Preordered Service in Contract Enforcement

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Paper number 17/04

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June 5, 2017

Abstract

We propose a procedural rule that we refer to as preordered service to replace sequential service of civil cases for breach of contract. The judiciary preannounces a list that ranks all entities that may enter contracts by some uniquely identifying information, such as taxpayer numbers. Courts use this list to enforce the contracts of the highest ranked entities that file a contract case. In theory, unlike sequential service, preordered service ensures efficiency in a population of investment games. Results from a laboratory experiment suggest that it may substantially reduce the caseload at courts and mitigate payoff inequality.

Keywords: Judicial system, courts, judiciary performance, legal procedure, civil cases, caseload, contract enforcement, population of investment games, experiments.
JEL classification: K00, K12, K40, O17, C92.

1 Introduction

An ideal legal system that is conducive to economic prosperity is one that, among other things, resolves disputes without lengthy delay (see Posner (1998)). In practice, however, many countries' civil courts experience long delay and large case backlog (Agor et al. (2015), Esposito et al. (2014), Palumbo et al. (2013)). The bulk of the caseload at civil courts are low-profile cases for breach of contract. In 2012–13, in some U.S. state courts, almost two-thirds of all non-domestic civil cases were contract disputes and, when recorded, damage awards in those disputes averaged at less than $10,000.[1] The time and resources it takes to enforce contract rights even in low-profile cases may discourage investment into profitable business and impede economic prosperity.[2] What reforms can help?

Posner (1998) argues that in many circumstances reforms towards an ideal legal system should initially focus on devising simple rules that the judicial institutions in place can implement.[3] Only at a later stage should more costly investments into improvements of the institutions themselves be made. In this vein, we propose a procedural rule for queuing civil cases for breach of contract to replace sequential service, which processes cases in order of arrival. Our rule amounts to preordered service. It aims at ultimately reducing the number of cases submitted to the judiciary. Fewer cases to look at can help to reduce both waiting times and the costs of the legal system (Palumbo et al. (2013), Esposito et al. (2014)). Once the incoming caseload is reduced, courts can address the case backlog.

Preordered service requires the judiciary to list all entities that may enter contracts and to order them by some uniquely identifying information, such as taxpayer numbers, in any way it likes. It announces this list publicly at the beginning of a given period of time, say a calendar month. It commits to serving civil cases for breach of contract filed within this period in the order of that list, those associated with the highest ranked entities first. In practice, courts would collect all claims filed during the specified period. At the end of the period, they would rank the claims according to the list and serve the highest ranked ones first. That is, courts line up breached contracts using a preannounced order and enforce the first ones in line.

We present preordered service in a stylized economy that consists of many investors and many entrepreneurs. While entrepreneurs have productive projects, investors are endowed with the capital required to implement them. Investors and entrepreneurs are randomly matched with

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[1] According to Agor et al. (2015), 64% of all (925,344—about 5% of U.S. state civil cases) non-domestic civil cases disposed between July 1, 2012 and June 30, 2013 in 152 courts in 10 urban counties were contract cases. Despite fluctuations, this share tends to be above 50% over time (p. 36). The mean of recorded damage awards was $9,428; award values at the 25th, 50th, and 75th percentiles were $1,251, $2,272, and $4,981, respectively.

[2] In this regard, more generally, see Jappelli et al. (2005) on lending activity, Nunn (2007) on specialization, Fabbri (2010) on firm size and finance, and Chemin (2012) on contract breaches, investment, and finance.

[3] Hay et al. (1996) describe a dysfunctional legal system instead to then provide a similar argument.

one another and can enter a contract to use the investor's capital in the entrepreneur's project. After production, however, the entrepreneur can breach the contract and keep all the output, in which case a claim is filed with the court. For simplicity, we assume that the court can enforce exactly one claim and imposes a fine on the respective breaching party; all other claims go unenforced. This simplification is an extreme version of costs and gains to the respective parties to a breached contract that are associated with long court delays.

In this environment, we represent sequential service by a random draw of one breached contract for enforcement. This assumption captures the idea that civil cases queue randomly when filed, and only the first in line is enforced. Modeled in this way, sequential service can lead to inefficiencies: some investors do not invest because all contracts are breached, but not all are enforced. In contrast, we model preordered service using an ad hoc label to represent some uniquely identifying information relating to the contract or the parties to it. This way we capture the idea that, to do its job the best it can, the judiciary could in principle make use of the identities of the parties to a dispute. The ranking of potential contracts by these labels is announced before they are signed. The judiciary then enforces the highest ranked contract that is breached. In this environment, preordered service achieves efficiency. The intuition is that no entrepreneur wants to be associated with the highest ranked contract that is breached. Thus, in equilibrium, no entrepreneur breaches the contract. The credible promise to enforce one contract therefore deters breach altogether. It thereby ensures that all investors can and do safely enter contracts with entrepreneurs, which maximizes aggregate production and consumption.

This theoretical success of preordered service builds on unraveling of iterated elimination of dominated strategies and backward induction. Experimental evidence suggests that this fact may undermine its effectiveness in practice (e.g., Nagel (1995), McKelvey and Palfrey (1992), respectively), which we cannot examine in the field as preordered service is counterfactual. Therefore, we test its performance in a laboratory experiment. We implement our environment and undertake ceteris paribus comparisons of individual and aggregate behavior with preordered and sequential service. We find that, over the course of the experiment, the court caseload is more than 40 percent lower with preordered service than it is with sequential service. In the last round of the experiment, preordered service results in half as many court cases as sequential service. While investment levels do not differ across both rules, with preordered service, as fewer contracts are breached, investors on average secure a higher payoff.

We discuss related literature in Section 2 and present our stylized environment along with its predictions in Section 3. In Section 4, we report the experimental design, our hypotheses, and our empirical findings. We conclude with a few practical considerations in Section 5.

2 Relation to the Literature

We focus on a large inflow of civil cases as one source of court delay and case backlog in the judicial system. Our procedural rule, preordered service, reduces the case inflow and thus the workload facing the judiciary. A related source of delay and backlog is the case flow time—the time it takes to conclude a case. Building on insights of Coviello et al. (2014, 2015) into task juggling, Bray et al. (2016) take an operations management perspective to reduce case flow times in a field experiment in an Italian court. They focus on scheduling the sequence of hearings a case requires, when many cases are in progress at the same time, taking the inflow of cases as given. By contrast, our focus is on scheduling initial hearings—i.e., the order in which cases are opened—to affect the inflow of cases, taking case flow times as given.

We model contractual relationships with an option to breach similar to the underlying game in Bohnet et al. (2001), who modify the original investment game introduced by Berg et al. (1995). We use a population of such investment relationships and think of it as a stylized economy with aggregate output. When service is sequential, the probability of any one breached contract to be enforced—i.e., to arrive first in line—is endogenous. Of course, the patterns of economic interactions are more complex than we model them. Basu et al. (2009) let many investors and many entrepreneurs have multiple investment relationships at the same time to study record keeping and trust. Preordered service can incorporate multiple investment relationships in a straightforward fashion. For example, contracts can be ranked lexicographically, where the first and second coordinates of the label are the respective taxpayer numbers of the associated investor and entrepreneur. This approach also works with financial intermediaries.

Our focus is on civil cases for breach of contract. Shavell (1980, 1984) discusses contracts, breach, and remedies. However, preordered service can be adopted whenever accurate reports of violations can be generated at low enough costs. In terms of Mookherjee and Png (1992)'s analysis of costly monitoring versus investigation of violations of law or regulation, we focus on the investigation of violations, which in our set up are reported with probability one. However, since reports are accurate and investigation costs are low in our environment, Mookherjee and Png (1992) would in fact suggest to use only investigation. In their terminology, we describe and compare preordered and sequential investigation.

In the literature on public goods games, Andreoni and Gee (2012, 2015) and Kamijo et al. (2014) study centralized punishment institutions to sanction non-cooperative behavior that induce a similar strategic interaction. The institutions in these papers punish the least cooperative player. Thus, in equilibrium, all players contribute just enough to the public good in order not to be the least cooperative player. In such strategic games with small self-governing groups, the authors argue, this sort of centralized punishment institution comes quite naturally. However, it comes with the caveat that it requires information about the contributions

of all players in order to rank them and calculate the fines. By contrast, while preordered service might not come quite as naturally—and to some may even appear unfair at first—it requires only little information: it relies solely on a report being filed if and when an individual or an entity has violated a convention, a law, a regulation, or a contract. A notification that a violation occurred is enough. In principle, while it can be used, the degree of the violation is irrelevant. Therefore, as preordered service can make use of existing uniquely identifying information, such as taxpayer numbers, the additional administrative costs of introducing it are likely to be manageable. What is more, we show that it has bite across a population of otherwise strategically independent games.

3   The Model and Predictions

In this section, we discuss a stylized environment with many contracts. A breached contract generates a claim with the judiciary. We focus on an extreme case in which only one such claim is enforced. We consider two alternative procedural rules labeled sequential service and preordered service, respectively. We model sequential service at the courts as a random draw of one claim for enforcement: upon filing, a claim's position in the queue is random and only the first claim in line is served. We model preordered service as enforcing the first claim on a preordered list of all claims: all claims filed are ranked according to a preannounced order of labels that are associated with the contracts. We interpret this environment as an extreme version of a richer one in which the prospect of a potential claim being served first at the courts, rather than somewhere down the line and later in time, tips entrepreneurs over from breaching the contract to honoring it. We use "enforcement" to mean that the courts hold hearings and issue a judgment that can then be used in enforcement proceedings. Both preordered and sequential service therefore are procedural rules pertaining to the order of initial hearings of court cases. The main point to take away is that, while sequential service allows for inefficient outcomes, preordered service ensures the efficient outcome.

We describe the environment in Section 3.1, present its predictions in Sections 3.2–3.4, and provide a discussion of our assumptions, interpretations, and predictions in Section 3.5.

3.1   The Environment

There are $2n$ risk neutral agents, n investors and n entrepreneurs. Investors are endowed with one unit of productive capital each that they can either rent out or convert into one unit of the consumption good. Entrepreneurs are endowed with one unit of time each that they can allocate indivisibly to one of two uses. They can either produce one unit of the consumption good or implement a project that requires one unit of capital. The project is risk-free and converts one unit of capital into $2w$ units of the consumption good, where $n > w \geq 2$.

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Investors and entrepreneurs are randomly paired. Before any interaction, the n pairs are assigned distinct labels $z_1, \ldots, z_n$. These labels could be associated with only one party to the pair, such as a taxpayer number. All agents know the label of their pair. The investor in a pair moves first and decides whether or not to enter a contract with the entrepreneur in that pair. All investors move simultaneously and all entrepreneurs observe all moves. If the investor in a pair declines, then both parties separately produce one unit of the consumption good. If the investor enters the contract, then the entrepreneur implements the project and produces $2w$ units of the consumption good. The terms of the contract are fixed exogenously to sharing the project's output equally.

Then, all entrepreneurs move simultaneously and decide whether or not to honor the contract, if they have one. An entrepreneur can breach the contract by keeping all the output. In this case, a claim is filed with the judiciary. The judiciary selects only one such claim, using a procedure described below, and enforces it with certainty. A potential claim associated with a pair is enforced with endogenous probability $p \in [0, 1]$. If a claim is enforced, then the pair's investor receives the promised payment w and the pair's entrepreneur is fined $f \in (0, w)$.

The game as described is common knowledge among all agents. In autarky, all agents produce separately and aggregate production and consumption equal $2 \cdot n$. In the efficient outcome, all contracts are entered and honored; aggregate consumption equals maximum aggregate output $2 \cdot n \cdot w$ and is thus maximized. Figure 1 depicts the game tree for the interaction between one investor and one entrepreneur. The triplet (n, w, f) summarizes the economic fundamentals. We focus on pure strategy, subgame perfect equilibria.

For reference throughout, as $f < w$, a contract is honored if and only if

$$w \geq p(w - f) + (1 - p)2w \quad \Leftrightarrow \quad p \geq \frac{w}{w + f} \equiv \bar{p} > \frac{1}{2}.$$

Even if a contract is not honored, as $w \geq 2$, an investor enters the contract if and only if

$$pw \geq 1 \quad \Leftrightarrow \quad p \geq \frac{1}{w} \equiv \underline{p} \quad \text{and} \quad \underline{p} \leq \frac{1}{2}.$$

3.2   No Service

As a benchmark, we briefly discuss the predictions when there is no enforcement whatsoever. With no enforcement whatsoever, $p = 0$, no contract is entered, and no project is implemented.

