0.625 in M-0 to 0.938 in M-1, and from 0.271 in H-0 to 0.604 in H-1. Over the 15 rounds, output holds steady in M-1 and H-1, where producing is rewarded in period 2, whereas it unravels in B-1 alongside B-0, M-0, and H-0. By round 15, the initial baseline effect disappears (0.167 in B-0 versus 0.208 in B-1, $p = 1.000$); even in B-1 session 2 (0.800 overall; Table 16), output starts at 1.000 in rounds 1–5 before dropping to 0.333 in rounds 13–15 (Figure 5).

Table 17 estimates linear time trends by interacting treatment indicators with a round variable from 1 to 15, without controls in Column (1) and with controls in Column (2) (because individual controls are constant across rounds, slope point estimates are identical across columns and p-values differ by at most 0.001). In Panel A, output declines over time in every treatment without observable past play or without recommendations: in Column (1), the round slope is steepest in B-1 at -0.020 per round ($p = 0.002$) as player 2 subjects abandon the unrewarded recommendation, and is also negative in M-0 (-0.018 per round, $p = 0.003$), B-0 (-0.009, $p = 0.022$), and H-0 (-0.005, $p = 0.053$). In contrast, round slopes are flat in M-1 (0.001 per round, $p = 0.779$) and H-1 (0.004, $p = 0.331$). From Panel B, we reject equal trends between either equilibrium treatment and B-1: the round slopes in M-1 and H-1 exceed the B-1 slope by 0.021 ($p = 0.008$) and 0.024 ($p = 0.003$) per round, and exceed the M-0 and H-0 slopes by 0.019 ($p = 0.011$) and 0.010 ($p = 0.067$) per round.

Subject-level distributions across the 89 subjects in the role of player 2 tell the same story (Table 18). Positive output in B-1 does not come from a subset of subjects who follow the recommendation in every round: no baseline subject produces in all 15 rounds or even in all five of rounds 11–15 (0.000 in both B-0 and B-1). Instead, most player 2 subjects in B-1

try producing early on (only 0.267 never produce over all 15 rounds) and stop once they learn it does not pay, leaving 0.692 in B-0 and 0.600 in B-1 who never produce in rounds 11–15 (alongside 0.688 in M-0 and 0.800 in H-0). In contrast, sustained trade in M-1 and H-1 reflects broad adherence across subjects: in rounds 11–15, 0.667 of subjects in the role of player 2 in both treatments produce in all five rounds, and 1.000 in M-1 and 0.733 in H-1 produce in at least four of the five.

Table 17: Linear Round-Trend Regressions Across the Main Treatments

	(1) No controls			(2) With controls		
	Slope	SE	p-value	Slope	SE	p-value
Panel A. Linear round slope by treatment (per round)						
B-0	-0.009	[0.004]	0.022	-0.009	[0.004]	0.022
M-0	-0.018	[0.005]	0.003	-0.018	[0.005]	0.003
H-0	-0.005	[0.003]	0.053	-0.005	[0.003]	0.054
B-1	-0.020	[0.006]	0.002	-0.020	[0.006]	0.003
M-1	0.001	[0.004]	0.779	0.001	[0.004]	0.780
H-1	0.004	[0.004]	0.331	0.004	[0.004]	0.332
Panel B. Differences in linear round slopes						
M-0 − B-0	-0.009	[0.007]	0.195	-0.009	[0.007]	0.196
H-0 − B-0	0.004	[0.005]	0.439	0.004	[0.005]	0.440
M-1 − B-1	0.021	[0.007]	0.008	0.021	[0.007]	0.008
H-1 − B-1	0.024	[0.007]	0.003	0.024	[0.007]	0.003
B-1 − B-0	-0.011	[0.007]	0.134	-0.011	[0.007]	0.135
M-1 − M-0	0.019	[0.007]	0.011	0.019	[0.007]	0.011
H-1 − H-0	0.010	[0.005]	0.067	0.010	[0.005]	0.068
M-0 − H-0	-0.012	[0.006]	0.053	-0.012	[0.006]	0.054
M-1 − H-1	-0.003	[0.006]	0.612	-0.003	[0.006]	0.613
Individual controls	No			Yes		
Observations	1,335			1,335		
Sessions (clusters)	24			24		
R^2	0.374			0.420		

Notes: OLS linear probability models of player 2's period 1 production decision on treatment indicators and their interactions with a linear round variable (Round $\in \{1, \ldots, 15\}$), without individual controls in Column (1) and with the individual controls from Table 11 in Column (2). Session-clustered standard errors (24 clusters, df = 23) are in brackets, with two-sided t-test p-values.

Table 18: Subject-Level Distribution of Period 1 Production

Treatment	Subjects	All 15 rounds			Rounds 11–15		
		Never (= 0)	At least 0.800	Always (= 1)	Never (= 0)	At least 0.800	Always (= 1)
B-0	13	0.462	0.000	0.000	0.692	0.077	0.000
M-0	16	0.125	0.125	0.063	0.688	0.125	0.125
H-0	15	0.533	0.200	0.133	0.800	0.200	0.200
B-1	15	0.267	0.133	0.000	0.600	0.067	0.000
M-1	15	0.000	0.933	0.467	0.000	1.000	0.667
H-1	15	0.067	0.733	0.400	0.133	0.733	0.667

Notes: Entries report the fraction of subjects in the role of player 2 in each treatment whose period 1 production rate equals 0.000, is at least 0.800, or equals 1.000 over all 15 rounds and over rounds 11–15.

C Endgame Play, Strategy Adherence, and Dynamic Incentives

Section 4.4 showed that subjects follow the recommended Nash equilibrium actions in period 2 across B-1, M-1, and H-1 (Figure 3 and Table 7), making the recommended strategy strictly more profitable than deviating under money and public histories (Table 8 and Result 4). This appendix examines each piece of that evidence in detail. Appendix C.1 tests baseline endgame play in B-0 and B-1 against the static mixed strategy equilibrium and evaluates selective compliance across periods 1 and 2 (Section 4.2). Appendix C.2 turns to M-0, M-1, H-0, and H-1, reporting full two-period strategy adherence, disaggregated conditional endgame outcomes and payoffs, session-clustered incentive regressions, and within-subject reinforcement of next-round production. Appendix C.3 reports unconditional endgame outcomes across treatments and sessions together with gross payoffs by action.

C.1 Baseline Endgame Play and Selective Compliance

Consider first the baseline treatments B-0 and B-1. Table 19 reports all four period 2 outcome frequencies (Panel A) and tests session averages against the static mixed strategy equilibrium of the battle of the sexes game using one-sample t-tests with df $= 3$ (Panel B). Without recommendations (B-0), (X, X) averages 0.350 over all rounds, (Y, Y) averages 0.203, and miscoordination averages 0.447. From the p-values in Panel B, we cannot reject

the static mixed strategy equilibrium (0.750 for player 2, 0.250 for player 3, and 0.188 for (X, X); all $p \geq 0.264$), whereas pure (X, X) (1.000) is rejected ($p \leq 0.028$). In contrast, with recommendations (B-1), (X, X) rises to 0.865 over all rounds and 0.933 in rounds 11–15 ($p = 0.057$ versus B-0), (Y, Y) and (Y, X) nearly disappear ($p = 0.029$ for both), and we reject the mixed strategy benchmark on every metric ($p \leq 0.004$).

Comparing period 1 and period 2 choices in the baseline also provides a direct test of selective compliance, because subjects in the role of player 2 in B-1 receive a non-equilibrium recommendation to produce in period 1 alongside a Nash equilibrium recommendation to play X in period 2. In Table 20, player 2's period 1 production rate in B-1 falls from 0.413 in rounds 1–5 to 0.200 in rounds 11–15, and we cannot reject equality with B-0 ($p = 0.486$ over all rounds and $p = 0.971$ in rounds 11–15). In period 2, however, player 2 chooses X at 0.988 over all rounds in B-1 versus 0.708 in B-0 ($p = 0.029$), reaching 1.000 in rounds 11–15, and player 3's X play in B-1 rises from 0.804 in rounds 1–5 to 0.933 in rounds 11–15 (0.874 overall versus 0.439 in B-0, $p = 0.057$). Even immediately after choosing not to produce in period 1, player 2 subjects in B-1 follow the period 2 recommendation to play X at 0.985 over all rounds and 1.000 in rounds 11–15. Notice that all 15 subjects in the role of player 2 in B-1 choose X in period 2 strictly more often than they produce in period 1 (exact Wilcoxon signed-rank $p < 0.001$), ruling out both inattention and experimenter demand (Result 2).

Table 19: Endgame Play and Mixed-Equilibrium Benchmark Tests in B-0 and B-1

Panel A. Endgame outcome frequencies and WMW tests

	(X,X)		(Y,Y)		(X,Y)		(Y,X)	
Rounds	B-0	B-1	B-0	B-1	B-0	B-1	B-0	B-1
All	0.350	0.865	0.203	0.004	0.358	0.122	0.089	0.008
1–5	0.367	0.779	0.158	0.000	0.408	0.196	0.067	0.025
6–15	0.342	0.908	0.225	0.006	0.333	0.085	0.100	0.000
11–15	0.342	0.933	0.221	0.000	0.346	0.067	0.092	0.000
WMW p (All)	0.057		0.029		0.057		0.029	

Panel B. Mixed-equilibrium benchmark and treatment tests (all rounds)

		B-0		B-1		
Metric	MSNE	Mean	p_{MSNE}	Mean	p_{MSNE}	WMW p (B-0 vs. B-1)
Player 2 plays X	0.750	0.708	0.606	0.988	< 0.001	0.029
Player 3 plays X	0.250	0.439	0.264	0.874	0.004	0.057
Outcome (X,X)	0.188	0.350	0.345	0.865	0.003	0.057

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values. In Panel B, p_{MSNE} reports two-sided one-sample t-test p-values ($\text{df} = 3$) comparing the four session averages against the static mixed strategy equilibrium (0.750, 0.250, and 0.188).

Table 20: Selective Compliance with Strategy Recommendations in B-0 versus B-1

Action	Rounds	B-0	B-1	WMW p-value
Player 2: Produce (period 1)	All	0.171	0.300	0.486
	1–5	0.233	0.413	0.600
	6–15	0.140	0.244	0.486
	11–15	0.133	0.200	0.971
Player 2: Play X (period 2)	All	0.708	0.988	0.029
	1–5	0.775	0.975	0.057
	6–15	0.675	0.994	0.029
	11–15	0.688	1.000	0.029
Player 2: Play X if no production	All	0.710	0.985	0.029
	1–5	0.802	0.952	0.400
	6–15	0.670	0.993	0.029
	11–15	0.662	1.000	0.029
Player 3: Play X (period 2)	All	0.439	0.874	0.057
	1–5	0.433	0.804	0.057
	6–15	0.442	0.908	0.057
	11–15	0.433	0.933	0.057

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values. "Player 2: Play X if no production" conditions on rounds in which player 2 did not produce in period 1 ($N = 3$ sessions in rounds 1–5 of B-1 because all player 2 subjects in session 2 produced in rounds 1–5). In B-1, all 15 subjects in the role of player 2 choose X in period 2 strictly more often than they produce in period 1 (exact two-sided Wilcoxon signed-rank $p < 0.001$ over all 15 rounds and in rounds 11–15).

C.2 Conditional Endgame Play, Payoffs, and Reinforcement under Money and Public Histories

We next turn to the treatments with money and public histories. While Table 7 measures period 2 adherence alone, Figures 6 and 7 track round-by-round adherence to the full two-period productive equilibrium strategy across all 15 rounds, by role and for three-player groups jointly. With money (Figure 6), player 1 offers the token in nearly every round (0.888 in M-0 versus 0.992 in M-1, WMW $p = 0.029$), while recommendations raise full two-period adherence from 0.213 to 0.944 for player 2 and from 0.079 to 0.683 for all three group members jointly (both $p = 0.029$). With public histories (Figure 7), recommendations raise two-period adherence from 0.226 to 0.746 for player 2 ($p = 0.029$) and from 0.181 to 0.668 for the group ($p = 0.057$). Because player 3 acts only in period 2, player 3's adherence matches Table 7 (0.458 to 0.743 in M and 0.532 to 0.906 in H, both $p = 0.029$). Notice also that the frequency of the efficient outcome, period 1 production followed by (X, X) in period 2, is virtually identical in M-1 and H-1 (0.683 versus 0.668 over all rounds, $p = 1.000$, and 0.688 versus 0.725 in rounds 11–15, $p = 0.943$), consistent with Result 3.

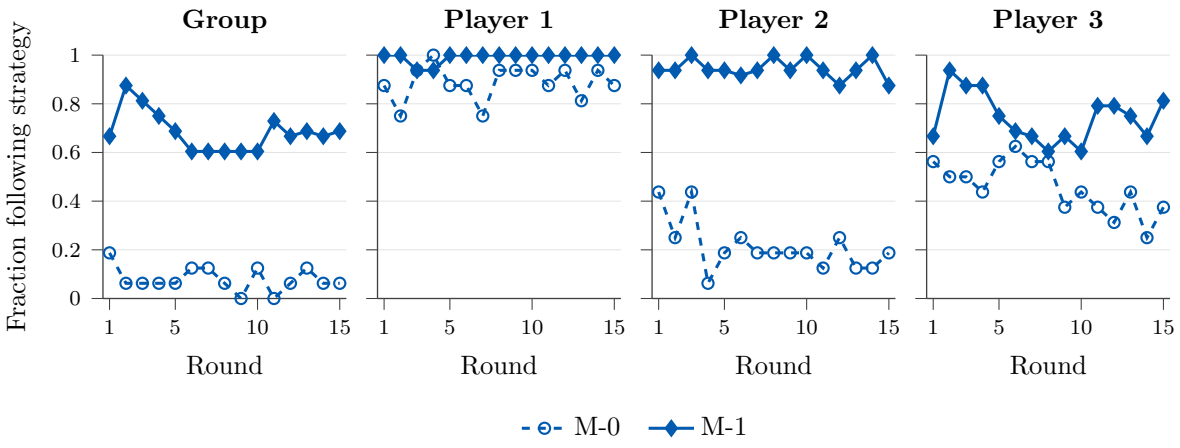


Figure 6: Choices Consistent with the Monetary Strategy in M-0 and M-1

Notes: Panel 1 shows the fraction of three-player groups in which all three subjects make choices consistent with the monetary equilibrium; Panels 2–4 report each role separately. Consistency means offering the token for player 1; producing if and only if offered the token, offering an acquired token to player 3, and choosing X when offering the token and Y otherwise for player 2; and choosing X when offered the token and Y otherwise for player 3.

Tables 21–23 disaggregate the pooled payoff comparisons in Table 8 by reporting conditional

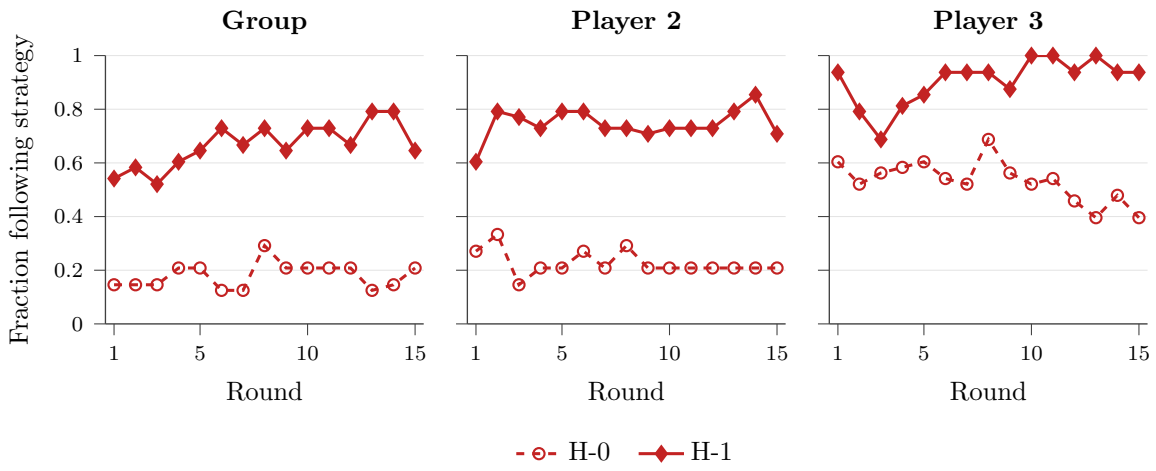


Figure 7: Choices Consistent with the Productive Equilibrium Strategy in H-0 and H-1

Notes: Panel 1 shows the fraction of three-player groups in which player 2 produces in period 1 and players 2 and 3 play (X, X) in period 2; Panels 2 and 3 report each role separately. Consistency means producing in period 1 and choosing X in period 2 for player 2, and choosing X if player 2 produced in period 1 and Y otherwise for player 3.

endgame outcomes, player 2's net round payoffs, and player 3's expected endgame payoffs separately for M-0, M-1, H-0, and H-1. In Table 21, conditional coordination without recommendations breaks down on opposite branches across the two institutions: (1) in M-0, token transfers end in miscoordination (X, Y) in 0.606 of meetings and (X, X) in only 0.276 (below the 0.435 (X, X) rate without a transfer); (2) in H-0, producing yields (X, X) at 0.746 overall (0.867 in rounds 11–15), but (X, X) persists after non-production as well (0.475 overall and 0.650 in rounds 11–15, with (Y, Y) at 0.076 overall). In contrast, recommendations establish clear history-contingent coordination on both branches. Over all rounds of M-1, (X, X) occurs in 0.728 of meetings with a token transfer versus 0.000 without one, where (Y, Y) reaches 0.560 (0.667 in rounds 11–15). Similarly, in H-1, (X, X) reaches 0.848 after production (0.913 in rounds 11–15) versus 0.011 after non-production, where (Y, Y) averages 0.386 overall, climbing from 0.278 in rounds 1–5 to 0.817 in rounds 11–15. Unconditionally across all meetings, joint coordination on (X, X) after a transfer or production and (Y, Y) otherwise rises from 0.142 in M-0 to 0.725 in M-1 and from 0.244 in H-0 to 0.800 in H-1, and we reject the null of no recommendation effect in both institutions (WMW $p = 0.029$).