Proposition 1 (NONE)**.** *With no service, the unique equilibrium is autarky.*

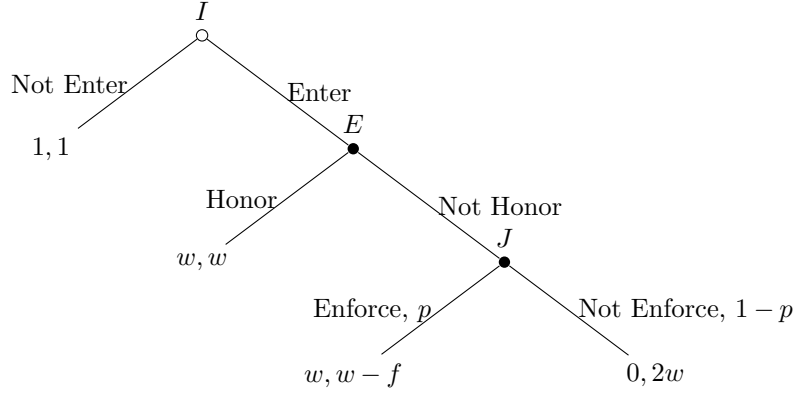*Proof.* As $p = 0$, each investment game's unique equilibrium is (Not Enter, Not Honor).   ∎

Figure 1: The investor I first decides whether or not to enter the exogenously specified contract with the entrepreneur E. If I enters the contract, then E implements the productive project. E then decides whether or not to honor the terms of the contract. If E breaches it, then the judiciary J may or may not enforce the contract. If J enforces the contract, then I is paid and E pays a fine. If J does not enforce the contract, then E keeps all output.

However, as soon as one claim is enforced, autarky is not an equilibrium anymore, irrespective of what n is. The contract of an investor that deviates to entering is either honored or enforced.

3.3 Sequential Service

With sequential service, only the first claim in line for enforcement is served. As a filed claim's position in the queue for service is random, one claim is randomly selected for enforcement, with equal probability for each claim. Let $x \geq 0$ be the number of all claims filed with the judiciary. If $x \geq 1$, then the probability of any individual claim to be enforced is $p = \frac{1}{x}$.[4]

Proposition 2 (SEQSERVE). *With sequential service, there is an efficient equilibrium as well as inefficient equilibria in which $\lfloor w \rfloor < n$ contracts are entered and not honored.*

Proof. First, consider the strategy profile in which all entrepreneurs always honor the contract if they have one and all investors enter it. In any subgame starting after the investors have moved, for all entrepreneurs with a contract, deviating to not honoring the contract implies that the resulting claim is enforced and the fine is incurred with certainty. For all investors, deviating to not entering the contract implies a strictly smaller payoff. Thus, the efficient outcome is an equilibrium. Second, consider any strategy profile in which all entrepreneurs honor the contract if and only if their contract is the only one that was entered and some $\lfloor w \rfloor < n$ investors enter the contract, while the other $n - \lfloor w \rfloor$ investors do not. In any subgame

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[4]The probability of an individual case to be served (i.e., to arrive first in line) equals the number of all permutations of the labels associated with claims filed that have that case listed first, $(x-1)!$, divided by the number of all permutations of those labels, $x!$.

starting after the investors have moved in which only one entrepreneur has a contract, that entrepreneur cannot profitably deviate to not honoring it, as the resulting claim is enforced and the fine is incurred with certainty. In any subgame starting after the investors have moved in which at least two entrepreneurs have a contract, the number of claims filed is $x \geq 2$, implying that $p \leq \frac{1}{2} < \bar{p}$, so that no entrepreneur can profitably deviate to honoring it. Therefore, as $\lfloor w \rfloor \geq 2$ investors enter contracts, the number of claims is $x = \lfloor w \rfloor \geq 2$. Thus, $p = \frac{1}{\lfloor w \rfloor} \geq \frac{1}{w} = \underline{p}$ and no entering investor can profitably deviate to not entering the contract. For all investors not entering, deviating to entering the contract implies an enforcement probability $p' = \frac{1}{\lfloor w \rfloor + 1} < \frac{1}{w} = \underline{p}$ so that it is not profitable. Therefore, any such strategy profile is an equilibrium with less than maximum aggregate output and consumption. ∎

With sequential service, the efficient outcome is an equilibrium because enforcement and imposition of the fine are certain if only one entrepreneur decides to breach. However, sequential service also allows for inefficient equilibria. In these equilibria, investment is inefficiently low and all entrepreneurs that have a contract breach it. If enough entrepreneurs breach, then enforcement and imposition of the fine become unlikely enough for all entrepreneurs that have a contract to breach it. At the same time, enforcement is still likely enough to motivate some investors to enter the contract. We are not interested in equilibrium selection: we simply wish to show that sequential service can lead to inefficient outcomes. In the next section, we contrast this prediction with the finding that preordered service ensures efficiency.

3.4 Preordered Service

With preordered service, at the outset, the judiciary generates and announces an ordered list of the labels $z_1, \ldots, z_n$. Every claim is associated with a breached contract, the parties to which belong to a pair identified by one such label. The judiciary then uses the ordered list of labels to order the claims and enforces the first claim in line.

Proposition 3 (PreOrder). *With preordered service, the unique equilibrium is efficient.*

Proof. First, in any subgame starting after the investors have moved, the unique equilibrium is that all entrepreneurs that have a contract honor it. Suppose for a contradiction that there is at least one entrepreneur with a contract who breaches. Then, there is a claim that is first in line for enforcement. This claim is enforced with certainty. The associated entrepreneur incurs the fine and can profitably deviate to honoring the contract, a contradiction. Thus, all entered contracts must be honored. Given that all entrepreneurs with a contract honor it, no entrepreneur with a contract can profitably deviate to not honoring it as the fine would be incurred with certainty. Second, all contracts are entered. Suppose for a contradiction that some investor does not enter the contract. As all entered contracts are honored, the investor not entering the contract can profitably deviate to entering since a potential claim would

be enforced with certainty, a contradiction. Hence, all contracts are entered. As all entered contracts are honored, deviating to not entering the contract is not profitable for any investor because it decreases the payoff. Thus, the strategy profile in which all entrepreneurs always honor the contract if they have one and all investors enter it is the unique equilibrium. ∎

Preordered service eliminates the inefficient equilibria. Using a preannounced ordered list to line up breached contracts and enforce the first in line induces full compliance; enforcement is not needed. The intuition is as follows. The entrepreneur whose contract is listed first does not breach it because enforcement and the fine are certain. The entrepreneur whose contract is listed second understands this reasoning and concludes that therefore, upon breach, enforcement and the fine are certain. As this entrepreneur thus does not breach the contract, the same argument holds for the entrepreneur whose contract is listed next—and so on. In conclusion, all contracts are honored. Therefore, all contracts are entered and aggregate production and consumption are maximized. Compared to some outcomes with sequential service, preordered service reduces the case inflow and thus caseload facing the courts to zero. These implications carry over to many interesting environments, in which, with sequential service, at least some entrepreneurs breach their contracts, despite costs incurred upon enforcement.

3.5 Discussion

Preordered service requires the judiciary to commit to a preordered list of economic agents, and thus potential associated contracts, before any interaction takes place. Every such preordered list initiates a game that has a unique efficient equilibrium. By contrast, sequential service assigns a claim's place in the queue for service at random. It amounts to a move by nature that draws the ordered list according to which claims queue from all possible such lists and reveals it to all agents only after they interacted. With sequential service, therefore, agents interact while uncertain about which list they are facing. Compared to preordered service, they do not know which game was initiated. As a result, there are many equilibria, and many of them are inefficient. We thus suggest for the judiciary to break the uncertainty by committing to one of the possible lists. However, it is not sufficient to announce that a list will be used to order claims; the courts must announce the specific list they will refer to.

We present preordered service and contrast it with sequential service in a static, stylized model. Our analysis focuses on low-profile civil cases for breach of contract, which constitute the bulk of the caseload in civil courts. We assume that claims are either enforced in this period or not all. To the extent that preordered and sequential service determine the order in which arriving court cases are served, this all-or-nothing assumption can be interpreted as an extreme version of discounting the benefits and costs of a breached contract being enforced at some point in the future. The exact terms of the contracts, which we fix exogenously,

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are not important as long as there is a meaningful trade-off between breaching and honoring the contract and sequential service allows for an inefficient equilibrium. More generally, in related but richer environments of interest, breach of contract occurs and sequential service generates waiting times that drive the costs and benefits of breach to the respective parties. Preordered service then improves the outcome as long as the payoff from honoring the contract is larger than that of breaching it and being served first at the courts and thus being held liable immediately. That is, preordered service is effective in environments in which the prospect of being served first rather than somewhere down the line tips entrepreneurs over from breaching the contract to honoring it. As in our environment, no entrepreneur would want the case associated with their contract be the first in line.

Some of the claims filed with the courts initially might be settled later, somewhere along the case's way through the courts, before a judgment is issued.[5] The probability p of enforcement in our environment can be interpreted as the probability of the case being disposed by the court (in this period), be it by judgment or by settlement. The payoffs upon enforcement (settlement) could easily be altered to account for a potential haircut associated with settlement, or disposal of the case more generally, without affecting the strategic interaction and predictions. Settlements of claims filed with the courts do suffer from court delay when they come about only after initial or even multiple hearings. With regards to the implementation of preordered service, when more than one case is served in a period, settled cases simply vacate a spot in the queue. If settlement currently takes place with a large haircut because the delay until court service is too long, then preordered service should lead to fewer settlements of filed claims as court delay decreases.

We assume that the intent of courts and their agents to enforce contracts does not compete with other objectives, such as political values (see Landes and Posner (2009)). More generally, of course, judges are subject to incentives posed by the environment they work in (see Posner (2005)). The possibility of convergence to efficient law has been discussed by Gennaioli and Shleifer (2007) and studied empirically by Niblett et al. (2010). We further assume that, upon enforcement of the contract, the investor is fully compensated. We do not take a stand on what form that compensation takes. Schwartz (1979) and Shavell (1980, 1984) discuss damages and specific performance as remedies for breach of contract. In addition, the fine the breaching party incurs upon enforcement gives rise to a trade-off between breaching and honoring the contract.

If, for whatever reason, some number of court cases is being filed irrespective of whether or not the contract was breached, then we would have to require that the courts have the capac-

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[5]Agor et al. (2015) report that about 7% of the contract cases in their data were settled; some settled contract cases might have been reported as dismissed.

ity to serve at least one more case than that quantity. These cases would of course be filed irrespective of the procedural rule in place. As long as sequential service allows for inefficient outcomes, our theoretical predictions hold. By introducing a fine for investors whose claims are found to be unsubstantiated, preordered service could also deal with intentionally false claims, if they were allowed. Either the entrepreneur or the investor associated with the first claim on the list faces a fine and would prefer for the claim not to exist.

The court's ruling is certain. Risk aversion thus plays no role when service is preordered. If a contract is first in the preordered list, then enforcement and imposition of the fine are certain upon breach. As long as sequential service allows for an inefficient equilibrium, preordered service compares favorably to it. If the court's ruling were uncertain, then there might be a role for risk aversion. However, with sequential service, a probability of enforcement upon service combines with the probability of arriving first in line and thus of service. By contrast, for the first contract on the preordered list, the probability of a potential court case being served is one. Therefore, the probability of enforcement and thus imposition of the fine is higher when service is preordered than when it is sequential. Given a probability of enforcement upon service and a specified fine, the theoretical predictions then remain unaffected for certain levels of risk aversion.

The nature of our predictions is largely unaffected when allowing for heterogeneous projects. All else unchanged, let there be two types of projects, $n_L \geq 1$ projects with low productivity $w_L > 1$ and $n_H \geq 2$ projects with high productivity $w_H \geq 2$, $\lfloor w_H \rfloor > \lfloor w_L \rfloor$. Assume that $n_L + n_H > w_L$. With one claim enforced, the efficient outcome is always an equilibrium. With preordered service, it is the unique equilibrium. With sequential service, an inefficient equilibrium exists. If $n_H > w_H$, then $\lfloor w_H \rfloor$ projects with productivity w_H are implemented, all contracts are breached, and one is enforced. If $n_H \leq w_H$, then there are two cases. If $w_L < n_H + 1$, then only the n_H projects with productivity w_H are implemented. If $w_L \geq n_H + 1$, then a total of $\lfloor w_L \rfloor$ projects are implemented. In both cases, all contracts are breached and one is enforced. As $n_L + n_H > w_L \geq \lfloor w_L \rfloor$, production is inefficiently low.

Finally, regarding our parameter choices, it can be verified that with $w < 2$, the unique equilibrium with sequential service is efficient. Similarly, it can be verified that with $w \geq n$, in the inefficient equilibrium with sequential service, while no contract is honored, all contracts are entered and thus aggregate production is maximized. This equilibrium is inefficient because one claim is enforced and the fine is incurred so that aggregate consumption is not maximized. With a focus on the case of many contracts, our parameterization, $n > w \geq 2$, rules out these uninteresting cases.

4 Evidence

For preordered service to be effective, it requires individuals to iteratively eliminate dominated strategies and backward induct. As we do not observe preordered service in practice, we ran a laboratory experiment to verify whether or not, compared to sequential service, preordered service may reduce the number of contract cases filed with the courts. We describe the experimental design in Section 4.1, state our hypotheses in Section 4.2, and report our findings in Section 4.3. We discuss and interpret our empirical findings in Section 4.4.

4.1 Experimental Design

Our experiment has three treatments that implement the model studied in Section 3, comprising a population of the game described in Figure 1. The exogenous contract terms allow us to focus on the strategic interaction the different procedural rules give rise to. The payoffs to both players are equal when there is no investment, as well as when the investor enters the contract and the entrepreneur honors it. Thus, aversion to inequality in payoffs cannot be a potential motive for investors' actions.[6] The binary action set of both players lends conceptual simplicity. If the investor had a continuous investment decision as in the standard trust game, then the entrepreneur would have to have a choice to decline the contract: the entrepreneur's payoff in the case of no investment might be greater than the payoff implied by a very small investment. Such additional complications would be a distraction.

When describing the game in the experimental materials we avoided the terminology used in the paper. Instead, we used generic labels for both players, as well as their actions. In all experimental sessions, we randomly assigned subjects to one of two roles: Player A and Player B. Each Player A was given a label, A1 to A8, which he or she kept for the duration of the experiment; Player Bs had no labels. At the start of each experimental round, Player B was told the label for the Player A he or she was matched with. Player A had the option to *Enter* or to *Stay*; in the latter case, both players got a payoff of 1 Experimental Currency Unit (ECU). If Player A chose *Enter*, then Player B had the option of either *Send* or *Keep*. The former option gave each player 2 ECU; the latter option gave 0 ECU to Player A and 4 ECU to Player B if the contract was not enforced, and 2 ECU to Player A and 1.8 ECU to Player B if the contract was enforced. In terms of our model, this corresponds to $w = 2$ and $f = 0.2$, or 10% of Player B's share of the surplus.

The three treatments differ in the way we operationalize the probability of enforcing a claim in the event of Player B choosing *Keep*. Our baseline condition is the NONE treatment, in

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[6]Bohnet and Huck (2004) and Bohnet et al. (2005) study binary trust games in which the no-investment outcome has first-movers earning more than second-movers. In contrast, the no-investment outcome in Bohnet et al. (2001) is zero for both players.

Table 1: Experimental Design

	NONE	SEQSERVE	PREORDER
# subjects	96	96	96
# economies	6	6	6

which the probability of enforcement, p, is set to zero. This treatment corresponds to the case where there is no contract enforcement. Its purpose is to establish the baseline level of investment and compliance—the extent to which entered contracts are being honored—in our sample. The SEQSERVE treatment introduces enforcement of exactly one claim, which is chosen at random from the pool of claims in a given experimental round. This treatment captures sequential service at courts: claims are processed on a first-come-first-served basis and a claim's spot in the queue is random. Finally, the PREORDER treatment implements preordered service. All pairs in which Player B chose *Keep* in a given round are ranked in descending order of Player A label. The pair whose Player A has the highest label is selected for enforcement with certainty, and all others are not selected. Table 1 outlines the experimental design, and the sample size in each treatment. The unit of observation is an economy, which consists of eight investor-entrepreneur pairs. We collected six economies per treatment.

4.1.1 Information and Feedback

We assigned labels to Player As because they are essential to operationalize the PREORDER treatment; Player As in NONE and SEQSERVE also had labels for consistency. Individual Player As kept their label throughout the experiment for two reasons. First, had we randomly assigned Player A labels every round, then the positions of Player As on the list in PREORDER would have changed every round. That is, while the strategic environment for Player As is unchanged from round to round in SEQSERVE and NONE, it would have changed every round for Player As in PREORDER. By fixing labels, we made the strategic environment stationary for Player As in all three treatments. Second, fixed labels are also realistic, as most identifying information for individuals and firms (e.g., taxpayer numbers) are unique to those individuals and do not vary. We did not assign labels to Player Bs because we did not want to introduce reputation concerns: if Player As could keep a history of past interactions for each Player B in the economy, they would be able to discriminate between trustworthy and untrustworthy Player Bs. This discrimination in turn can work as an extremely effective disciplining device, even in finitely repeated trust games (Bohnet and Huck (2004)).

In all treatments, the end-of-round feedback screen subjects saw had two separate sections: one section had information about the round for all pairs in the economy, and the other had information about the outcome for that subject's pair (see Appendix C). The former section

provided information about the 'Number of Player Bs that chose *Keep*' and the 'Number of Pairs *Selected*'.[7] This feedback levels the playing field from an informational point of view for all subjects within a treatment, as well as across treatments. Even though our experiment simulates a one-shot environment, subjects can still learn about the population of players with whom they interact. Therefore, the informational environment should provide the same conditions for learning in the three treatments. This is particularly so in cases where the investor enters the contract, the entrepreneur breaches it, and their contract is enforced. In PREORDER, both players can immediately infer that either there were no instances of contract breach for any pair whose Player A had a higher label and invested, or that no Player A with a higher label invested. That is, those two players can infer an upper bound on the number of breaches in a round. This inference is not possible in SEQSERVE. Providing the total number of breaches in the economy in that round levels the information subjects can use to learn from round to round in both treatments.

4.1.2 Procedures

We implemented economies with eight pairs. We ran 10 experimental sessions, eight of which had 32 participants while two sessions had 16 participants for a total of 288 participants. In each of the 32-participant sessions, we ran two separate economies in parallel.[8] In each session, subjects sat at a booth which did not allow for visual or verbal communication. The experimenter handed written copies of the instructions, which included a quiz to check for understanding.[9] Subjects read the instructions and completed the quiz in their own time; the experimenter checked their answers and clarified any questions individually if necessary. Once all subjects had their questions checked, the experiment started. There were two practice rounds so that subjects could get familiarized with the software interface. After the second practice round ended, the software informed subjects that the incentivized part would begin. The incentivized part of the experiment consisted of 20 rounds, three of which were randomly selected by the computer for payment. In all rounds, the computer randomly matched a Player A with a Player B. We programmed the experimental software in Z-Tree (Fischbacher (2007)), and we recruited our subjects from a pool of undergraduate students from the University of Exeter from a variety of majors using ORSEE (Greiner (2015)). No subject in the sample took part in more than one session, and nobody in the sample had registered for a trust game experiment before taking part in our experiment. The sessions took place in the FEELE lab at the University of Exeter in October and November 2016. Payoffs in the experiment were denominated in Experimental Currency Units (ECU). One ECU was worth £2; subjects also received £3 for participating. Sessions lasted on average 50 minutes and the average payment

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[7]The second sentence was not included in the feedback screen in the NONE treatment.

[8]A large number of no-shows in one of the scheduled sessions forced us to run the two 16-person sessions.

[9]All instructions as well as screenshots of the experimental interface are collected in Appendices B and C.

was £12.26 ($15.30).

4.2 Hypotheses

The objective of the experiment is to check if preordered service decreases the number of contract cases filed with courts relative to sequential service in an economy consisting of many two-party contracts. As shown in Section 3, the unique equilibrium with preordered service is efficient, while sequential service allows for an efficient equilibrium as well as inefficient equilibria; without enforcement, autarky prevails. Our first hypothesis concerns the caseload faced by courts, and the impact the different procedural rules have on cases filed. We therefore focus on the two treatments where there is a judiciary to enforce claims.

Hypothesis 1 (Caseload)**.** *The caseload faced by courts in* PreOrder *is smaller than or equal to that in* SeqServe*.*

Our next two hypotheses decompose caseload by looking at its two behavioral determinants: entrepreneur compliance, and investors' decision whether or not to invest. The None treatment serves as a no-enforcement benchmark condition.

Hypothesis 2 (Compliance)**.** *Treatments are ranked on the basis of the proportion of contracts being honored:* PreOrder $\geq$ SeqServe $>$ None*.*

Hypothesis 3 (Investment)**.** *Treatments are ranked on the basis of the number of contracts being entered:* PreOrder $\geq$ SeqServe $>$ None*.*

4.3 Empirical Findings

In our analysis of the experimental data we use two complementary approaches. We test average treatment effects using non-parametric statistical tests, using economy averages as independent observations.[10] Panel data econometric techniques, which take advantage of the repeated nature of the experiment, merely confirm the results; as such that analysis is relegated to Appendix A. All p-values refer to two-sided tests. To facilitate the discussion, and to be consistent with the terminology of our model, we refer to the first-mover player (Player A in the experimental instructions) as the investor. Likewise, we refer to the second-mover player (Player B in the experimental instructions) as the entrepreneur. In Sections 4.3.1–4.3.5, we present our results with support. In Section 4.4, we offer an integrated discussion and interpretations.

Figure 2: Caseload. **Top Left:** All rounds. **Top Right:** Last round of the experiment. **Bottom:** Time series.

4.3.1 Caseload

In this section, we compare the caseload generated by economies under the two procedural rules. We compare the average of six observations of the total caseload in an economy, either over 20 rounds or in the final round. The top left panel in Figure 2 displays the caseload in SEQSERVE and PREORDER over the course of the twenty rounds; the top right panel shows the same information for the final round of the experiment. The bottom panel displays average caseload round-by-round.

Result 1 (Caseload). *Caseload is substantially lower in* PREORDER *than in* SEQSERVE.

Support. In Figure 2, we observe a significantly lower (average) caseload in PREORDER than in SEQSERVE. Over the whole sample, there were just over 32 cases filed on average in PREORDER and just over 57 in SEQSERVE ($z = 2.882, p = 0.004$; MW test). In the final round, there were 1.17 cases filed on average in PREORDER and 2.5 in SEQSERVE ($z = 2.331, p = 0.020$; MW test). The analysis of caseload over time corroborates this result. Average caseload is always lower in PREORDER than in SEQSERVE; we also find a negative and significant linear time trend on caseload in both PREORDER ($-0.064, p < 0.001$), and

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[10]We use MW to denote the Mann-Whitney test, WSR to denote the Wilcoxon Signed Ranks test and JT to denote the Jonkheere-Thepstra test.

SEQSERVE ($-0.029, p = 0.003$). However, the linear trend in PREORDER is significantly different from that in SEQSERVE ($F = 7.48, p = 0.019$), indicating a faster rate of decline in caseload in PREORDER. ∎

In the next two sections, we decompose this result; first by looking at average compliance conditional on investment, and then by looking at average investment. We add to the analysis the data from the NONE treatment, which gives us benchmark compliance and investment in the absence of courts.

4.3.2 Compliance

In this section, we compare entrepreneurs' behavior in the three treatments. We take the average of compliance decisions for all eight entrepreneurs in an economy (conditional on investment taking place) over 20 rounds ($N \leq 160$ per economy, since investment did not always happen) as one observation. We have six such observations. Figure 3 depicts the relative frequency with which entrepreneurs complied with the contract, that is, honored it. The top left panel considers all twenty experimental rounds; the top right panel looks at the final round only. The bottom panel depicts average compliance round-by-round.

Result 2 (Compliance)**.** *Average compliance is higher in* PREORDER *than in both* SEQSERVE *and* NONE*. There is no difference in average compliance between* SEQSERVE *and* NONE*.*

Support. In Figure 3, we observe a significantly higher average compliance level in PREORDER than in SEQSERVE or NONE, when considering the whole sample (PREORDER vs SEQSERVE: $z = 2.882, p = 0.004$; PREORDER vs NONE: $z = 2.402, p = 0.016$; MW test), or the final round (PREORDER vs SEQSERVE: $z = 2.887, p = 0.004$; PREORDER vs NONE: $z = 2.917, p = 0.004$; MW test). In contrast, we do not find a significant difference in average compliance between SEQSERVE and NONE (all rounds: $z = 0.000, p = 1.000$; final round: $z = 1.624, p = 0.104$; MW test). The bottom panel of Figure 3 corroborates this result by displaying the evolution of compliance over time. Fitting a linear time trend on the average per round compliance in each treatment, we find a negative and significant trend in NONE ($-0.014, p = 0.004$), a negative, though not significantly different from zero, trend in SEQSERVE ($-0.002, p = 0.191$), and a positive and highly significant time trend in PREORDER ($0.012, p < 0.001$).[11] That is, while compliance declined over time in the NONE treatment—which is consistent with the existing evidence on trust games (e.g., Bohnet and Huck (2004))—compliance remained roughly constant in the SEQSERVE treatment and actually increased over time in the PREORDER treatment. ∎