These outcome frequencies directly govern player 2's net round payoffs after subtracting the 1-point production cost (Table 22). Without recommendations, period 1 trade lowers player

2's average net payoff over all rounds from 1.321 without a transfer to −0.171 with one in M-0, and from 1.501 without production to 1.253 with production in H-0 (1.963 versus 1.600 in rounds 11–15). With recommendations, higher (X, X) after trade and (Y, Y) after non-trade reverse both comparisons, raising player 2's net payoff over all rounds from 0.524 to 1.185 in M-1 and from 0.419 to 1.563 in H-1.

As for player 3, Table 23 reports empirical expected endgame payoffs from choosing X versus Y given player 2's conditional play. Without recommendations, because player 2 chooses X at high rates after both histories, playing X yields a higher expected payoff than Y on every branch in M-0 and H-0 (including 0.882 versus 0.354 after a transfer in M-0). With recommendations, conditional adherence is player 3's empirical best response on each branch: playing X pays strictly more after a transfer or production (1.000 versus 0.000 in M-1 and 0.978 versus 0.065 in H-1 over all rounds), whereas playing Y pays more otherwise (1.786 versus 0.405 in M-1 and 1.192 versus 0.603 in H-1 overall, widening to 2.000 versus 0.333 and 2.450 versus 0.183 in rounds 11–15). Figures 8–11 plot the corresponding round-by-round conditional coordination rates (Figures 8 and 10) and player 2 gross and net payoffs (Figures 9 and 11), showing that history-contingent coordination and positive net returns to trading persist across all 15 rounds of M-1 and H-1.

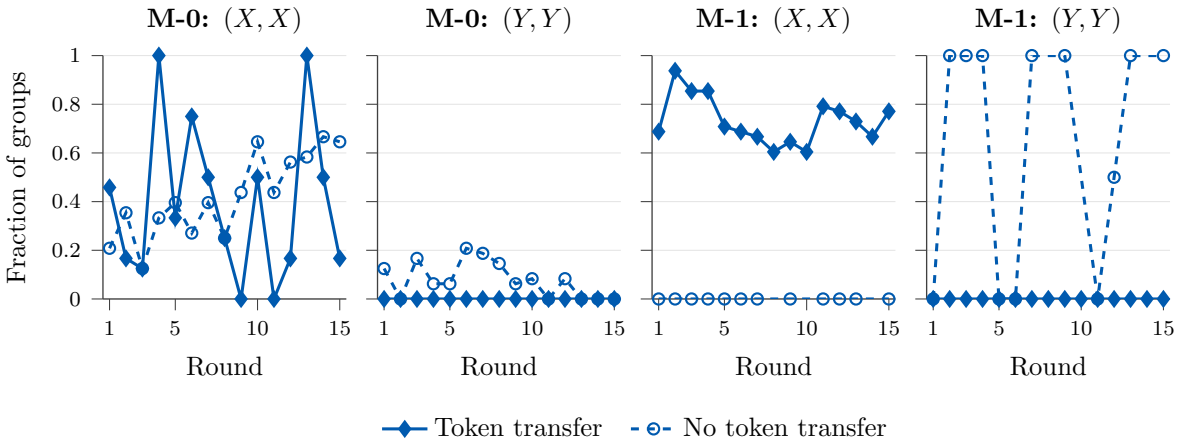


Figure 8: (X, X) and (Y, Y) Frequencies in M-0 and M-1 by Token Transfer

Notes: The first two panels show M-0 and the last two panels show M-1, conditional on whether player 2 transferred the token to player 3.

Table 21: Conditional Endgame Outcome Frequencies with Money and Public Histories

		M-0		M-1		H-0		H-1	
Outcome	Rounds	Trans.	No trans.	Trans.	No trans.	Prod.	No prod.	Prod.	No prod.
(X,X)	All	0.276	0.435	0.728	0.000	0.746	0.475	0.848	0.011
	1–5	0.274	0.300	0.811	0.000	0.650	0.345	0.754	0.000
	6–15	0.281	0.497	0.686	0.000	0.845	0.538	0.897	0.023
	11–15	0.276	0.573	0.742	0.000	0.867	0.650	0.913	0.000
(Y,Y)	All	0.000	0.076	0.000	0.560	0.015	0.076	0.017	0.386
	1–5	0.000	0.078	0.000	0.833	0.042	0.112	0.023	0.278
	6–15	0.000	0.076	0.000	0.542	0.000	0.059	0.014	0.652
	11–15	0.000	0.014	0.000	0.667	0.000	0.013	0.000	0.817
(X,Y)	All	0.606	0.376	0.272	0.405	0.224	0.377	0.130	0.592
	1–5	0.664	0.486	0.189	0.083	0.267	0.414	0.224	0.685
	6–15	0.478	0.324	0.314	0.458	0.155	0.359	0.083	0.326
	11–15	0.557	0.297	0.258	0.333	0.133	0.325	0.075	0.183
(Y,X)	All	0.118	0.113	0.000	0.036	0.015	0.072	0.004	0.011
	1–5	0.063	0.135	0.000	0.083	0.042	0.129	0.000	0.037
	6–15	0.241	0.102	0.000	0.000	0.000	0.044	0.006	0.000
	11–15	0.167	0.116	0.000	0.000	0.000	0.013	0.013	0.000
Observations	All	53	187	210	15	51	174	173	52
	1–5	22	58	69	6	19	56	57	18
	6–15	31	129	141	9	32	118	116	34
	11–15	14	66	69	6	15	60	59	16

Notes: Entries report unweighted averages across all sessions (out of four per treatment) with at least one observation in that cell, conditional on whether player 2 transferred the token to player 3 (M-0 and M-1) or produced in period 1 (H-0 and H-1). Boundary sessions with a single realized history in a window are M-0 session 4 (0.000 transfers in rounds 6–15), M-1 session 4 (1.000 transfers in rounds 1–5 and 11–15), H-0 session 3 (0.000 production in rounds 6–15), and H-1 sessions 2 and 4 (1.000 production in all rounds and in rounds 6–15, respectively). Observation counts pool all four outcomes across sessions.

Table 22: Player 2 Net Round Payoffs Conditional on Token Transfer or Production

	M-0		M-1		H-0		H-1	
Rounds	Trans.	No trans.	Trans.	No trans.	Prod.	No prod.	Prod.	No prod.
All	-0.171	1.321	1.185	0.524	1.253	1.501	1.563	0.419
1–5	-0.179	0.859	1.432	0.833	0.992	1.147	1.284	0.278
6–15	-0.156	1.537	1.057	0.458	1.536	1.672	1.703	0.720
11–15	-0.171	1.713	1.225	0.500	1.600	1.963	1.738	0.817

Notes: Entries report unweighted session averages (across sessions with at least one observation in that cell; boundary sessions match Table 21) of player 2's net round payoff (period 2 endgame payoff minus the 1-point production cost whenever player 2 produced), conditional on producing and transferring the token versus not transferring a token (M-0 and M-1) and on producing versus not producing in period 1 (H-0 and H-1).

Table 23: Player 3 Expected Endgame Payoffs by Action Conditional on Token Transfer or Production

Panel A. Money (M-0 and M-1)

	M-0				M-1			
	Transfer		No transfer		Transfer		No transfer	
Rounds	X	Y	X	Y	X	Y	X	Y
All	0.882	0.354	0.811	0.568	1.000	0.000	0.405	1.786
1–5	0.938	0.188	0.787	0.640	1.000	0.000	0.083	2.750
6–15	0.759	0.722	0.822	0.535	1.000	0.000	0.458	1.625
11–15	0.833	0.500	0.870	0.391	1.000	0.000	0.333	2.000

Panel B. Public histories (H-0 and H-1)

	H-0				H-1			
	Production		No production		Production		No production	
Rounds	X	Y	X	Y	X	Y	X	Y
All	0.971	0.088	0.852	0.445	0.978	0.065	0.603	1.192
1–5	0.917	0.250	0.759	0.722	0.977	0.068	0.685	0.944
6–15	1.000	0.000	0.897	0.310	0.980	0.060	0.348	1.955
11–15	1.000	0.000	0.975	0.075	0.988	0.038	0.183	2.450

Notes: Entries report unweighted session averages (across sessions with at least one observation in that cell; boundary sessions match Table 21) of player 3's empirical expected period 2 payoff from choosing X ($1 \times \Pr(\text{player 2 plays } X)$) versus Y ($3 \times \Pr(\text{player 2 plays } Y)$), conditional on token transfer (Panel A) or period 1 production (Panel B).