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[11]All time trends are significantly different from each other at the 5% level or lower. See Table A1 in the Appendix for details.
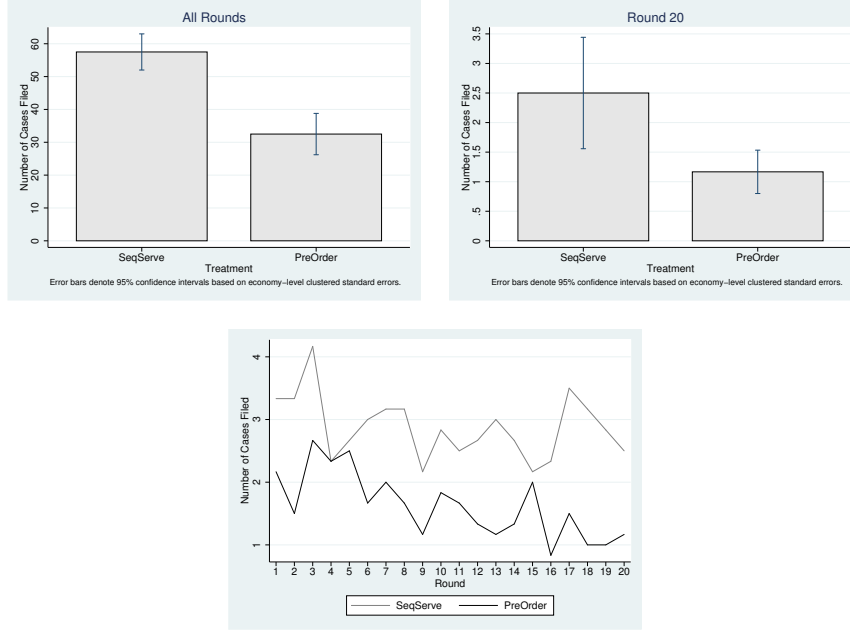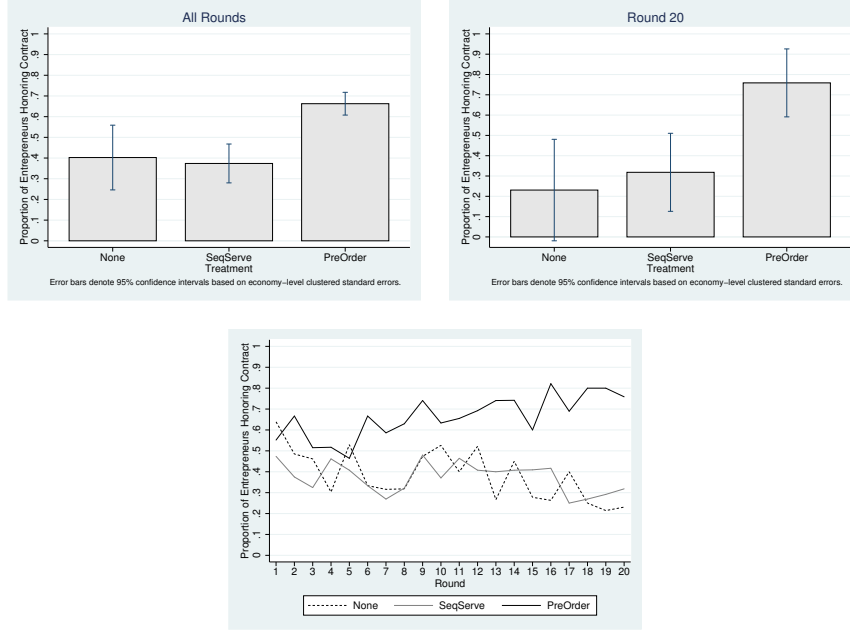
Figure 3: Compliance. **Top Left:** All rounds. **Top Right:** Last round of the experiment. **Bottom:** Time series.

4.3.3 Investment

In this section, we compare investors' behavior in the three treatments. We take the average of investment decisions for all eight investors in an economy over 20 rounds as one observation ($N = 160$ per economy). We have six such observations. Figure 4 depicts the relative frequency with which investors entered into a contract with the entrepreneur. The top left panel considers all twenty experimental rounds; the top right panel looks at the final round only. The bottom panel shows average investment in each experimental round in all three treatments.

Result 3 (Investment). *Average investment is higher in both* PREORDER *and* SEQSERVE *than in* NONE*. There is no difference in average investment between* PREORDER *and* SEQSERVE*.*

Support. In Figure 4, we observe a significantly higher level of investment in PREORDER than in NONE irrespective of the set of rounds under analysis (all rounds: $z = 2.406, p = 0.016$; final round: $z = 2.303, p = 0.021$; MW test). While SEQSERVE outperforms NONE over all rounds, there is no significant difference in average investment in the final round (all rounds: $z = 2.330, p = 0.020$; final round: $z = 1.483, p = 0.138$; MW test). When comparing SEQSERVE to PREORDER, the difference in average investment is not significant (all rounds: $z = 1.000, p = 1.000$; final round: $z = 1.257, p = 0.209$; MW test). However, this belies the evolution of
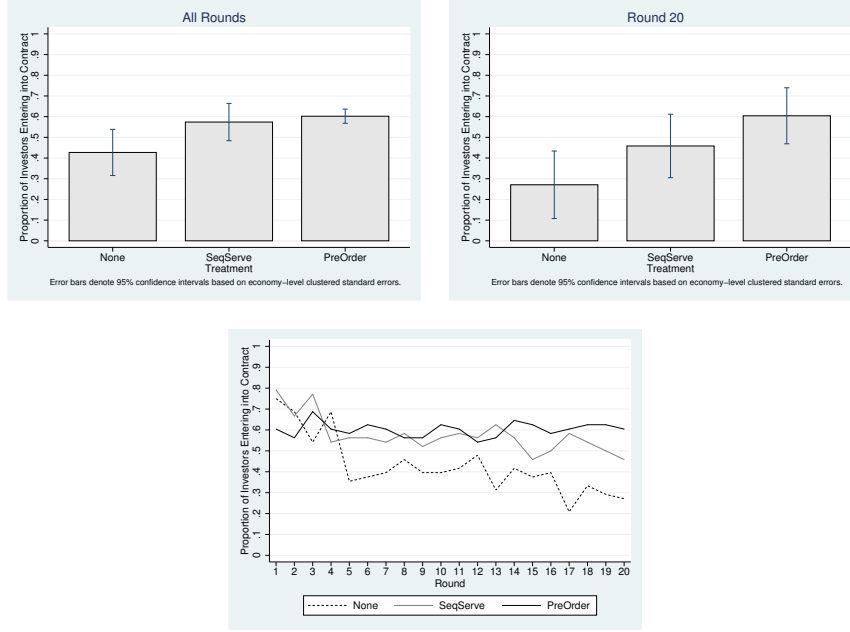
Figure 4: Investment. **Top Left:** All rounds. **Top Right:** Last round of the experiment. **Bottom:** Time series.

average investment over time in the three treatments as depicted in the bottom panel of Figure 4: in NONE, there is a negative and significant $(-0.019, p = 0.002)$ time trend on investment. The time trend on investment in SEQSERVE is also negative and significantly different from zero $(-0.010, p = 0.003)$. In contrast, the linear trend on investment in PREORDER is flat and not significantly different from zero $(0.0003, p = 0.948)$. ∎

4.3.4 Compliance and Investment Conditional on Labels

In this section we look at individual behavior in more detail. A closer look at Figures 3 and 4, in particular the 95% confidence intervals around the sample means, suggests that the variance in both compliance and investment is higher in SEQSERVE than in PREORDER. Figure 5 breaks down investment and compliance by investor label. Column heights represent average investment for each label. The dark portion in each column represents the proportion of cases where the entrepreneurs honored the contract; the light portion represents the proportion of cases where entrepreneurs breached the contract.

Result 4 (Labels)**.**

    a. There is a positive relationship between investment/compliance and investor labels in PREORDER *but not in* NONE *or* SEQSERVE.

b. *The levels of investment and compliance for investors with the six highest labels in* PRE-ORDER *are greater than or equal to those levels for the average investor in* SEQSERVE *and* NONE.

c. *There is less variance in individual investment decisions in* PREORDER *than either in* NONE *or* SEQSERVE.

Support.

a. Visual inspection of Figure 5 suggests that average investment and compliance vary with label in PREORDER but not in NONE or SEQSERVE. To test for a relationship between labels and investment/compliance, we compute the average investment by individual investor, as well as the average compliance experienced by individual investors over twenty rounds in an economy and take that to be our independent unit of observation. We then compute Spearman correlation coefficients (ρ), as well as the (non-parametric) Jonkheere-Thepstra test (JT) for ordered alternatives.[12]

In PREORDER we find evidence of a strong relationship between investor labels and investment. There is a positive correlation between investment and investor labels ($\rho = 0.76, p < 0.001$); the JT test rejects the null of joint equality against the ordered alternative ($J^* = 5.950, p < 0.001$). We also find evidence of a strong relationship between investor labels and compliance. The correlation between compliance and labels is positive and highly significant ($\rho = 0.92, p < 0.001$); the JT test rejects the null of joint equality against the ordered alternative ($J^* = 7.213, p < 0.001$).

In SEQSERVE we find no evidence of a relationship between investor labels and investment. There is a negative but non-significant correlation between investment and investor labels ($\rho = -0.14, p = 0.350$); the JT test does not reject the null of joint equality ($J^* = -0.945$, $p = 0.345$). We also find no evidence of any relationship between investor labels and compliance. The correlation between compliance and labels is essentially zero and non-significant ($\rho = 0.03, p = 0.844$); the JT test does not reject the null of joint equality ($J^* = 0.296$, $p = 0.767$).

In NONE we find very weak evidence of a positive relationship between investor labels and investment. There is a positive, weakly significant correlation between investment and investor labels ($\rho = 0.24, p = 0.098$), but the JT test does not reject the null of joint equality ($J^* = 1.637, p = 0.102$). We find no evidence of any relationship between investor labels and compliance. The correlation between compliance and labels is positive but non-significant ($\rho = 0.16, p = 0.283$); the JT test does not reject the null of joint equality ($J^* = 1.037$, $p = 0.300$).

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[12]The JT test tests for the null hypothesis of joint equality of medians against an ordered alternative hypothesis. The alternative is that the median investment level is increasing or decreasing in the investor labels. We report J^*-statistics that correct for ties.
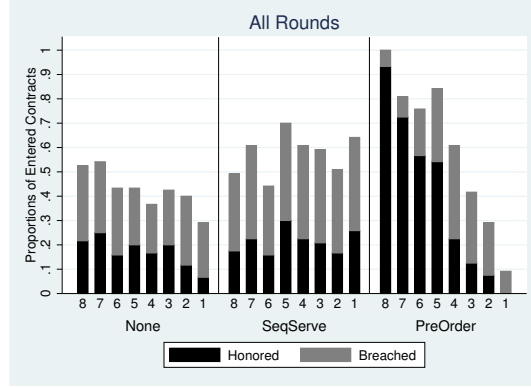
Figure 5: Breakdown of Average Investment and Compliance By Investor Label.

b. We compare the average investment by an investor in PreOrder conditional on a label to the average investment by all investors in None and SeqServe. To do this, we take the average investment over the twenty rounds of the experiment for each investor as the independent observation.[13]

The level of investment by investors with labels A8–A5 in PreOrder is significantly higher than that by investors in None (all comparisons, $z \geq 2.193, p \leq 0.028$, MW test) and SeqServe (A6: $z = 1.740, p = 0.082$; all other comparisons, $z \geq 2.168, p \leq 0.030$, MW test). Also, the level of investment by investors with labels A4–A2 in PreOrder is not significantly different to that by investors in None (all comparisons, $z \leq 1.448, p \geq 0.148$, MW test); the average level of investment by investors with labels A4–A3 in PreOrder is not significantly different to that by investors in SeqServe (all comparisons, $z \leq 1.269, p \geq 0.204$, MW test). In contrast, the level of investment by investors with label A1 in PreOrder is significantly lower than that by investors in None ($z = 3.146, p = 0.002$, MW test). Likewise, the level of investment by investors with labels A2 and A1 in PreOrder is significantly lower than that by investors in SeqServe (all comparisons, $z \geq 2.168, p \leq 0.030$, MW test).

We observe the same pattern of behavior when looking at compliance conditional on investment. The level of compliance experienced by investors with labels A8–A5 in PreOrder is significantly higher than that experienced by investors in None (all comparisons, $z \geq 3.175, p \leq 0.001$, MW test) and SeqServe (all comparisons, $z \geq 3.622, p \leq 0.001$, MW test). The level of compliance experienced by investors with labels A4 and A3 in PreOrder is not significantly different to that experienced by investors in None (all comparisons, $z \leq 0.734, p \geq 0.463$, MW test) or SeqServe (all comparisons, $z \leq 0.868, p \geq 0.385$, MW test). In contrast, average compliance experienced by investors with labels A2 and

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[13]Note that since we are making comparisons across treatments and labels were fixed within an economy, these are statistically independent observations. Also note that the non-parametric tests we employ are robust to unequal sample sizes.
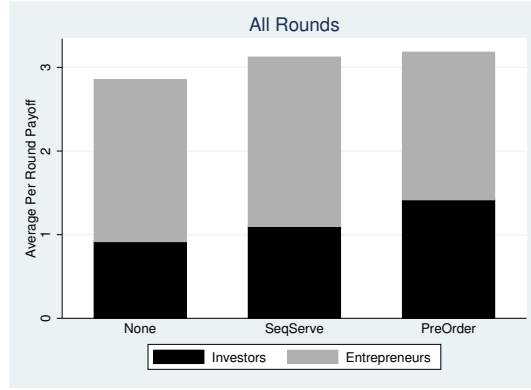
Figure 6: Average Payoffs to Investors and Entrepreneurs.

A1 in PREORDER is significantly lower than that by investors in NONE (all comparisons, $z \geq 1.658, p \leq 0.097$, MW test) or SEQSERVE (all comparisons, $z = 1.936, p \leq 0.053$, MW test).

c. We take advantage of the fact that the investors' decision is a binary variable. Therefore, the sample standard deviation of an individual's investment decisions over the twenty rounds in the experiment is maximized when a given investor invests half of the time (i.e., in ten out of twenty rounds), and it is minimized when an investor either always invests or never invests. We calculate the standard deviation of investment decisions for each investor in our sample. The mean standard deviation in investment decisions in PREORDER is 0.248, and it is significantly lower than that in SEQSERVE ($0.401; z = 4.260, p < 0.001$, MW test) and NONE ($0.403, z = 3.945, p < 0.001$, MW test). In contrast, there is no difference in mean standard deviation between SEQSERVE and NONE ($z = 0.774, p = 0.439$; MW test). ∎

4.3.5 Payoffs

We complete our analysis by examining players' payoffs across the three treatments. Figure 6 depicts average payoffs for both types of players over the 20 rounds. The height of the bar represents average total payoffs. The dark portion of the bar denotes average investor payoffs, and the light portion denotes average entrepreneur payoffs.

Result 5 (Payoffs)**.**

  a. *In all treatments, the average payoff to investors from investing is at least as high as that from not investing.*

  b. *Average investor payoff is higher in both* PREORDER *and* SEQSERVE *than in* NONE. *Average investor payoff is higher in* PREORDER *than in* SEQSERVE.

c. Average entrepreneur payoff is lower in PREORDER *than in* SEQSERVE.

Support.
a. Using the average payoff of all investors in an economy as an independent observation, we test against the null that average payoff is equal to one. We reject the null in the cases of PREORDER ($z = 3.064, p < 0.01$, WSR test) and SEQSERVE ($z = 2.680, p < 0.01$, WSR test), but we cannot reject it in the case of NONE ($z = 1.503, p = 0.133$, WSR test).

b. Using the average investor payoff in an economy as an independent observation, we perform a series of pairwise tests of equality of payoffs. The average investor payoff in PREORDER is 1.42, which is significantly higher than the average investor payoff in SEQSERVE ($1.10, z = 2.882, p < 0.01$, MW test), and significantly higher than the average investor payoff in NONE ($0.93; z = 2.882, p < 0.01$, MW test). In turn, average investor payoff in SEQSERVE is also significantly higher than that in NONE ($z = 2.082, p = 0.04$, MW test).

c. We perform the same exercise using average entrepreneur payoffs. The average entrepreneur payoff in PREORDER is 1.77, which is not significantly different from the average entrepreneur payoff in NONE ($1.94, z = 1.601, p = 0.11$, MW test); it is significantly different from the average entrepreneur payoff in SEQSERVE ($2.02, z = 2.817, p < 0.01$, MW test). Finally, there is no statistically significant difference in average entrepreneur payoffs between SEQSERVE and NONE ($z = 1.506, p = 0.132$, MW test). ∎

4.4   Discussion

The objective of our experiment is to compare the performance of different procedural rules in contract enforcement. We find that preordered service indeed fulfills its primary objective vis-à-vis sequential service, in that it substantially reduces the case inflow facing the judiciary. It does so throughout the experiment, up to the very last round.

To better understand the difference in performance between the two procedural rules, it is useful to look at the benchmark case in which there is no formal procedure for the enforcement of contracts. In this case, the game we consider is a binary version of the trust game introduced by Berg et al. (1995). While theory predicts zero compliance and therefore zero investment, an abundance of evidence from the laboratory and the field suggests that individuals are able to sustain positive levels of cooperation in the trust game, at least in the short run.[14] In our experiment, NONE provides the baseline level of investment and compliance in the absence of any procedure for contract enforcement. Consistent with the existing literature, we observe positive, albeit declining levels of trust and trustworthiness in our sample. How do the two procedural rules do in comparison?

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[14]For a review of evidence from the lab, see Camerer (2003); for field data, see Henrich et al. (2001).

The sequential service of claims implemented in the SeqServe treatment yields a significant improvement in investment levels relative to None, but no significant improvement in compliance. The increase in investment is easily rationalizable: investors have a positive probability of at least 1/8 of being made whole, which makes investment more attractive than in the case where that likelihood is zero. Investors in both treatments earn on average the same payoff; that amount is roughly the same as what they would have earned had they not invested at all. However, average aggregate payoff in both treatments is 50% higher than that if investment had not taken place. Therefore, the behavior by investors could be explained by efficiency preferences (Engelmann and Strobel (2004)).

In contrast, the preordered service of claims implemented in our PreOrder treatment provides a significant improvement in both investment and compliance relative to None. In other words, effective enforcement is not just about the availability of arbitration per se, but also about what procedures govern arbitration itself. Comparing the two procedural rules directly, we find that, over the course of the experiment, preordered service outperforms sequential service in terms of entrepreneur compliance, but not in terms of investment. We did not consider a longer time horizon in the lab, as this could lead to subject fatigue and compromise decision quality. However, the negative time trend in investment in SeqServe and the flat time trend in PreOrder suggest that with a sufficiently long time horizon, PreOrder might have outperformed SeqServe in this dimension as well.

The preordered service rule implemented in PreOrder exhibits sorting characteristics: investors whose labels put them on the top half of the list almost always invested, and on average their contracts where almost always honored. In turn, investors on the very bottom of the list understood that a contract would likely be breached, and rarely invested. In contrast, investment decisions in SeqServe exhibited a very high degree of variability irrespective of labels, not statistically different to that in None. This observation suggests that the presence of enforcement per se is not enough to create an environment that is conducive to investment.

Remarkably, the sorting we observe in PreOrder occurred with only very little information compared to reputation transmission environments that sucessfully promote trust (Bohnet and Huck (2004)). In addition to their own pair's label, players only knew the total number of breaches by entrepreneurs, which at best puts a lower bound on the total level of investment. This lack of detailed information is particularly important when service is preordered. The equilibrium in that case requires players to realize that entrepreneurs should choose to comply, and that therefore investment is safe, which might be easier with detailed information.

Had we provided full disclosure about the outcome in each pair in the economy, the performance in PreOrder might have improved (faster), as subjects would have observed almost

perfect compliance for the pairs with label A8 and A7. Had we not provided any feedback to subjects on breaches by other entrepreneurs in the economy, PreOrder would have had an inherent advantage over SeqServe. In PreOrder, if the entrepreneur in a pair breaches the contract, then the players in that pair can infer from their enforcement outcome the behavior of players in pairs in line before them as well as, potentially, an upper bound on the number of breaches in that round. This sort of inference is not possible in SeqServe. As such, our choice of end-of-round aggregate feedback removed an informational edge inherent to PreOrder.

It is clear that the behavior in the experiment did not match the equilibrium predictions of our model, particularly in the case of PreOrder. That being said, we are not interested in testing theoretical point predictions, but rather treatment effects. While efficiency and reciprocity preferences likely played a role in determining investment and compliance, they do not explain the decline in compliance and investment along the queue. A natural candidate explanation is that subjects in our experiment were boundedly rational, and that they may have had difficulty performing the reasoning inherent to the equilibrium of the model.

However, popular models of boundedly rational agents struggle to explain behavior in this treatment. Take for instance the level-k model proposed by Stahl and Wilson (1995) and Nagel (1995). This model proposes a hierarchy of cognitive types that manifests itself in the ability to play strategically. The least sophisticated type is a level-0 player who selects a strategy at random. A level-1 player assumes all other players are level-0, and best-replies to their profile of actions. A level-2 player assumes all other players are level-1 and best-replies accordingly, and so on. It can be verified that the optimal action by a Player B with sophistication level 1 (and a reasonable degree of risk aversion) in our game for any position below A8 is to breach. By breaching the contract, such a player can either achieve a payoff of 4 ECU with probability 0.5, or a payoff of 1.8 ECU with probability 0.5; by honoring the contract, that player gets a sure payoff of 2 ECU.[15] In turn, a level-2 Player B will find it dominant to breach. As such, any Player B that has a sophistication level greater than or equal to 1 will also optimally breach. As a consequence, the cognitive hierarchy model of Camerer et al. (2004) where a player of level-k believes she is playing against a distribution of types in the support $\{0, \ldots, k-1\}$, makes the same prediction.

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[15]A player would require a coefficient of relative risk aversion of at least 7.53 to prefer compliance; this degree of risk aversion is not only implausible, as argued by Rabin (2000), but also not observed in most laboratory or field experiments measuring individual risk aversion (Biswanger (1980); Holt and Laury (2002)).

5 Concluding Remarks

Long trial times and civil case backlog are evident in many countries. In this paper, we offer a conceptual demonstration of a procedural rule that helps to mitigate an important source of delay and backlog: the large number of civil cases filed. Our procedural rule stipulates that courts line up low-profile contract cases for service using a preannounced ranking by some uniquely identifying information of at least one party to the contract, such as a taxpayer number. We call this procedure preordered service, as compared to sequential service, which lines up civil cases in order of arrival.

We offer proof of concept by presenting preordered service in a stylized environment. We demonstrate that replacing sequential service of low-profile contract cases with preordered service has the potential to reduce the caseload at courts. In an experimental investigation, preordered service reduces the number of contract cases filed with the courts by more than 40 percent compared to sequential service. As contracts are honored more frequently, investors can secure a larger share of the surplus that investment generates.

The additional administrative costs of introducing preordered service in practice are likely to be manageable. Preordered service can make use of existing uniquely identifying information, such as taxpayer numbers. It can equally well use newly randomly generated and assigned numbers. In principle it does not matter whether the identifying information associated with a claim refers to the plaintiff, the defendant, or is a combination of information related to both. Courts can use any randomly generated ordered list of these numbers. There can be many different lists for different contract types—as well as many different lists for the same contract type, in which case each entity can appear on only one of the lists. All of these lists can order the (taxpayer) numbers in the same way, or in different ways.

Regarding concerns that some might have about equal access to justice when service at courts is preordered—which may explain why this sort of procedural rule has not emerged—we submit two observations. First, in theory, while there is a hierarchy of potential contract cases ex ante, in equilibrium, access to justice is equal. Second, to some extent, many legal systems already distinguish between various types of court cases at a general level. Often civil cases are differentiated by the amount claimed. For instance, small claims are typically dealt with by different courts than larger commercial disputes. That is, specialized courts already exist, and they give priority to certain types of cases by definition.

When implementing preordered service, a potential issue, some might hold, is that very long lists may make it more difficult for preordered service to work as theory suggests. However, this issue could be addressed by assigning pairs to smaller batches of cases—these assignments would need to be announced at the outset—and applying the preordered service protocol to

these smaller batches. Courts could also apply subject matter categories to generate smaller batches of cases; lists could be based on contract type, industry, geography or any other relevant characteristic. How many (or how few) lists are necessary to successfully implement preordered service will depend on the legal jurisdiction, legal custom, and the type of claim. Answering this question—which is inevitably an empirical one—is beyond the scope of this paper and straddles economics, public policy, and law.

Our analysis is not suited to discuss the economy's progression when preordered service is first adopted to replace sequential service. However, to some extent the sequence of one-shot games our subjects played in the experiment can be interpreted as one-shot interactions in a stylized economy. In that case, the time series of the investment and breach decisions seems to suggest that the positive effects of preordered service compared to sequential service become more pronounced over time.

There are several avenues for future research. One line of inquiry is to explore whether the provision of more information and a role for reputation can make preordered service even more effective. As the fine for breach was very small in our experiment, one may ask whether higher fines can improve the performance of preordered service. Another natural extension is to rank breached contracts by their value. Low-profile contracts can be ranked in that way prior to their signing, because a number of transactions, like cellphone contracts or housing rental contracts, which are prone to disputes due to non-payment, have standardized values. Contracts could then be ordered by two pieces of information in a lexicographic fashion: first sort contracts by their standardized values and then, second, by the taxpayer number of either one of the parties to it. While this extension requires heterogeneity in contract values, the theoretical predictions would largely remain unaffected. Finally, one could allow players to bargain over the distribution of the project's proceeds prior to investors deciding whether or not to enter into the contract. More generally, once the distribution of the project proceeds is endogenous, one can investigate the implications of preordered service for the distribution of income amongst investors and between investors and entrepreneurs.