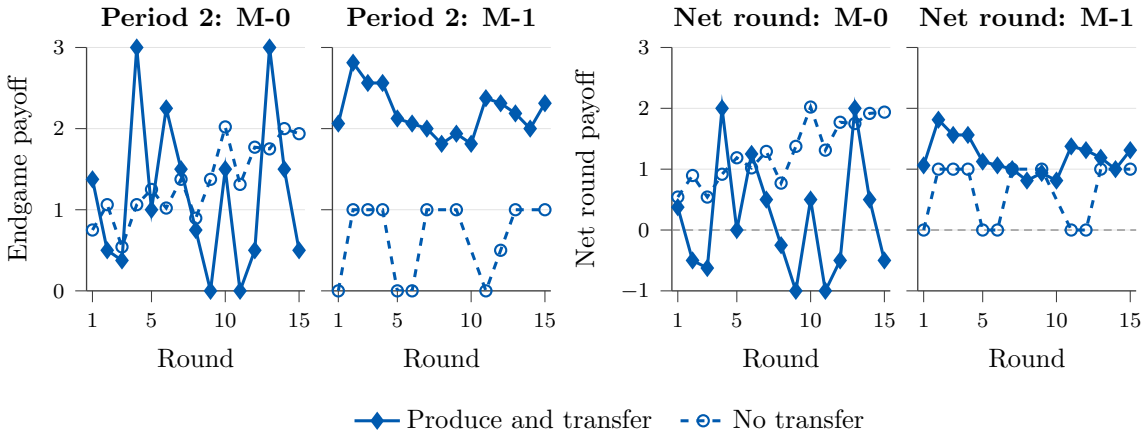


Figure 9: Player 2 Payoffs in M-0 and M-1 by Token Transfer

Notes: The first two panels show player 2's gross period 2 endgame payoff and the last two panels show player 2's net round payoff across both periods, conditional on producing and transferring the token versus not transferring a token.

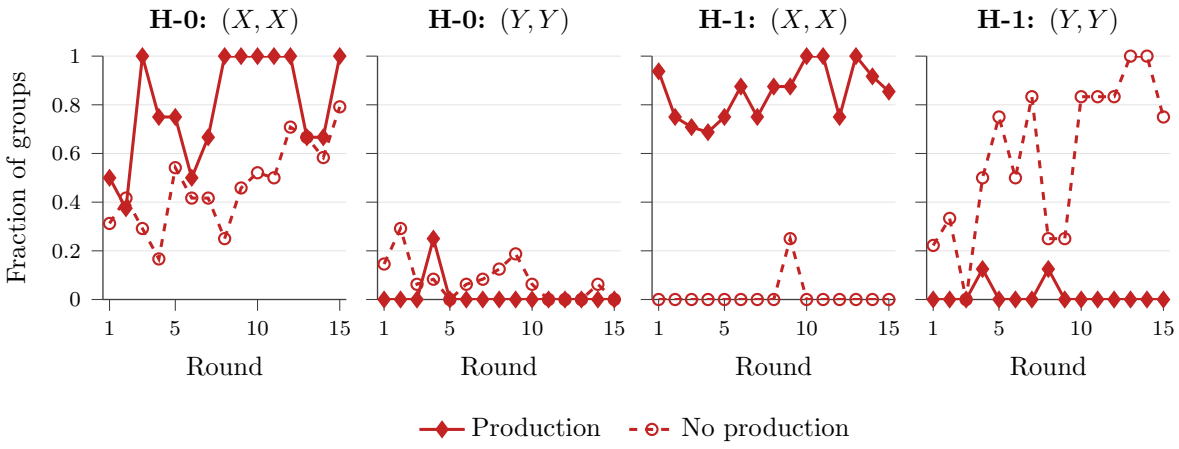


Figure 10: (X, X) and (Y, Y) Frequencies in H-0 and H-1 by Period 1 Production

Notes: The first two panels show H-0 and the last two panels show H-1, conditional on whether player 2 produced in period 1.

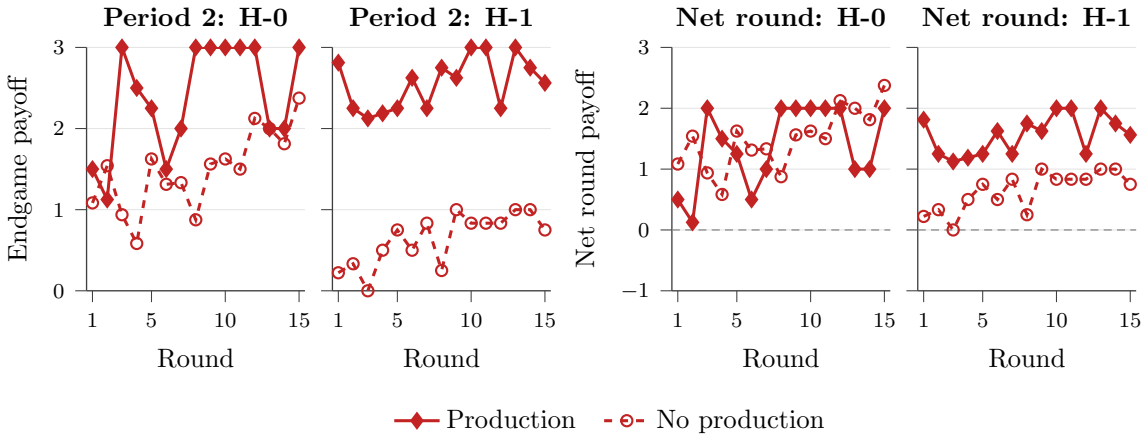


Figure 11: Player 2 Payoffs in H-0 and H-1 by Period 1 Production

Notes: The first two panels show player 2's gross period 2 endgame payoff and the last two panels show player 2's net round payoff across both periods, conditional on whether player 2 produced in period 1.

To test these conditional wedges and recommendation effects parametrically, Table 24 estimates OLS regressions across all 915 group-rounds in M-0, M-1, H-0, and H-1 with round fixed effects and session-clustered standard errors (16 clusters). Without recommendations (Panel A), transferring a token in M-0 fails to raise (X, X) (-0.049), while producing in H-0 raises (X, X) by 0.300 ($p < 0.001$) with no significant shift in (Y, Y) (-0.068); on net, period 1 trade reduces player 2's round payoff in both treatments (-1.170 in M-0, $p < 0.001$, and -0.166 in H-0, $p = 0.051$). In contrast, with recommendations, transferring a token or producing raises (X, X) by 0.728 in M-1 and 0.856 in H-1, lowers (Y, Y) by 0.670 and 0.528, and raises player 2's net payoff by 0.579 in M-1 and 1.039 in H-1 (all $p < 0.001$), with player 3 choosing Y in 14 of the 15 non-transfer meetings in M-1 (and all 6 in rounds 11–15). In Panel B, difference-in-differences estimates confirm that recommendations increase the responsiveness of (X, X), (Y, Y), player 3's X choices, and player 2's net payoff to period 1 trade (1.749 points in M and 1.205 points in H; $p < 0.001$ for all four outcomes in both institutions).

Finally, Table 25 tests at the subject level whether experiencing (X, X) rather than miscoordination (X, Y) after producing in round $t-1$ reinforces period 1 production in round t. Because subjects in the role of player 2 almost always choose X after producing, (X, X) versus (X, Y) accounts for nearly all variation in post-production outcomes (Table 21). Using rounds $t \geq 2$ in which player 2 produced in round $t-1$ and experienced either (X, X) or (X, Y) in period 2, we estimate the within-subject linear probability model

$$\text{Produce}_{i,t} = \beta_g \mathbb{1}\{\text{Outcome}_{i,t-1} = (X, X)\} + \alpha_i + \lambda_{g,t} + \varepsilon_{i,t},$$

where (X, Y) is the omitted category and α_i and $\lambda_{g,t}$ are subject and subsample-specific round fixed effects, with standard errors clustered by subject (player 2). Since period 2 play cannot depend on period 1 production in B-0 or B-1, we pool B-0 and B-1 into a single baseline benchmark and estimate M-0, M-1, H-0, and H-1 separately. Prior (X, X) has no significant effect on next-round production in the pooled baseline (0.109, SE 0.142, $p = 0.455$) or without recommendations (-0.037, $p = 0.754$ in M-0, and 0.240, $p = 0.240$ in H-0). In contrast, with recommendations, experiencing (X, X) rather than (X, Y) raises

next-round production by 0.074 in M-1 ($p = 0.069$) and by 0.209 in H-1 ($p = 0.041$). Using HC2 standard errors with the Bell–McCaffrey degrees-of-freedom adjustment yields the same pattern: insignificant in B-0/B-1 ($p = 0.517$), M-0 ($p = 0.798$), and H-0 ($p = 0.334$), and positive at the 10% level in M-1 ($p = 0.071$) and H-1 ($p = 0.061$).