Table A1: Estimated Linear Time Trends in Caseload, Compliance, and Investment

DV:	(1) Caseload		(2) Compliance		(3) Investment	
PreOrder	-0.879***	(0.244)	0.048	(0.097)	-0.025	(0.090)
SeqServe			-0.108	(0.105)	0.053	(0.104)
PreOrder × Round	-0.035**	(0.013)	0.026***	(0.005)	0.019***	(0.005)
SeqServe × Round			0.012**	(0.005)	0.009*	(0.005)
Round	-0.029***	(0.008)	-0.014***	(0.004)	-0.019***	(0.004)
Constant	3.179***	(0.138)	0.489***	(0.092)	0.625***	(0.082)
N	240		357†		360	
R^2	0.266		0.297		0.250	

Economy-clustered robust standard errors in parentheses.

***, **, *: $p < 0.01, p < 0.05, p < 0.10$.

†: 3 missing observations correspond to zero investment in those rounds.

A   Additional Econometric Analysis

Table A1 reports OLS regressions of $y \in \{$Caseload, Compliance, Investment$\}$ on a linear time trend for each of the three treatments, using as the unit of observation the average y across all six economies in a given experimental round. In the Caseload regression, we exclude the data from None; the omitted treatment is SeqServe. The Compliance and Investment regressions include data from the three treatments.

In the Caseload regression, we find a negative and significant coefficient on Round, indicating declining caseload levels over time in SeqServe. The coefficient on PreOrder × Round is negative and highly significant, indicating a faster rate of decline in caseload in PreOrder than SeqServe.

In the Compliance regression, we find a negative and significant coefficient on Round, indicating declining compliance levels over time in None. The coefficient on PreOrder × Round is positive and highly significant; furthermore, Round + PreOrder × Round is positive and significantly different from zero ($F(1, 17) = 24.76, p < 0.001$), indicating a positive time trend in compliance in PreOrder. The coefficient on SeqServe × Round is also positive and highly significant; however, Round + SeqServe × Round is negative and not significantly different from zero ($F(1, 17) = 1.85, p = 0.191$), indicating a flat time trend in compliance in SeqServe.

In the Investment regression, we find a negative and significant coefficient on Round, indicating declining investment levels over time in None. The coefficient on PreOrder × Round is positive and highly significant; furthermore, Round + PreOrder × Round is positive and

Table A2: Estimated Determinants of Investment and Compliance Conditional on Labels

DV:	Compliance		Investment	
	(1)	(2)	(3)	(4)
A-Label	0.073**	0.059*	0.099	0.110
	(0.029)	(0.031)	(0.082)	(0.081)
A-Label × SEQSERVE	-0.078*	-0.062	-0.119	-0.125
	(0.043)	(0.046)	(0.093)	(0.095)
A-Label × PREORDER	0.486***	0.532***	0.478***	0.500***
	(0.097)	(0.093)	(0.101)	(0.111)
Round	-0.054***	-0.037**	-0.066***	-0.067***
	(0.016)	(0.019)	(0.011)	(0.014)
Round × SEQSERVE	0.037*	0.022	0.032**	0.037*
	(0.021)	(0.025)	(0.015)	(0.019)
Round × PREORDER	0.099***	0.081***	0.066***	0.057**
	(0.025)	(0.029)	(0.023)	(0.028)
$DNRC_{t-1}$		-0.037		-0.146***
		(0.065)		(0.028)
$DNRC_{t-1}$ × SEQSERVE		-0.010		0.023
		(0.068)		(0.047)
$DNRC_{t-1}$ × PREORDER		-0.019		-0.021
		(0.089)		(0.086)
SEQSERVE	-0.052	0.125	0.696	0.738
	(0.334)	(0.459)	(0.442)	(0.571)
PREORDER	-2.547***	-2.513***	-1.974***	-1.926***
	(0.661)	(0.779)	(0.422)	(0.630)
Constant	-0.337**	-0.418	0.009	0.235
	(0.212)	(0.359)	(0.293)	(0.380)
LL	-760.8	-700.1	-1,438.6	-1,346.0
N (obs, individuals)	(1,539, 144)	(1,436, 144)	(2,880, 144)	(2,736, 144)

Economy-clustered robust standard errors in parentheses.

***, **, *: $p < 0.01, p < 0.05, p < 0.10$.

not significantly different from zero ($F(1, 17) = 0.00, p = 0.949$), indicating a flat time trend in investment in PREORDER. The coefficient on SEQSERVE × Round is also positive and highly significant; however, Round + SEQSERVE × Round is negative and significantly different from zero ($F(1, 17) = 12.15, p = 0.003$), indicating a negative time trend in compliance in SEQSERVE.

Table A2 reports Random Effects Probit regressions of $y \in \{\text{Compliance}, \text{Investment}\}$ on investor labels, a linear time trend (Round), and, in a second specification (regressions 2 and 4), a variable counting how many entrepreneurs did not honor the contract in the previous round ($DNRC_{t-1}$), as well as dummy interactions of all the above with treatment dummies

for SEQSERVE and PREORDER.

We start by looking at the Compliance regressions. In regression (1), the coefficient on A-Label is positive and significant, although small in magnitude, indicating that compliance levels in NONE increased in investor label. The interaction with SEQSERVE is negative and significant, and (A-Label + A-Label × SEQSERVE) is not significantly different from zero ($\chi^2(1) = 0.02, p = 0.882$), indicating no relationship between label and compliance in SEQSERVE. In contrast, (A-Label × PREORDER) is large, positive and highly significant, indicating a positive relationship between label and compliance in PREORDER. We also find a negative and significant time trend in NONE; the interaction of Round with SEQSERVE is positive and marginally significant; (Round + Round × SEQSERVE) is not significantly different from zero ($\chi^2(1) = 1.57, p = 0.211$); the interaction of Round with PREORDER is positive and significant; (Round + Round × PREORDER) is significantly different from zero ($\chi^2(1) = 5.38, p = 0.020$) indicating a positive time trend in compliance. The same results hold in regression (2) which includes $DNRC_{t-1}$, while the coefficients on that variable and its treatment interactions are not significant.

We now turn to the Investment regressions. The coefficient on A-Label is not significantly different from zero; likewise for A-Label × SEQSERVE, indicating no relationship between investor labels and investment in NONE or SEQSERVE. In contrast, the coefficient on the interaction between A-Label and PREORDER is positive and highly significant, indicating a positive relationship between investment and labels. The coefficient on Round is negative and significant, indicating a downward trend in investment in NONE; the coefficient on (Round × SEQSERVE) is positive and significant, although (Round + Round × SEQSERVE) is negative and significantly different from zero ($\chi^2(1) = 10.85, p = 0.001$) indicating a downward trend in investment over time in SEQSERVE. The coefficient on (Round × PREORDER) is also positive and significant; furthermore (Round + Round × PREORDER) is equal to zero and not statistically significant ($\chi^2(1) = 0.00, p = 0.970$) indicating stable investment levels over time in PREORDER. These results hold in the specification that includes $DNRC_{t-1}$. The coefficient on $DNRC_{t-1}$ is negative and significant, indicating that low compliance in the previous period is negatively correlated with investment; this statistical relationship is consistent across the three treatments, as per the non-significant interactions of $DNRC_{t-1}$ with the two treatment dummies.

Table A3 reports Random Effects Probit regressions of $y \in \{$Compliance, Investment$\}$. The first specification—regressions (1) and (3)—includes as regressors a set of dummy variables for different investor labels (label A1 is the omitted category), a linear time trend (Round), as well as a variable counting how many entrepreneurs did not honor the contract in the previous round ($DNRC_{t-1}$), as well as dummy interactions of all the above with treatment

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Table A3: Estimated Determinants of Investment and Compliance Conditional on Labels, Alternative Specification

DV:	Investment				Compliance			
	(1)		(2)		(3)		(4)	
A8 × SeqServe	-1.328**	(0.675)			-0.936**	(0.454)		
A7 × SeqServe	-0.944	(0.977)			-0.505	(0.450)		
A6 × SeqServe	-1.178*	(0.609)			-0.465	(0.309)		
A5 × SeqServe	0.005	(0.832)			-0.876**	(0.309)		
A4 × SeqServe	-0.307	(0.798)			-0.867***	(0.244)		
A3 × SeqServe	-0.732	(0.781)			-0.700*	(0.382)		
A2 × SeqServe	-0.926	(0.626)			-0.425	(0.336)		
SeqServe	0.865*	(0.517)	0.180	(0.491)	0.436	(0.507)	-0.117	(0.303)
Round × SeqServe	0.037*	(0.019)	0.037*	(0.019)	0.023	(0.024)		
$DNRC_{t-1}$ × SeqServe	0.022	(0.047)	0.023	(0.047)	-0.005	(0.075)		
A8 × PreOrder	0†	(-)	0†	(-)	8.781***	(0.573)	9.149***	(0.423)
A7 × PreOrder	2.071	(1.297)	3.115***	(0.940)	8.591***	(0.526)	8.824***	(0.357)
A6 × PreOrder	2.151**	(1.012)	2.786***	(0.872)	8.135***	(0.431)	8.193***	(0.392)
A5 × PreOrder	2.440***	(0.668)	3.062***	(0.461)	6.959***	(0.429)	7.636***	(0.385)
A4 × PreOrder	1.731**	(0.764)	1.992***	(0.421)	6.226***	(0.400)	6.682***	(0.349)
A3 × PreOrder	0.573	(0.868)	1.207***	(0.420)	5.967***	(0.498)	6.233***	(0.447)
A2 × PreOrder	0.368	(0.552)	0.859**	(0.410)	6.420***	(0.598)	6.586***	(0.531)
PreOrder	-1.343**	(0.560)	-1.908***	(0.518)	-7.137***	(0.597)	-6.712***	(0.420)
Round × PreOrder	0.057**	(0.028)	0.056**	(0.028)	0.086***	(0.030)		
$DNRC_{t-1}$ × PreOrder	-0.023	(0.087)	-0.024	(0.087)	-0.028	(0.106)		
A8	0.914	(0.586)			0.697*	(0.361)		
A7	1.018	(0.915)			0.514	(0.395)		
A6	0.612	(0.567)			0.291	(0.230)		
A5	0.602	(0.488)			0.856***	(0.186)		
A4	0.252	(0.640)			0.593***	(0.180)		
A3	0.630	(0.767)			0.496***	(0.177)		
A2	0.490	(0.378)			0.224	(0.278)		
Round	-0.066***	(0.014)	-0.066***	(0.014)	-0.039**	(0.018)		
$DNRC_{t-1}$	-0.145***	(0.029)	-0.145***	(0.028)	-0.047	(0.071)		
Constant	0.165	(0.425)	0.727**	(0.363)	-0.588	(0.396)	-0.455*	(0.247)
LL	-1,339.5		-1,478.2		-690.8		-771.9	
N (obs, subjects)	(2,622, 138)		(2,622, 138)		(1,539, 144)		1,436, 144	

Bootstrapped standard errors in parentheses. ***, **, *: $p < 0.01, p < 0.05, p < 0.10$.

†: A8 was dropped from estimation as $L8 = 1$ predicted success perfectly; 114 obs not used.

dummies for SeqServe and PreOrder. We observe in both Investment and Compliance regressions a discernible pattern as we move down labels in PreOrder which is absent in None and SeqServe. The coefficients on the time trends corroborate the earlier findings: in the investment regression, we observe a negative and significant time trend in None.

The time trend in SEQSERVE is significantly different from the one in NONE but (Round + Round × SEQSERVE) is negative and significant ($\chi^2(1) = 10.61, p = 0.001$). The time trend in PREORDER is significantly different from the one in NONE but (Round + Round × PREORDER) is essentially zero and not significant ($\chi^2(1) = 0.00, p = 0.968$).

In the compliance regression, we observe a negative and significant time trend in NONE. The time trend in SEQSERVE is not significantly different from the one in NONE ($\chi^2(1) = 1.55, p = 0.213$). The time trend in PREORDER is significantly different from the one in NONE and (Round + Round × PREORDER) is positive and significant ($\chi^2(1) = 5.71, p = 0.017$). In other words, we find a positive time trend in compliance in PREORDER, but a negative time trend in NONE and SEQSERVE. The coefficient on $DNRC_{t-1}$ is negative and significant in the investment regression, indicating a negative correlation between past compliance and current investment, while it is not significant in the compliance regression, corroborating the earlier analysis.

The second specification includes label dummies only for PREORDER, in order for us to test for whether investors with label Ax in that treatment are more or less likely to experience higher investment/compliance than the average investor in NONE or SEQSERVE. Because we are interested in behavior across all rounds of the experiment, we do not include time trends or $DNRC_{t-1}$ as regressors.

In the investment regression, we first note that because investment level was 100% for all players with label A8 we are unable to make inference using the random effects probit estimator. We find investment levels by investors with labels A7-A5 to be significantly higher than average investment in NONE (all comparisons $\chi^2(1) \geq 3.44, p \leq 0.064$). Investment levels by investors with labels A4-A2 were not significantly different than average investment in NONE (all comparisons $\chi^2(1) \leq 1.75, p \geq 0.185$). Investors with label A1 in PREORDER had significantly lower investment levels than the average in NONE ($\chi^2(1) = 27.87, p < 0.001$). Comparing PREORDER to SEQSERVE, we find that investors with labels A7, A6, and A5 had significantly higher investment than the average in SEQSERVE, while investment levels by investors with label A4 were not significantly different than average investment in SEQSERVE ($\chi^2(1) = 0.09, p = 0.765$); in all other cases, investment levels conditional on a label in PREORDER were significantly lower than average investment in SEQSERVE (all comparisons, $\chi^2(1) \geq 3.10, p \leq 0.078$).

In the compliance regression, we find compliance levels for investors with labels A8-A5 to be significantly higher than average compliance in NONE (all comparisons $\chi^2(1) \geq 18.35, p \leq 0.001$). Compliance levels for investors with labels A4-A2 were not significantly different from the average compliance in NONE (all comparisons

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$\chi^2(1) \leq 2.40, p \geq 0.121$). Investors with label A1 in PREORDER had significantly lower compliance levels than the average in NONE ($\chi^2(1) = 255.19, p < 0.001$). Comparing PREORDER to SEQSERVE, we find that investors with labels A8-A5 had significantly higher compliance levels than the average in SEQSERVE (all comparisons, $\chi^2(1) \geq 26.65, p < 0.001$), while compliance levels for investors with labels A4-A2 were not significantly different from the average investment in SEQSERVE (all comparisons, $\chi^2(1) \leq 0.1.96, p \geq 0.162$). Investors with label A1 in PREORDER had significantly lower compliance levels than the average in SEQSERVE ($\chi^2(1) = 222.62, p < 0.001$).

B Instructions

B.1 NONE

Instructions

Welcome to our experiment. Please do not talk to other participants in the room. If you have any questions, please raise your hand and we will take your question in private. These instructions explain how the experiment works. Please read them carefully. Your compensation will depend on your decisions, as well as the decisions of other people in the room.

The payments in the experiment are in Experimental Currency Units (ECU): 1 ECU is worth £2. There are 22 rounds, 2 for practice and 20 for compensation. After the last round, the computer will pick 3 rounds at random. We will convert your payments in those rounds into pounds and pay you in cash. You will also receive £3 for participating in this experiment.

There are two types of players: 8 Player As and 8 Player Bs. You will be informed about your type once the experiment starts and retain it for the entire experiment. Every Player A will be given a label: A1, A2, A3, A4, A5, A6, A7, and A8. Player As will be informed about their label once the experiment starts and retain it for the entire experiment.

At the start of each round, the computer will randomly match one Player A to one Player B.

Player B is told the label of the Player A he or she is paired with in that round.

Player A must then make a decision: *Stay* or *Enter*.

If Player A chooses *Stay*, then both Player A and Player B get 1 ECU and the round ends.

If Player A chooses *Enter*, then Player B must make a decision: *Send* or *Keep*.

If Player B chooses *Send*, then both Player A and Player B get 2 ECU.

If Player B chooses *Keep*, then Player A gets 0 ECU and Player B gets 4 ECU.

This is the end of the round. Your payments do not carry over to the next round.