Table 24: Conditional Endgame Coordination and Player 2 Net Payoff Regressions

	(1) Endgame (X, X)	(2) Endgame (Y, Y)	(3) Player 3 plays X	(4) Player 2 net payoff
Panel A. Conditional difference (transfer/production minus no transfer/no production)				
M-0 (Transfer − No transfer)	-0.049	-0.078***	-0.089***	-1.170***
	[0.073]	[0.022]	[0.025]	[0.228]
M-1 (Transfer − No transfer)	0.728***	-0.670***	0.665***	0.579***
	[0.040]	[0.113]	[0.080]	[0.122]
H-0 (Production − No production)	0.300***	-0.068	0.246***	-0.166*
	[0.028]	[0.040]	[0.020]	[0.078]
H-1 (Production − No production)	0.856***	-0.528***	0.844***	1.039***
	[0.064]	[0.041]	[0.077]	[0.195]
Panel B. Recommendation effect on conditional wedge (difference-in-differences)				
M-1 − M-0 (Recommendation effect in M)	0.777***	-0.592***	0.754***	1.749***
	[0.083]	[0.115]	[0.086]	[0.254]
H-1 − H-0 (Recommendation effect in H)	0.556***	-0.459***	0.597***	1.205***
	[0.068]	[0.057]	[0.079]	[0.206]
Round fixed effects	Yes	Yes	Yes	Yes
Group-round observations	915	915	915	915
Sessions (clusters)	16	16	16	16
R^2	0.252	0.292	0.209	0.109

Notes: OLS regressions across all 915 group-rounds in M-0, M-1, H-0, and H-1 interacting treatment indicators with an indicator for whether player 2 transferred the token to player 3 (M-0 and M-1) or produced in period 1 (H-0 and H-1), with round fixed effects. Session-clustered standard errors (16 clusters, df = 15) are in brackets; $^{*}p < 0.100$, $^{**}p < 0.050$, $^{***}p < 0.010$ (two-sided t-tests).

Table 25: Next-Round Production After (X, X) versus (X, Y)

	B-0/B-1	M-0	M-1	H-0	H-1
Prior (X,X)	0.109	-0.037	0.074	0.240	0.209
Clustered standard error	0.142	0.117	0.038	0.184	0.092
p-value	0.455	0.754	0.069	0.240	0.041
Observations	76	53	198	46	159
Clusters	17	14	15	7	14
Player fixed effects	Yes	Yes	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: Within-subject OLS linear probability models of player 2's round t production decision ($t \geq 2$) on an indicator for experiencing (X, X) rather than (X, Y) after producing in round $t-1$. Standard errors are clustered by subject (player 2), with two-sided t-test p-values (df = clusters -1).

C.3 Unconditional Endgame Outcomes and Action Payoffs

Whereas Appendix C.2 conditions on period 1 history, Tables 26–29 report unconditional session averages and exact WMW p-values for (X, X), (X, Y), (Y, X), and (Y, Y) across the six main treatments, and Table 30 gives the breakdown by session. Two patterns underlie the welfare comparisons in Section 4.3. First, recommendations sharply reduce endgame miscoordination: over all rounds, (Y, X) nearly disappears across all three environments (falling from 0.061–0.104 without recommendations to 0.004–0.008 with recommendations, $p = 0.029$ in each; Table 28), while (X, Y) falls from 0.358 to 0.122 in B ($p = 0.057$), from 0.425 to 0.271 in M ($p = 0.029$), and from 0.329 to 0.179 in H (0.292 to 0.079 in rounds 11–15, $p = 0.086$; Table 27). Second, unconditional (X, X) over all rounds is higher in B-1 (0.865) than in M-1 (0.683) or H-1 (0.672; $p = 1.000$ for M-1 versus H-1) because B-1 recommends (X, X) after every history, whereas M-1 and H-1 prescribe (Y, Y) whenever no token is transferred or no good is produced. In Table 30, H-1's two high-production sessions (sessions 2 and 4; see Appendix B.3) each record 0.917 (X, X) and 0.000 (Y, Y) over all rounds, whereas its two lower-production sessions (sessions 1 and 3) shift toward (Y, Y) at 0.317 and 0.244 (0.400 and 0.333 in rounds 11–15), raising overall (Y, Y) in H-1 to 0.140 and keeping group surplus equal to M-1.

Table 31 reports unconditional gross endgame payoffs overall and conditional on playing X or Y across all seven treatments. For player 2 (player 3 in M-1*), recommendations raise

mean endgame payoffs over all rounds from 1.253 to 2.600 in B, from 1.288 to 2.092 in M (2.183 in M-1*), and from 1.693 to 2.157 in H. Notice that under money and public histories, equilibrium coordination also raises mean endgame payoffs for player 3 (player 4 in M-1*), even though player 3 prefers (Y, Y) to (X, X): over all rounds, player 3's average payoff increases from 0.596 in M-0 to 0.808 in M-1 (1.350 in M-1*) and from 0.746 in H-0 to 1.093 in H-1. Without recommendations, frequent miscoordination holds player 3's realized payoff when playing Y to 0.376 in M-0 and 0.496 in H-0 over all rounds (0.094 and 0.083 in rounds 11–15). In contrast, with recommendations, avoiding miscoordination outweighs conceding to (X, X): playing X reliably yields close to 1 point (0.993 in M-1, 0.982 in H-1, and 0.966 in M-1* over all rounds), and in H-1 and M-1*, where non-trade meetings are more common, coordinating on (Y, Y) lifts the payoff from playing Y in rounds 11–15 to 2.173 and 2.190. In B-1, where (X, X) is recommended unconditionally, playing Y yields 0.036 over all rounds and player 3 averages 0.878 (0.958 in B-0).

Table 26: Unconditional Frequency of (X, X) in the Endgame

Panel A. Average (X,X) frequency

Rounds	B-0	B-1	M-0	M-1	H-0	H-1
All	0.350	0.865	0.408	0.683	0.542	0.672
1–5	0.367	0.779	0.300	0.758	0.425	0.579
6–15	0.342	0.908	0.463	0.646	0.600	0.719
11–15	0.342	0.933	0.513	0.688	0.683	0.725

Panel B. Exact two-sided WMW p-values

Rounds	B-0 vs. M-0	B-0 vs. H-0	B-1 vs. M-1	B-1 vs. H-1	B-0 vs. B-1	M-0 vs. M-1	H-0 vs. H-1	M-0 vs. H-0	M-1 vs. H-1
All	0.343	0.343	0.114	0.314	0.057	0.029	0.600	0.114	1.000
1–5	0.829	0.486	0.771	0.143	0.086	0.029	0.371	0.371	0.229
6–15	0.314	0.314	0.057	0.657	0.057	0.029	0.686	0.229	0.886
11–15	0.486	0.286	0.057	0.486	0.057	0.143	0.800	0.143	0.943

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values.

Table 27: Unconditional Frequency of (X, Y) in the Endgame

Panel A. Average (X,Y) frequency

Rounds	B-0	B-1	M-0	M-1	H-0	H-1
All	0.358	0.122	0.425	0.271	0.329	0.179
1–5	0.408	0.196	0.525	0.179	0.367	0.308
6–15	0.333	0.085	0.375	0.317	0.310	0.115
11–15	0.346	0.067	0.363	0.263	0.292	0.079

Panel B. Exact two-sided WMW p-values

Rounds	B-0 vs. M-0	B-0 vs. H-0	B-1 vs. M-1	B-1 vs. H-1	B-0 vs. B-1	M-0 vs. M-1	H-0 vs. H-1	M-0 vs. H-0	M-1 vs. H-1
All	0.971	0.343	0.171	0.429	0.057	0.029	0.200	0.114	0.429
1–5	0.200	0.486	0.971	0.343	0.229	0.029	0.314	0.057	0.229
6–15	1.000	0.371	0.029	1.000	0.057	0.400	0.114	0.400	0.143
11–15	0.829	0.400	0.057	1.000	0.057	0.171	0.086	0.143	0.114

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values.

Table 28: Unconditional Frequency of (Y, X) in the Endgame

Panel A. Average (Y,X) frequency

Rounds	B-0	B-1	M-0	M-1	H-0	H-1
All	0.089	0.008	0.104	0.004	0.061	0.008
1–5	0.067	0.025	0.113	0.013	0.108	0.013
6–15	0.100	0.000	0.100	0.000	0.038	0.006
11–15	0.092	0.000	0.113	0.000	0.013	0.013

Panel B. Exact two-sided WMW p-values

Rounds	B-0 vs. M-0	B-0 vs. H-0	B-1 vs. M-1	B-1 vs. H-1	B-0 vs. B-1	M-0 vs. M-1	H-0 vs. H-1	M-0 vs. H-0	M-1 vs. H-1
All	0.486	0.343	1.000	1.000	0.029	0.029	0.029	0.086	1.000
1–5	0.486	0.429	1.000	1.000	0.714	0.057	0.057	1.000	1.000
6–15	1.000	0.057	1.000	1.000	0.029	0.029	0.257	0.086	1.000
11–15	0.857	0.029	1.000	1.000	0.029	0.029	1.000	0.057	1.000

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values.

Table 29: Unconditional Frequency of (Y, Y) in the Endgame

Panel A. Average (Y,Y) frequency

Rounds	B-0	B-1	M-0	M-1	H-0	H-1
All	0.203	0.004	0.063	0.042	0.068	0.140
1–5	0.158	0.000	0.063	0.050	0.100	0.100
6–15	0.225	0.006	0.063	0.038	0.052	0.160
11–15	0.221	0.000	0.013	0.050	0.013	0.183

Panel B. Exact two-sided WMW p-values

Rounds	B-0 vs. M-0	B-0 vs. H-0	B-1 vs. M-1	B-1 vs. H-1	B-0 vs. B-1	M-0 vs. M-1	H-0 vs. H-1	M-0 vs. H-0	M-1 vs. H-1
All	0.200	0.229	0.143	0.429	0.029	0.629	1.000	0.971	0.829
1–5	0.514	0.857	0.143	0.429	0.029	1.000	1.000	0.743	0.829
6–15	0.343	0.229	0.257	0.429	0.143	0.457	0.914	0.543	0.914
11–15	0.143	0.143	0.143	0.429	0.143	0.371	0.429	1.000	0.914

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values.

Table 30: Endgame Outcome Frequencies by Session in the Main Treatments

	Rounds 1–5				Rounds 6–15			
	(X,X)	(X,Y)	(Y,X)	(Y,Y)	(X,X)	(X,Y)	(Y,X)	(Y,Y)
B-0	**0.367**	**0.408**	**0.067**	**0.158**	**0.342**	**0.333**	**0.100**	**0.225**
Session 1	0.333	0.533	0.067	0.067	0.233	0.367	0.133	0.267
Session 2	0.133	0.467	0.000	0.400	0.267	0.433	0.067	0.233
Session 3	0.800	0.133	0.000	0.067	0.767	0.133	0.100	0.000
Session 4	0.200	0.500	0.200	0.100	0.100	0.400	0.100	0.400
B-1	**0.779**	**0.196**	**0.025**	**0.000**	**0.908**	**0.085**	**0.000**	**0.006**
Session 1	0.950	0.050	0.000	0.000	0.975	0.025	0.000	0.000
Session 2	0.867	0.133	0.000	0.000	0.933	0.067	0.000	0.000
Session 3	0.800	0.100	0.100	0.000	1.000	0.000	0.000	0.000
Session 4	0.500	0.500	0.000	0.000	0.725	0.250	0.000	0.025
M-0	**0.300**	**0.525**	**0.113**	**0.063**	**0.463**	**0.375**	**0.100**	**0.063**
Session 1	0.200	0.550	0.100	0.150	0.500	0.275	0.125	0.100
Session 2	0.400	0.450	0.150	0.000	0.525	0.350	0.100	0.025
Session 3	0.400	0.550	0.050	0.000	0.500	0.375	0.050	0.075
Session 4	0.200	0.550	0.150	0.100	0.325	0.500	0.125	0.050
M-1	**0.758**	**0.179**	**0.013**	**0.050**	**0.646**	**0.317**	**0.000**	**0.038**
Session 1	0.500	0.350	0.050	0.100	0.625	0.325	0.000	0.050
Session 2	0.750	0.200	0.000	0.050	0.575	0.400	0.000	0.025

Continued...

Table 30 – Continued

	(X,X)	(X,Y)	(Y,X)	(Y,Y)	(X,X)	(X,Y)	(Y,X)	(Y,Y)
Session 3	0.850	0.100	0.000	0.050	0.650	0.275	0.000	0.075
Session 4	0.933	0.067	0.000	0.000	0.733	0.267	0.000	0.000
H-0	**0.425**	**0.367**	**0.108**	**0.100**	**0.600**	**0.310**	**0.038**	**0.052**
Session 1	0.600	0.267	0.133	0.000	0.600	0.367	0.000	0.033
Session 2	0.300	0.500	0.100	0.100	0.600	0.350	0.050	0.000
Session 3	0.350	0.350	0.150	0.150	0.475	0.300	0.075	0.150
Session 4	0.450	0.350	0.050	0.150	0.725	0.225	0.025	0.025
H-1	**0.579**	**0.308**	**0.013**	**0.100**	**0.719**	**0.115**	**0.006**	**0.160**
Session 1	0.300	0.450	0.050	0.200	0.300	0.325	0.000	0.375
Session 2	0.750	0.250	0.000	0.000	1.000	0.000	0.000	0.000
Session 3	0.467	0.333	0.000	0.200	0.600	0.133	0.000	0.267
Session 4	0.800	0.200	0.000	0.000	0.975	0.000	0.025	0.000
	Rounds 11–15				All Rounds			
B-0	**0.342**	**0.346**	**0.092**	**0.221**	**0.350**	**0.358**	**0.089**	**0.203**
Session 1	0.133	0.400	0.133	0.333	0.267	0.422	0.111	0.200
Session 2	0.333	0.400	0.067	0.200	0.222	0.444	0.044	0.289
Session 3	0.800	0.133	0.067	0.000	0.778	0.133	0.067	0.022
Session 4	0.100	0.450	0.100	0.350	0.133	0.433	0.133	0.300
B-1	**0.933**	**0.067**	**0.000**	**0.000**	**0.865**	**0.122**	**0.008**	**0.004**
Session 1	1.000	0.000	0.000	0.000	0.967	0.033	0.000	0.000
Session 2	0.933	0.067	0.000	0.000	0.911	0.089	0.000	0.000
Session 3	1.000	0.000	0.000	0.000	0.933	0.033	0.033	0.000
Session 4	0.800	0.200	0.000	0.000	0.650	0.333	0.000	0.017
M-0	**0.513**	**0.363**	**0.113**	**0.013**	**0.408**	**0.425**	**0.104**	**0.063**
Session 1	0.500	0.350	0.100	0.050	0.400	0.367	0.117	0.117
Session 2	0.650	0.300	0.050	0.000	0.483	0.383	0.117	0.017
Session 3	0.600	0.300	0.100	0.000	0.467	0.433	0.050	0.050
Session 4	0.300	0.500	0.200	0.000	0.283	0.517	0.133	0.067
M-1	**0.688**	**0.263**	**0.000**	**0.050**	**0.683**	**0.271**	**0.004**	**0.042**
Session 1	0.700	0.250	0.000	0.050	0.583	0.333	0.017	0.067
Session 2	0.600	0.350	0.000	0.050	0.633	0.333	0.000	0.033
Session 3	0.650	0.250	0.000	0.100	0.717	0.217	0.000	0.067
Session 4	0.800	0.200	0.000	0.000	0.800	0.200	0.000	0.000
H-0	**0.683**	**0.292**	**0.013**	**0.013**	**0.542**	**0.329**	**0.061**	**0.068**
Session 1	0.733	0.267	0.000	0.000	0.600	0.333	0.044	0.022

Continued...

Table 30 – Continued

	(X,X)	(X,Y)	(Y,X)	(Y,Y)	(X,X)	(X,Y)	(Y,X)	(Y,Y)
Session 2	0.750	0.250	0.000	0.000	0.500	0.400	0.067	0.033
Session 3	0.500	0.400	0.050	0.050	0.433	0.317	0.100	0.150
Session 4	0.750	0.250	0.000	0.000	0.633	0.267	0.033	0.067
H-1	**0.725**	**0.079**	**0.013**	**0.183**	**0.672**	**0.179**	**0.008**	**0.140**
Session 1	0.350	0.250	0.000	0.400	0.300	0.367	0.017	0.317
Session 2	1.000	0.000	0.000	0.000	0.917	0.083	0.000	0.000
Session 3	0.600	0.067	0.000	0.333	0.556	0.200	0.000	0.244
Session 4	0.950	0.000	0.050	0.000	0.917	0.067	0.017	0.000

Notes: Bold rows report unweighted averages across the four sessions in each treatment.

Table 31: Unconditional Endgame Payoffs by Action

Treatment	Player 2 (3 in M-1*)			Player 3 (4 in M-1*)		
	Mean	Conditional on		Mean	Conditional on	
		Play X	Play Y		Play X	Play Y
Rounds 1–5						
B-0	1.258	1.312	0.708	0.842	0.833	0.804
B-1	2.338	2.404	0.000	0.779	0.972	0.000
M-0	0.963	1.069	0.250	0.488	0.714	0.276
M-1	2.325	2.404	0.889	0.908	0.977	0.567
H-0	1.375	1.597	0.438	0.725	0.792	0.575
H-1	1.838	1.900	0.900	0.879	0.964	0.512
M-1*	1.800	2.238	0.750	1.267	0.933	1.641
Rounds 6–15						
B-0	1.250	1.366	0.561	1.017	0.705	0.953
B-1	2.731	2.739	1.000	0.927	1.000	0.091
M-0	1.450	1.658	0.383	0.650	0.818	0.443
M-1	1.975	2.013	1.000	0.758	1.000	0.305
H-0	1.852	1.971	0.542	0.756	0.938	0.388
H-1	2.317	2.474	0.667	1.200	0.994	1.804
M-1*	2.375	2.847	0.833	1.392	0.981	2.242
Rounds 11–15						
B-0	1.246	1.308	0.561	1.004	0.689	0.919
B-1	2.800	2.800	.	0.933	1.000	0.000
M-0	1.550	1.736	0.083	0.550	0.805	0.094
M-1	2.113	2.168	1.000	0.838	1.000	0.433
H-0	2.063	2.092	0.500	0.721	0.977	0.083
H-1	2.358	2.613	0.667	1.275	0.988	2.173
M-1*	2.350	2.801	0.625	1.317	0.963	2.190
All Rounds						
B-0	1.253	1.357	0.613	0.958	0.740	0.950
B-1	2.600	2.628	0.500	0.878	0.991	0.036
M-0	1.288	1.464	0.365	0.596	0.791	0.376
M-1	2.092	2.145	0.933	0.808	0.993	0.370
H-0	1.693	1.860	0.483	0.746	0.894	0.496
H-1	2.157	2.276	0.650	1.093	0.982	0.760
M-1*	2.183	2.639	0.792	1.350	0.966	1.956

Notes: Entries report unweighted session averages of gross endgame payoffs. A dot (.) indicates that no subject in that treatment and interval chose that action.