1

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                    ┌─────────────────────┐
                    │  Begin Experiment   │
                    └─────────────────────┘
                               │
                               ▼
                    ┌─────────────────────┐
         ┌─────────▶│   Start New Round   │
         │          └─────────────────────┘
         │                     │
         │                     ▼
         │          ┌─────────────────────┐    Stay    ┌─────────────────────┐
         │          │  Player A Chooses   │───────────▶│  Player A gets 1 ECU │
         │          └─────────────────────┘            │  Player B gets 1 ECU │
         │                     │ Enter                  └─────────────────────┘
         │   Send   ┌─────────────────────┐   Keep
         │  ┌───────│  Player B Chooses   │───────┐
         │  ▼       └─────────────────────┘       ▼
         │ ┌───────────────────┐      ┌───────────────────┐
         │ │ Player A gets 2 ECU│      │ Player A gets 0 ECU│
         │ │ Player B gets 2 ECU│      │ Player B gets 4 ECU│
         │ └───────────────────┘      └───────────────────┘
         │       No       │                  │
         │  ┌─────────────────────┐
         └──│    Final Round?     │◀──────────────
            └─────────────────────┘
                     │ Yes
                     ▼
            ┌─────────────────────┐
            │   Compensation and  │
            │  End of Experiment  │
            └─────────────────────┘
```

Quiz

1. Suppose you are one of the players in a pair, in which Player A6 chose *Stay*, so Player B had no choice. Of the Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

What are the payments in ECU to the two players in your pair? Player A: ____; Player B: ____.

2. Suppose you are one of the players in a pair, in which Player A6 chose *Enter* and Player B chose *Send*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

What are the payments in ECU to the two players in your pair? Player A: ____; Player B: ____.

3. Suppose you are one of the players in a pair, in which Player A6 chose *Enter* and Player B chose *Keep*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

What are the payments in ECU to the two players in your pair? Player A: ____; Player B: ____.

4. Suppose you are one of the players in a pair, in which Player A3 chose *Enter* and Player B chose *Keep*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

What are the payments in ECU to the two players in your pair? Player A: ____; Player B: ____.

2

B.2 SEQSERVE

Instructions

Welcome to our experiment. Please do not talk to other participants in the room. If you have any questions, please raise your hand and we will take your question in private. These instructions explain how the experiment works. Please read them carefully. Your compensation will depend on your decisions, as well as the decisions of other people in the room.

The payments in the experiment are in Experimental Currency Units (ECU): 1 ECU is worth £2. There are 22 rounds, 2 for practice and 20 for compensation. After the last round, the computer will pick 3 rounds at random. We will convert your payments in those rounds into pounds and pay you in cash. You will also receive £3 for participating in this experiment.

There are two types of players: 8 Player As and 8 Player Bs. You will be informed about your type once the experiment starts and retain it for the entire experiment. Every Player A will be given a label: A1, A2, A3, A4, A5, A6, A7, and A8. Player As will be informed about their label once the experiment starts and retain it for the entire experiment.

At the start of each round, the computer will randomly match one Player A to one Player B.

Player B is told the label of the Player A he or she is paired with in that round.

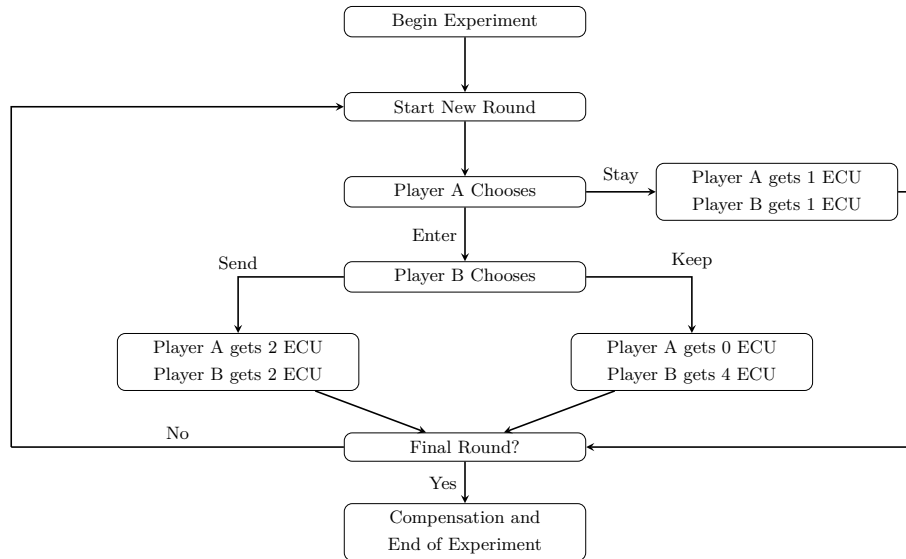Player A must then make a decision: *Stay* or *Enter*.

If Player A chooses *Stay*, then both Player A and Player B get 1 ECU and the round ends.

If Player A chooses *Enter*, then Player B must make a decision: *Send* or *Keep*.

If Player B chooses *Send*, then both Player A and Player B get 2 ECU and the round ends.

If Player B chooses *Keep*, then that pair is put on a list of all pairs whose Player B chose *Keep*.

The computer will select 1 pair from the list at random. Each pair on the list has an equal chance of being selected by the computer.

For example, if there are 2 pairs on the list, then each of those 2 pairs is selected with probability $\frac{1}{2}$.

For all pairs *Not selected*, Player A gets 0 ECU and Player B gets 4 ECU.

For the *Selected* pair, Player A gets 2 ECU and Player B gets 1.8 ECU.

This is the end of the round. Your payments do not carry over to the next round.

1

Begin Experiment → **Start New Round** → **Player A Chooses**

- Stay → Player A gets 1 ECU / Player B gets 1 ECU
- Enter → **Player B Chooses**
  - Send → Player A gets 2 ECU / Player B gets 2 ECU
  - Keep → **Does the random draw pick your pair?**
    - Yes → Player A gets 2 ECU / Player B gets 1.8 ECU
    - No → Player A gets 0 ECU / Player B gets 4 ECU

→ **Final Round?**
- No → Start New Round
- Yes → **Compensation and End of Experiment**

Quiz

1. Suppose you are one of the players in a pair, in which Player A6 chose *Stay*, so Player B had no choice. Of the Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

1. Will your pair be put on the list? Yes or No? _____.
2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.
3. What are the payments in ECU to the two players in your pair? Player A: _____; Player B: _____.

2. Suppose you are one of the players in a pair, in which Player A6 chose *Enter* and Player B chose *Send*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

1. Will your pair be put on the list? Yes or No? _____.
2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.
3. What are the payments in ECU to the two players in your pair? Player A: _____; Player B: _____.

3. Suppose you are one of the players in a pair, in which Player A6 chose *Enter* and Player B chose *Keep*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

1. Will your pair be put on the list? Yes or No? _____.
2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.
3. If selected, what are the payments in ECU to the players? Player A: _____; Player B: _____.

4. Suppose you are one of the players in a pair, in which Player A3 chose *Enter* and Player B chose *Keep*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

1. Will your pair be put on the list? Yes or No? _____.
2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.
3. If not selected, what are the payments in ECU to the players? Player A: _____; Player B: _____.

2

B.3 PreOrder

Instructions

Welcome to our experiment. Please do not talk to other participants in the room. If you have any questions, please raise your hand and we will take your question in private. These instructions explain how the experiment works. Please read them carefully. Your compensation will depend on your decisions, as well as the decisions of other people in the room.

The payments in the experiment are in Experimental Currency Units (ECU): 1 ECU is worth £2. There are 22 rounds, 2 for practice and 20 for compensation. After the last round, the computer will pick 3 rounds at random. We will convert your payments in those rounds into pounds and pay you in cash. You will also receive £3 for participating in this experiment.

There are two types of players: 8 Player As and 8 Player Bs. You will be informed about your type once the experiment starts and retain it for the entire experiment. Every Player A will be given a label: A1, A2, A3, A4, A5, A6, A7, and A8. Player As will be informed about their label once the experiment starts and retain it for the entire experiment.

At the start of each round, the computer will randomly match one Player A to one Player B.

Player B is told the label of the Player A he or she is paired with in that round.

Player A must then make a decision: *Stay* or *Enter*.

If Player A chooses *Stay*, then both Player A and Player B get 1 ECU and the round ends.

If Player A chooses *Enter*, then Player B must make a decision: *Send* or *Keep*.

If Player B chooses *Send*, then both Player A and Player B get 2 ECU and the round ends.

If Player B chooses *Keep*, then that pair is put on a list of all pairs whose Player B chose *Keep*.

The computer will select 1 pair from the list. It will sort all pairs on the list based on the label assigned to Player A and select the pair that has the Player A with the highest label.

For example, if there are two pairs on the list and the Player As in these pairs have labels A3 and A6, then the computer selects the pair whose Player A has the label A6.

For all pairs *Not selected*, Player A gets 0 ECU and Player B gets 4 ECU.

For the *Selected* pair, Player A gets 2 ECU and Player B gets 1.8 ECU.

This is the end of the round. Your payments do not carry over to the next round.

1

Begin Experiment

Start New Round

Player A Chooses — Stay → Player A gets 1 ECU / Player B gets 1 ECU

Enter

Player B Chooses — Keep → Does your pair have the highest label?

Send

Yes → Player A gets 2 ECU / Player B gets 1.8 ECU

No → Player A gets 0 ECU / Player B gets 4 ECU

Player A gets 2 ECU / Player B gets 2 ECU

Final Round?

No

Yes

Compensation and End of Experiment

Quiz

1. Suppose you are one of the players in a pair, in which Player A6 chose *Stay*, so Player B had no choice. Of the Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

  1. Will your pair be put on the list? Yes or No? _____.

  2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.

  3. What are the payments in ECU to the two players in your pair? Player A: _____; Player B: _____.

2. Suppose you are one of the players in a pair, in which Player A6 chose *Enter* and Player B chose *Send*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

  1. Will your pair be put on the list? Yes or No? _____.

  2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.

  3. What are the payments in ECU to the two players in your pair? Player A: _____; Player B: _____.

3. Suppose you are one of the players in a pair, in which Player A6 chose *Enter* and Player B chose *Keep*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

  1. Will your pair be put on the list? Yes or No? _____.

  2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.

  3. What are the payments in ECU to the two players in your pair? Player A: _____; Player B: _____.

4. Suppose you are one of the players in a pair, in which Player A3 chose *Enter* and Player B chose *Keep*. Of the other Player Bs who had a choice, those paired with Players A1, A4, and A5 chose *Keep*.

  1. Will your pair be put on the list? Yes or No? _____.

  2. If you answered "Yes" to the first question, what is the chance your pair is selected? _____.

  3. What are the payments in ECU to the two players in your pair? Player A: _____; Player B: _____.

2

C   Screenshots

Round 1 of 20

Your label is **A1**.

Please make your decision and press OK.

Your decision ○ Stay
               ○ Enter

OK

Figure C1: Player A decision. This screen is the same in all treatments.

Round 1 of 20

In this round you are paired with the Player A whose label is **A4**.

Player A has chosen **Enter**.

Please make your decision and press OK.

Your decision   ○ Keep
          ○ Send

OK

Figure C2: Player B decision, if one. This screen is the same in all treatments.

Round 1 of 20

In this round you are paired with the Player A whose label is **A1**.

Player A has chosen **Stay**.

You do not have a decision to make.
Please press Continue.

Continue

Figure C3: Player B decision, if none. This screen is the same in all treatments.

Continue

Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Detailed Information about Your Pair

Player A's label in your pair was   **A8**

The Player A in your pair chose   **Stay**

The Player B in your pair .   **did not have a choice to make**

Player A's payoff from this round is   **1.00 ECU**

Player B's payoff from this round is   **1.00 ECU**

Please press Continue.

Figure C4: Feedback screen when Player A chose *Stay* in NONE.

Round 1 of 20

Summary Information for All Pairs

Number of Player B's that chose **Keep**   2

Number of pairs selected   **1**

Detailed Information about Your Pair

Player A's label in your pair was   **A1**

The Player A in your pair chose   **Stay**

The Player B in your pair.   **did not have a choice to make**

Player A's payoff from this round is   **1.00 ECU**

Player B's payoff from this round is   **1.00 ECU**

Please press Continue.

Continue

Figure C5: Feedback screen when Player A chose *Stay* in SeqServe and PreOrder.

Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Detailed Information about Your Pair

Player A's label in your pair was   **A7**
The Player A in your pair chose   **Enter**
The Player B in your pair chose.   **Send**
Player A's payoff from this round is   **2.00 ECU**
Player B's payoff from this round is   **2.00 ECU**

Please press Continue.

Continue

Figure C6: Feedback screen when Player B chose *Send* in NONE.

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Figure C7: Feedback screen when Player B chose *Send* in SeqServe and PreOrder.

Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Detailed Information about Your Pair

Player A's label in your pair was   **A1**

The Player A in your pair chose   **Enter**

The Player B in your pair chose.   **Keep**

Player A's payoff from this round is   **0.00 ECU**

Player B's payoff from this round is   **4.00 ECU**

Please press Continue.

Continue

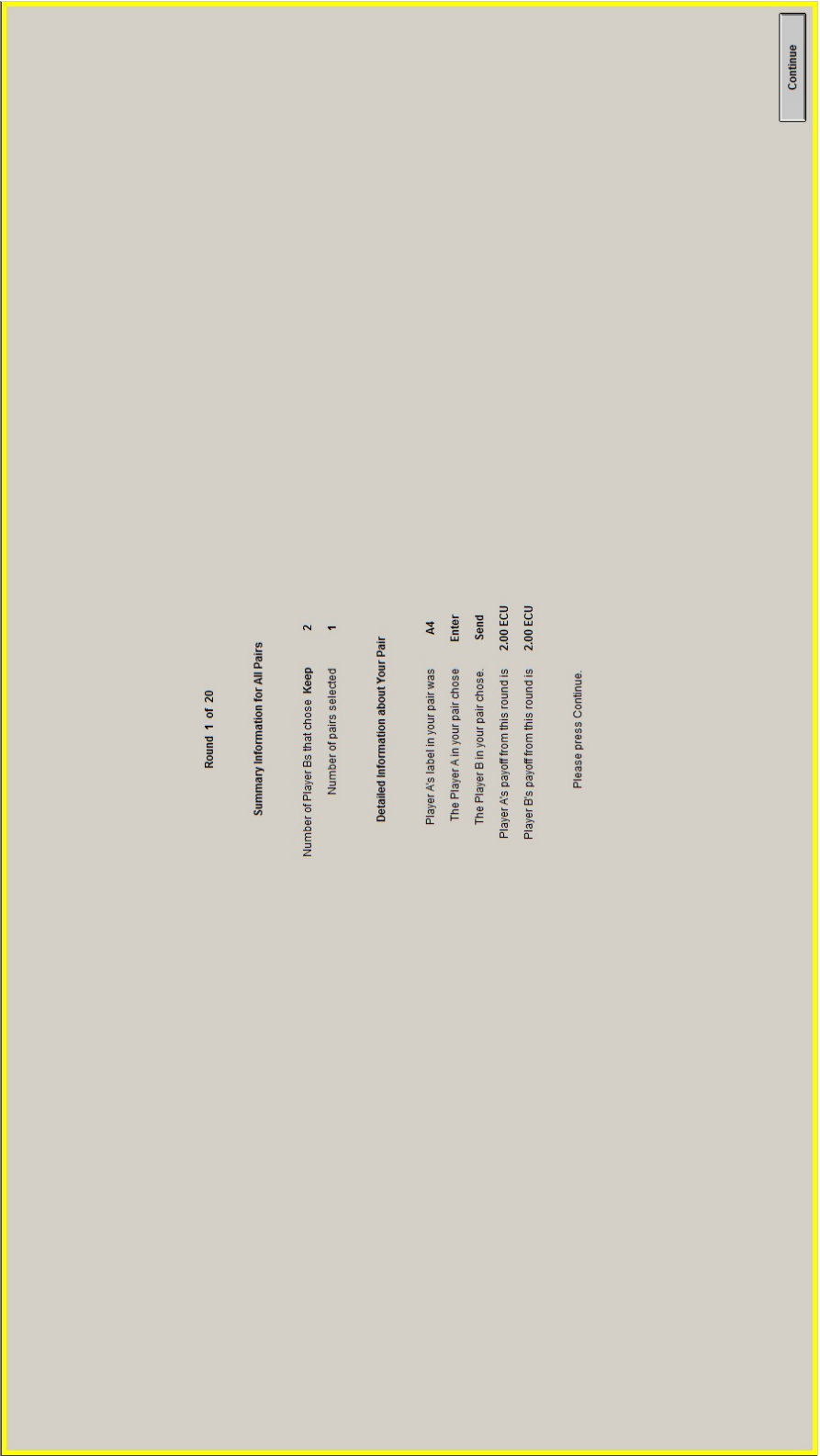**Figure C8:** Feedback screen when Player B chose *Keep* in NONE.

Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Number of pairs selected   1

Detailed Information about Your Pair

Player A's label in your pair was   **A3**

The Player A in your pair chose   **Enter**

The Player B in your pair chose.   **Keep**

The computer selected **one pair** at random from amongst those whose Player B chose Keep.

Your pair was not selected.

Player A's payoff from this round is   **0.00 ECU**

Player B's payoff from this round is   **4.00 ECU**

Please press Continue.

Continue

Figure C9: Feedback screen when Player B was *Not selected* in SeqServe.

Figure C10: Feedback screen when Player B was *Not selected* in PreOrder.

Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Number of pairs selected   1

Detailed Information about Your Pair

Player A's label in your pair was   **A6**

The Player A in your pair chose   **Enter**

The Player B in your pair chose.   **Keep**

The Player A in your pair **did not have** the highest label among those whose Player B chose Keep.

Your pair was not selected.

Player A's payoff from this round is   **0.00 ECU**

Player B's payoff from this round is   **4.00 ECU**

Please press Continue.

Continue

Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Number of pairs selected   1

Detailed Information about Your Pair

Player A's label in your pair was   **A7**

The Player A in your pair chose   **Enter**

The Player B in your pair chose.   **Keep**

The computer selected **one pair** at random from amongst those whose Player B chose Keep.

Your pair was selected.

Player A's payoff from this round is   **2.00 ECU**

Player B's payoff from this round is   **1.80 ECU**

Please press Continue.
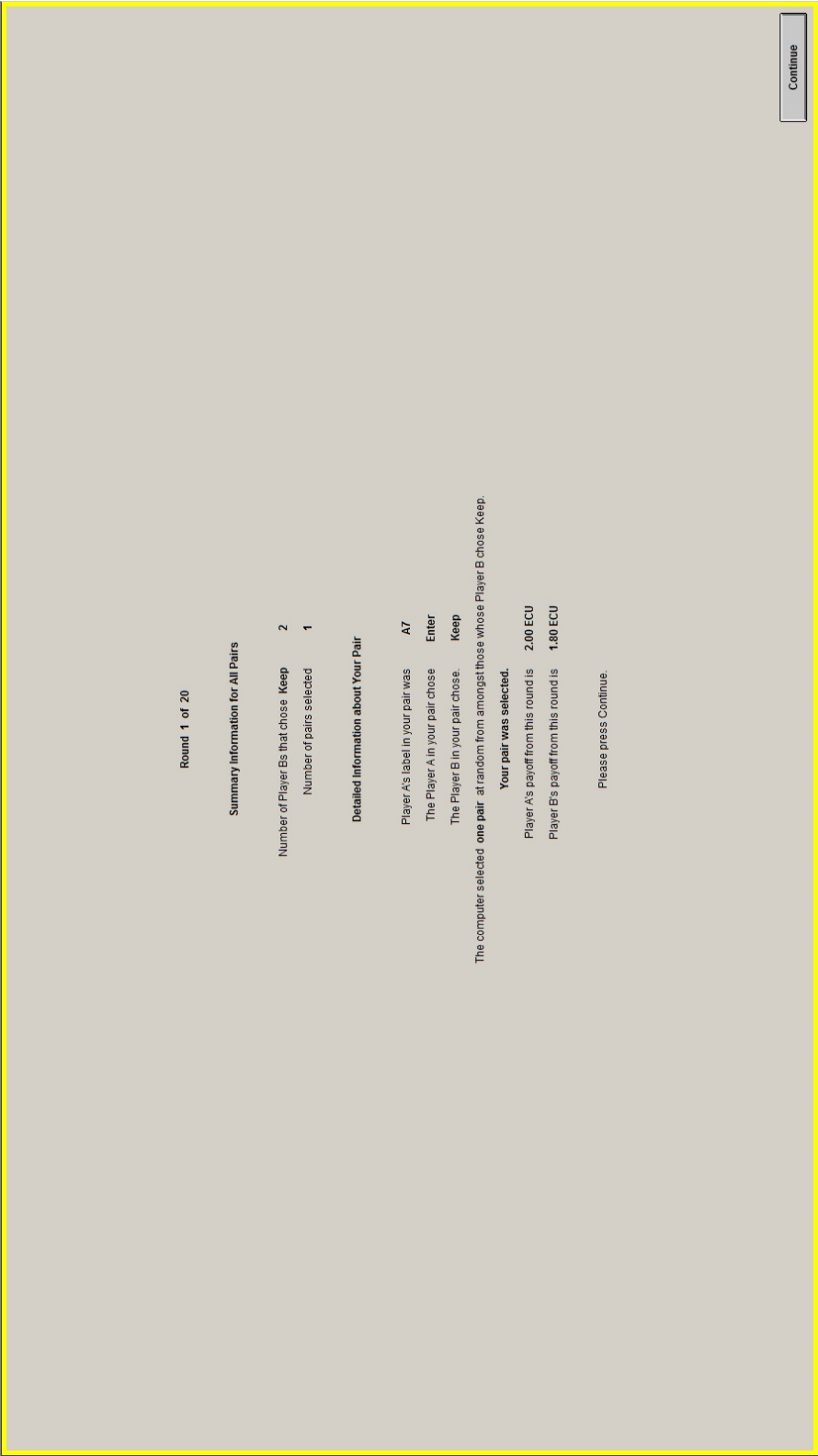
Continue

Figure C11: Feedback screen when Player B was *Selected* in SEQSERVE.

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Round 1 of 20

Summary Information for All Pairs

Number of Player Bs that chose **Keep**   2

Number of pairs selected   **1**

Detailed Information about Your Pair

Player A's label in your pair was   **A8**

The Player A in your pair chose   **Enter**

The Player B in your pair chose.   **Keep**

The Player A in your pair has **the highest label** among those whose Player B chose Keep.

Your pair was selected.

Player A's payoff from this round is   **2.00 ECU**

Player B's payoff from this round is   **1.80 ECU**

Please press Continue.

Continue

Figure C12: Feedback screen when Player B was *Selected* in PREORDER.

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