D Four-Player Treatment M-1*

This appendix provides additional evidence on treatment M-1* to complement Section 4.5 and Result 5. Appendix D.1 supplements the four-session rank tests in Figure 4 and Table 9 with session-clustered production and round-trend regressions as well as session-level production rates across both exchange periods. Appendix D.2 disaggregates the realized strategy payoffs in Table 10 by history branch and compares unconditional period 3 endgame outcomes against M-1.

D.1 Production Regressions and Session-Level Exchange

In Section 4.5, the four-session rank tests in Table 9 compare session averages without using within-session magnitudes or individual controls. To account for both, Table 32 estimates session-clustered OLS linear probability models pooling player 2's period 1 production in B-1 and M-1 with period 1 (player 2) and period 2 (player 3) production in M-1* across the 12 sessions (810 producer-round observations from 54 subjects). Panel A reports pairwise contrasts with round fixed effects, without individual controls in Column (1) and with the controls from Table 11 in Columns (2)–(5). Period 1 production in M-1* matches M-1 in every round interval (0.001 without controls and −0.002 with controls over all rounds, $p = 0.966$) and far exceeds B-1 (0.672 without controls and 0.685 with controls, $p < 0.001$). As in Appendix B.1, B-1 session 2 (0.800 overall; Table 16) makes the four-session WMW comparison between B-1 and period 2 of M-1* in Table 9 conservative ($p = 0.171$ over all rounds and $p = 0.086$ in rounds 11–15). Once session magnitudes are taken into account in Table 32, period 2 production in M-1* significantly exceeds B-1 over all rounds (0.389 without controls, $p = 0.024$, and 0.499 with controls, $p = 0.003$), widening to 0.655 in rounds 11–15 ($p = 0.001$). Notice also that while period 2 production in M-1* is 0.282 below M-1 without controls ($p < 0.010$), once individual controls are included we cannot reject equal production in any round interval (−0.188 over all rounds, $p = 0.124$, and −0.156 in rounds 11–15, $p = 0.243$), consistent with the raw gap coming from the few subjects who opted out rather than a breakdown of monetary exchange.

Panel B of Table 32 confirms the round-by-round dynamics in Figure 4 by interacting each

series with a linear round variable (Round $\in \{1, \ldots, 15\}$). Whereas production in B-1 declines by -0.020 per round ($p = 0.007$), production holds steady at 0.005 per round in both period 1 ($p = 0.159$) and period 2 ($p = 0.404$) of M-1*, matching the stability of M-1 (0.001, $p = 0.787$) and exceeding the B-1 slope by 0.025 per round in each exchange period ($p = 0.004$ and $p = 0.012$ without controls, and $p = 0.004$ and $p = 0.013$ with controls). Session-level production rates in Table 33 and Figure 12 show how these patterns arise across the four sessions of M-1*. Period 1 exchange is uniformly high across all four sessions (0.889 to 0.978 over all rounds, and 0.933 to 1.000 in rounds 11–15). In period 2, production averages 0.867 in session 1 (1.000 in rounds 11–15) and holds steady at 0.578 to 0.600 over all rounds (0.533 to 0.667 in rounds 11–15) in sessions 2–4: because each session has three subjects in the role of player 3, sessions 2, 3, and 4 each reflect one of the three subjects in the role of player 3 who opted out of monetary exchange (Section 4.5), while the remaining player 3 subjects continued producing for tokens across rounds.

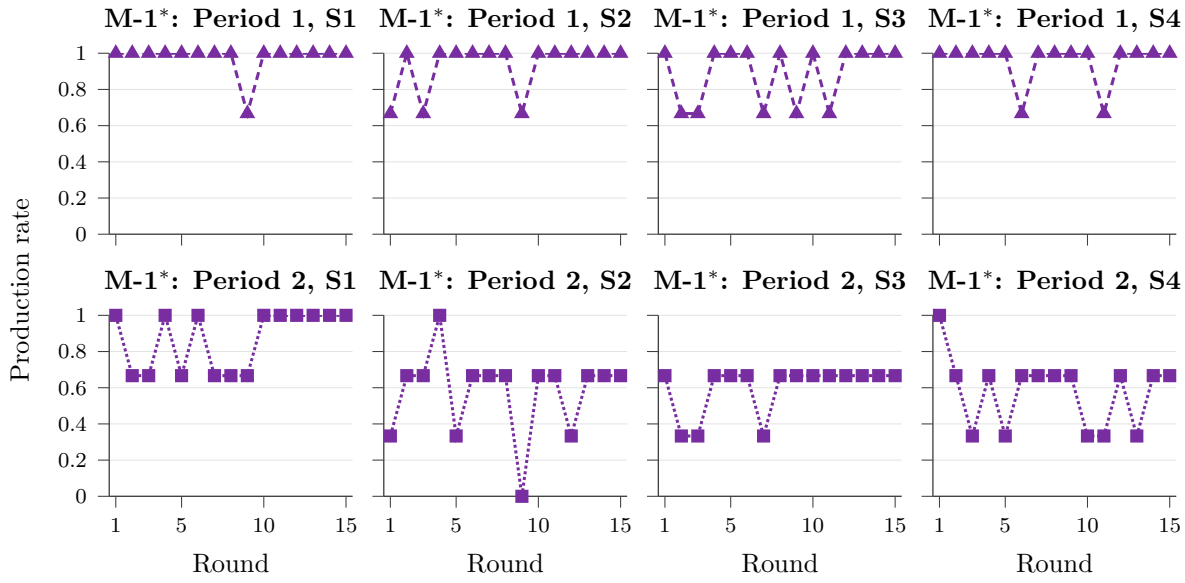


Figure 12: Period 1 and Period 2 Production Rates by Session in Treatment M-1*

Table 32: Production Rate and Round-Trend Regressions in Treatment M-1*

Panel A. Pairwise production differences across round windows

	(1) All rounds	(2) All rounds	(3) Rounds 1–5	(4) Rounds 6–15	(5) Rounds 11–15
M-1* period 1 − B-1	0.672***	0.685***	0.554***	0.750***	0.831***
	[0.135]	[0.119]	[0.146]	[0.115]	[0.119]
M-1* period 2 − B-1	0.389**	0.499***	0.346**	0.575***	0.655***
	[0.148]	[0.133]	[0.151]	[0.130]	[0.144]
M-1* period 1 − M-1	0.001	-0.002	0.007	-0.007	0.021
	[0.022]	[0.047]	[0.056]	[0.059]	[0.060]
M-1* period 2 − M-1	-0.282***	-0.188	-0.200	-0.182	-0.156
	[0.066]	[0.113]	[0.130]	[0.111]	[0.126]
M-1* period 2 − M-1* period 1	-0.283***	-0.186*	-0.207	-0.175*	-0.177
	[0.055]	[0.099]	[0.124]	[0.097]	[0.105]
Individual controls	No	Yes	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes	Yes	Yes
Producer-round observations	810	810	270	540	270
Individuals	54	54	54	54	54
Sessions (clusters)	12	12	12	12	12
R^2	0.389	0.443	0.350	0.505	0.548

Panel B. Linear round slopes (per round, all 15 rounds)

	(1) No controls			(2) With controls		
	Slope	SE	p-value	Slope	SE	p-value
B-1	-0.020	[0.006]	0.007	-0.020	[0.006]	0.007
M-1	0.001	[0.004]	0.787	0.001	[0.004]	0.788
M-1* period 1	0.005	[0.003]	0.159	0.005	[0.003]	0.160
M-1* period 2	0.005	[0.006]	0.404	0.005	[0.006]	0.406
M-1* period 1 − B-1	0.025	[0.007]	0.004	0.025	[0.007]	0.004
M-1* period 2 − B-1	0.025	[0.008]	0.012	0.025	[0.008]	0.013

Notes: OLS linear probability models pooling player 2's period 1 production in B-1 and M-1 with period 1 (player 2) and period 2 (player 3) production in M-1* (810 producer-round observations across 54 subjects in 12 sessions). Panel A reports pairwise contrasts with round fixed effects, without individual controls in Column (1) and with the controls from Table 11 in Columns (2)–(5). Panel B reports treatment-specific linear round slopes (Round $\in \{1, \ldots, 15\}$) and slope differences relative to B-1. Session-clustered standard errors (12 clusters, df = 11) are in brackets; $^{*}p < 0.100$, $^{**}p < 0.050$, $^{***}p < 0.010$ (two-sided t-tests).

Table 33: Production Rate by Session in Treatment M-1*

	Period 1				Period 2			
	1–5	6–15	11–15	All	1–5	6–15	11–15	All
Session 1	1.000	0.967	1.000	0.978	0.800	0.900	1.000	0.867
Session 2	0.867	0.967	1.000	0.933	0.600	0.567	0.600	0.578
Session 3	0.867	0.900	0.933	0.889	0.533	0.633	0.667	0.600
Session 4	1.000	0.933	0.933	0.956	0.600	0.567	0.533	0.578
Average	**0.933**	**0.942**	**0.967**	**0.939**	**0.633**	**0.667**	**0.700**	**0.656**

Notes: The bottom row reports unweighted averages across the four sessions.

D.2 Endgame Coordination and Payoff Incentives

Tables 34 and 35 disaggregate the strategy payoff comparisons in Table 10 by history branch (whether a token is offered or not), mirroring the three-player analysis in Tables 21–23. In the period 3 endgame (Panel A of Table 34), offering the token yields (X, X) in 0.900 of meetings over all rounds and 0.991 in rounds 6–15 (0.983 in rounds 11–15), with 0.000 (Y, Y) in every interval; when no token is offered, (X, X) drops to 0.148 overall while (Y, Y) occurs at 0.644 (0.681 in rounds 6–15).

Given this endgame coordination and period 2 token acceptance, acquiring and transferring a token strictly pays for both player 2 and player 3, whether measured in gross payoffs before subtracting the 1-point production cost (Table 35) or in net round payoffs (Panel B of Table 34). For player 2, who never enters the endgame and acquires a token only to buy period 2 consumption, offering a token in period 2 raises the gross consumption payoff from 0.150 to 2.168 over all rounds (Table 35); net of the 1-point period 1 cost, producing in period 1 and offering the token in period 2 averages 1.168 points overall (1.236 in rounds 11–15), compared with 0.000 when not producing and -0.254 across all meetings without a period 2 token offer. For player 3, offering a token in period 3 to buy (X, X) raises the gross endgame payoff from 1.087 to 2.700 over all rounds (1.131 to 2.972 in rounds 6–15; Table 35); net of the 1-point period 2 cost, producing in period 2 and offering the token in period 3 averages 1.700 points overall (1.972 in rounds 6–15), compared with 1.072 when not producing and 1.053 across all meetings without a period 3 token offer.[13] As for player 4

[13] In Table 10, player 3's "Followed" payoff (1.689) also includes meetings in which player 2 did not offer a token and player 3 followed the recommendation by not producing and playing Y; likewise, "No offer" in

(Panel C of Table 34), conditional best responses mirror those of player 3 in M-1 (Table 23): playing X pays more than Y when offered a token (0.976 versus 0.071 over all rounds, and 0.991 versus 0.028 in rounds 6–15), whereas Y pays more than X otherwise (2.092 versus 0.303 over all rounds).

Finally, paralleling Appendix C.3, Tables 36 and 37 report unconditional period 3 endgame outcomes in M-1*. Even though reaching (X, X) in M-1* requires a token to circulate across two exchange meetings rather than one, unconditional (X, X) in period 3 averages 0.650 over all rounds versus 0.683 in M-1 ($p = 0.971$), and 0.717 versus 0.688 in rounds 11–15 ($p = 0.886$). From the p-values in Table 36, we cannot reject equal (X, X) coordination between the three-player and four-player monetary treatments in any round interval. Across the four sessions of M-1* (Table 37), unconditional (X, X) over all rounds is 0.800 in session 1 (0.933 in rounds 11–15) and 0.733 in session 2, versus 0.600 and 0.467 in sessions 3 and 4, where (Y, Y) accounts for most remaining meetings (0.400 and 0.378, and 0.233 across all four sessions); averaged across sessions, miscoordination on (X, Y) and (Y, X) falls from 0.183 and 0.050 in rounds 1–5 to 0.050 and 0.033 in rounds 11–15.

Panel B of Table 34 pools non-production with the few meetings in which a producer acquired a token but withheld it.

Table 34: Conditional Endgame Outcomes and Payoff Incentives in Treatment M-1*

Panel A. Period 3 endgame outcomes conditional on player 3 token offer

	Token offered			No token offered		
Rounds	(X, X)	(Y, Y)	Obs.	(X, X)	(Y, Y)	Obs.
All	0.900	0.000	115	0.148	0.644	65
1–5	0.726	0.000	37	0.143	0.613	23
6–15	0.991	0.000	78	0.150	0.681	42
11–15	0.983	0.000	42	0.111	0.675	18

Panel B. Net round payoffs for player 2 and player 3 (net of production cost)

	Player 2 (periods 1–2)			Player 3 (periods 2–3)		
Rounds	Prod. & offer	No prod.	No offer	Prod. & offer	No prod.	No offer
All	1.168	0.000	-0.254	1.700	1.072	1.053
1–5	1.143	0.000	-0.500	1.177	1.083	1.006
6–15	1.180	0.000	-0.067	1.972	1.090	1.097
11–15	1.236	0.000	-0.333	1.950	1.008	1.008

Panel C. Player 4 empirical expected period 3 payoff by action

	Token offered		No token offered	
Rounds	Play X	Play Y	Play X	Play Y
All	0.976	0.071	0.303	2.092
1–5	0.948	0.156	0.304	2.089
6–15	0.991	0.028	0.303	2.092
11–15	0.983	0.050	0.270	2.190

Notes: Entries report unweighted averages across all sessions (out of four in M-1*, 180 four-player group-rounds) with at least one observation in that cell. Panel A reports period 3 (X, X) and (Y, Y) frequencies and observation counts conditional on player 3's token offer. Panel B reports net round payoffs (net of the 1-point production cost whenever incurred) for player 2 (periods 1–2) and player 3 (periods 2–3) conditional on producing and offering the token, not producing ("No prod."), or not offering a token to the next player ("No offer"; for player 2, "No prod." and "No offer" average across $N = 4, 2, 4, 2$ and $N = 4, 4, 4, 2$ sessions across the four windows, and in rounds 11–15 no-token cells in Panels A–C average across $N = 3$ sessions because session 1 had 1.000 period 2 production and period 3 token offers). Panel C reports player 4's empirical expected period 3 payoff ($1 \times \Pr(\text{player 3 plays } X)$ versus $3 \times \Pr(\text{player 3 plays } Y)$) conditional on player 3's token offer.

Table 35: Gross Period 2 and Period 3 Payoffs in Treatment M-1*

	Player 2 (Period 2)				**Player 3 (Period 3)**			
		Conditional on Money Offer				Conditional on Money Offer		
Rounds	Mean	Yes	No	Gain	Mean	Yes	No	Gain
All	1.967	2.168	0.150	2.018	2.183	2.700	1.087	1.613
1–5	1.900	2.143	0.000	2.143	1.800	2.177	1.042	1.135
6–15	2.000	2.180	0.250	1.930	2.375	2.972	1.131	1.841
11–15	2.100	2.236	0.000	2.236	2.350	2.950	1.008	1.942

Notes: Entries report unweighted session averages (across sessions with at least one observation in that cell; boundary sessions match Table 34) of player 2's gross period 2 consumption payoff and player 3's gross period 3 endgame payoff conditional on whether a token is offered. "Mean" reports the unconditional average and "Gain" reports the difference between offering and not offering a token.

Table 36: Unconditional Frequency of (X, X) in M-1 versus M-1*

	Average (X, X) frequency		WMW p-value
Rounds	M-1	M-1*	M-1 vs. M-1*
All	0.683	0.650	0.971
1–5	0.758	0.517	0.200
6–15	0.646	0.717	0.686
11–15	0.688	0.717	0.886

Notes: Entries report unweighted session averages ($N = 4$ sessions per treatment) and exact two-sided WMW p-values.

Table 37: Endgame Outcome Frequencies by Session in Treatment M-1*

	Rounds 1–5				Rounds 6–15			
	(X,X)	(X,Y)	(Y,X)	(Y,Y)	(X,X)	(X,Y)	(Y,X)	(Y,Y)
Average	**0.517**	**0.183**	**0.050**	**0.250**	**0.717**	**0.042**	**0.017**	**0.225**
Session 1	0.667	0.133	0.133	0.067	0.867	0.033	0.033	0.067
Session 2	0.600	0.200	0.067	0.133	0.800	0.100	0.033	0.067
Session 3	0.533	0.000	0.000	0.467	0.633	0.000	0.000	0.367
Session 4	0.267	0.400	0.000	0.333	0.567	0.033	0.000	0.400
	Rounds 11–15				All Rounds			
Average	**0.717**	**0.050**	**0.033**	**0.200**	**0.650**	**0.089**	**0.028**	**0.233**
Session 1	0.933	0.000	0.067	0.000	0.800	0.067	0.067	0.067
Session 2	0.733	0.133	0.067	0.067	0.733	0.133	0.044	0.089
Session 3	0.667	0.000	0.000	0.333	0.600	0.000	0.000	0.400
Session 4	0.533	0.067	0.000	0.400	0.467	0.156	0.000	0.378

Notes: Bold rows report unweighted averages across the four sessions in M-1*.